LIBRARY
ANNEX
2



ANNEX
LIBRARY
B
006641

CORNELL
UNIVERSITY
LIBRARY



FROM

Dat

Date Due

CORNELL UNIVERSITY LIBRARY

3 1924 078 273 848

PROCEEDINGS

OF THE

ROYAL SOCIETY OF MEDICINE

EDITED BY

JOHN NACHBAR, M.A., M.D.

UNDER THE DIRECTION OF

THE EDITORIAL COMMITTEE

VOLUME THE FIFTH

SESSION 1911-12

PART I

GENERAL REPORTS

SECTION OF ANÆSTHETICS BALNEOLOGICAL & CLIMATOLOGICAL SECTION

SECTION FOR THE STUDY OF DISEASE IN CHILDREN

CLINICAL SECTION DERMATOLOGICAL SECTION

ELECTRO-THERAPEUTICAL SECTION



LONDON

LONGMANS, GREEN & CO., PATERNOSTER ROW

1912

A7.6.37-40

LONDON:
JOHN BALE, SONS AND DANIELSSON, LTD.,
OXFORD HOUSE,
GREAT TITCHFIELD STREET, OXFORD STREET, W.

PROCEEDINGS

OF THE

ROYAL SOCIETY OF MEDICINE

VOLUME THE FIFTH

COMPRISING THE REPORT OF THE PROCEEDINGS FOR THE SESSION 1911-12

GENERAL REPORTS



LONDON
LONGMANS, GREEN & CO., PATERNOSTER ROW
1912

The Royal Society of Medicine.

OFFICERS FOR THE SESSION 1911-12.

President—

Sir HENRY MORRIS, Bt., F.R.C.S.

Vice-Presidents—

W. J. MCCARDIE, M.B. (Anæsthetics).
G. H. THOMPSON (Balneological and Climatological).
G. A. SUTHERLAND, M.D. (Study of Disease in Children).
Sir WILLIAM OSLER, Bt., M.D., F.R.S. (Clinical).
Sir MALCOLM MORRIS, K.C.V.O. (Dermatological).
ARCHIBALD D. REID (Electro-Therapeutical).
THEODORE THOMSON, C.M.G., M.D. (Epidemiological).
STCLAIR THOMSON, M.D. (Laryngological).
FREDERICK TAYLOR, M.D. (Medical).
F. W. MOTT, M.D., F.R.S. (Neurological).
AMAND ROUTH, M.D. (Obstetrical and Gynæcological).
H. LLOYD WILLIAMS (Odontological).
W. MILLIGAN, M.D. (Otological).
R. T. HEWLETT, M.D. (Pathological).
CLINTON T. DENT, M.C. (Surgical).
W. E. DIXON, M.D., F.R.S. (Therapeutical and Pharmacological).

Honorary Treasurers—

Sir WILLIAM SELBY CHURCH, Bt., K.C.B., M.D.
Sir FRANCIS H. CHAMPNEYS, Bt., M.D.

Honorary Librarians—

RICKMAN J. GODLEE, M.S. NORMAN MOORE, M.D.

Honorary Secretaries—

ARTHUR LATHAM, M.D. HERBERT S. PENDLEBURY, F.R.C.S.

Members of Council—

Sir WILLIAM ALLCHIN, M.D.
R. A. GIBBONS, M.D.
J. WARRINGTON HAWARD, F.R.C.S.
W. P. HERRINGHAM, M.D.
R. CLEMENT LUCAS, F.R.C.S.
D'ARCY POWER, F.R.C.S.
H. D. ROLLESTON, M.D.
CHARTERS J. SYMONDS, M.S.
E. F. WHITE, F.R.C.S.

Note.—The Vice-Presidents are the Presidents of the Sections the names of which are given in parentheses.

GENERAL REPORTS.

CONTENTS.

May 21, 1912.

PAGE

Opening of the New Building by His Majesty the King, accompanied by Her Majesty the Queen 1

The President's Conversazione 11

Description of the New Building 11

June 10, 1912.

A Discussion on Syphilis, with special reference to (*a*) its Prevalence and Intensity in the Past and at the Present Day; (*b*) its Relation to Public Health, including Congenital Syphilis; (*c*) the Treatment of the Disease :—

Introductory Remarks by the PRESIDENT (Sir Henry Morris, Bt., F.R.C.S.) 21

Opening Address, with special reference to the Prevalence and Intensity of the Disease in the Past and at the Present Day. By NORMAN MOORE, M.D. 29

Opening Address, with special reference to the Treatment of the Disease. By D'ARCY POWER, F.R.C.S. 38

Opening Address, with special reference to the Relation of the Disease to Public Health, including Congenital Syphilis. By F. W. MOTT, M.D., F.R.S. 50

June 17, 1912.

The Discussion continued :—

J. ERNEST LANE, F.R.C.S. 87

JONATHAN HUTCHINSON, F.R.C.S. 92

J. E. R. MCDONAGH, F.R.C.S. 97

P. MACLEOD YEARSLEY, F.R.C.S. 104

J. F. BRISCOE 106

G. PERNET, M.D. 109

C. F. MARSHALL, F.R.C.S. 111

June 24, 1912.

The Discussion continued :— PAGE

Major H. C. French, R.A.M.C. 115
Major T. W. Gibbard, R.A.M.C., M.B. 133
Sir George Savage, M.D. 138
J. Douglas White, M.D. 143
F. E. Fremantle, F.R.C.S. 148

July 8, 1912.

The Discussion continued :—

H. R. Dean, M.D. 151
John H. Dauber, F.R.C.S.I. 162
J. P. Candler, M.D. 169
J. W. Barrett, C.M.G., M.D. 174
J. M. Bernstein, M.B. 180
S. Arthur Powell, M.B. 186

Summary of the Debate by the President 189

The Council think it right to state that the Society does not hold itself in any way responsible for the statements made or the views put forward in the various papers.

John Bale, Sons & Danielsson, Ltd., 83-91, Great Titchfield Street, London, W.

The Royal Society of Medicine

Tuesday, May 21, 1912.

Opening of the New Building

BY

HIS MAJESTY THE KING,

ACCOMPANIED BY

HER MAJESTY THE QUEEN.

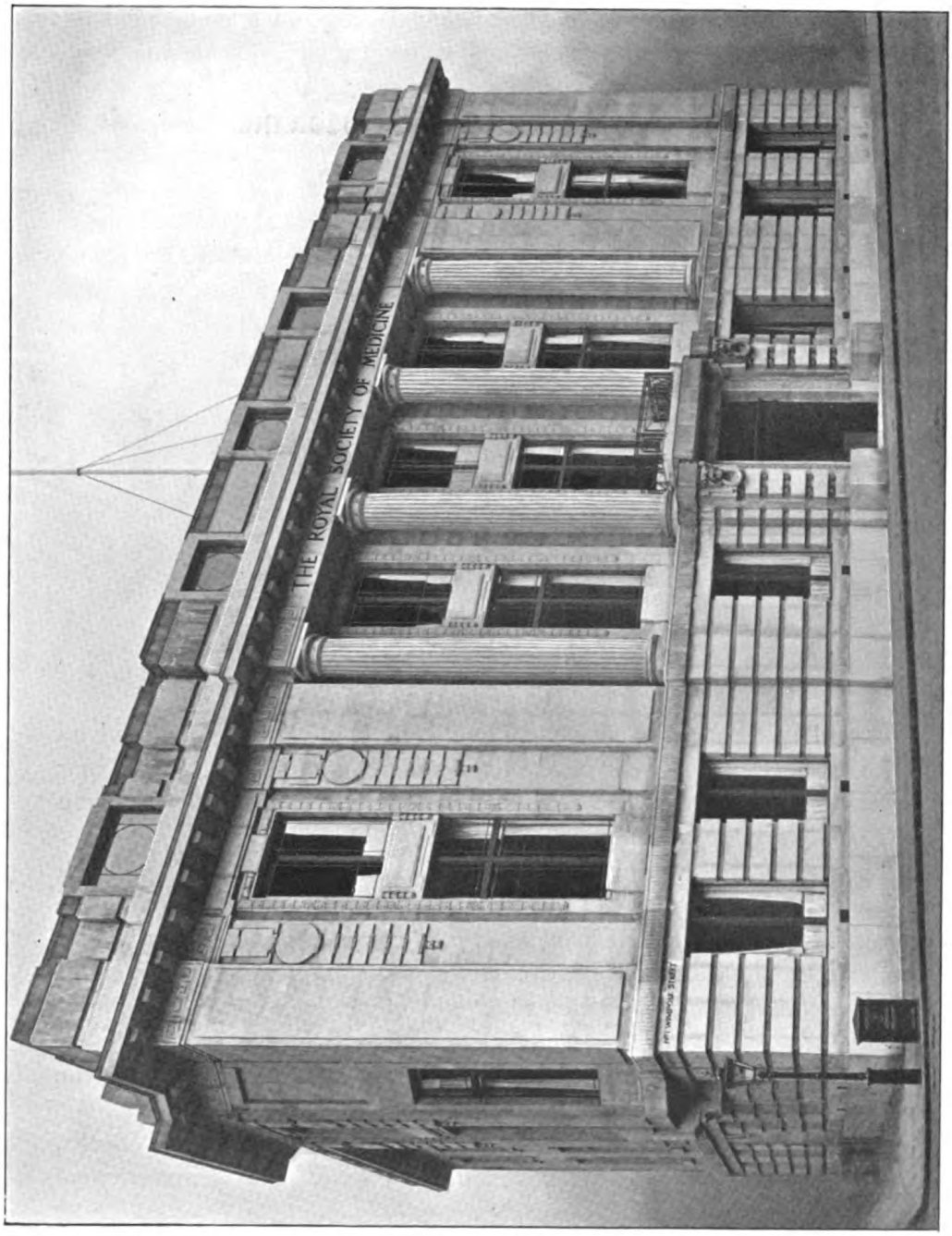
IT will be a source of gratification to all the Fellows and Members of the Society, and to the medical profession as a whole, that Their Majesties King George and Queen Mary were able to open in person the Society's new building in Wimpole Street.

Their Majesties' visit happened to fall a few days after the death of the King of Denmark, but happily for the complete success of the occasion it was not deemed necessary to curtail the ceremony in any way, though the fact that the Court was then in full mourning robbed the proceedings of much of the colour which otherwise might have been expected.

Their Majesties arrived at the Henrietta Street entrance of the building at 3.45 p.m.

A Guard of Honour (under the command of Major H. H. Tooth, C.M.G.) was furnished by the Medical Unit of the University of London Officers' Training Corps.

Their Majesties were received, on alighting, by the President, Sir Henry Morris Bt., the Hon. Secretaries, Dr. Arthur Latham and Mr. Herbert S. Pendlebury, and the Secretary, Mr. J. Y. W.



MAIN FAÇADE.

MacAlister, who was personally congratulated by His Majesty on the completion of the new building of the Royal Society of Medicine, to the formation of which he had devoted so much invaluable time and labour.

The Members of the Council and of the Building Committee waited on either side of the Entrance Hall. On entering the building a bouquet was presented to the Queen by Mrs. Arthur Latham. This bouquet was composed of flowers of medical herbs expressly selected from the Old Chelsea Physick Garden.[1]

Their Majesties then proceeded to the Robert Barnes Hall, and were conducted to the dais, followed by the Council and Building Committee, who took up their places on either side of the King and Queen. When Their Majesties had taken their seats, the following Address was read by the President:—

To the King's and the Queen's Most Excellent Majesties.

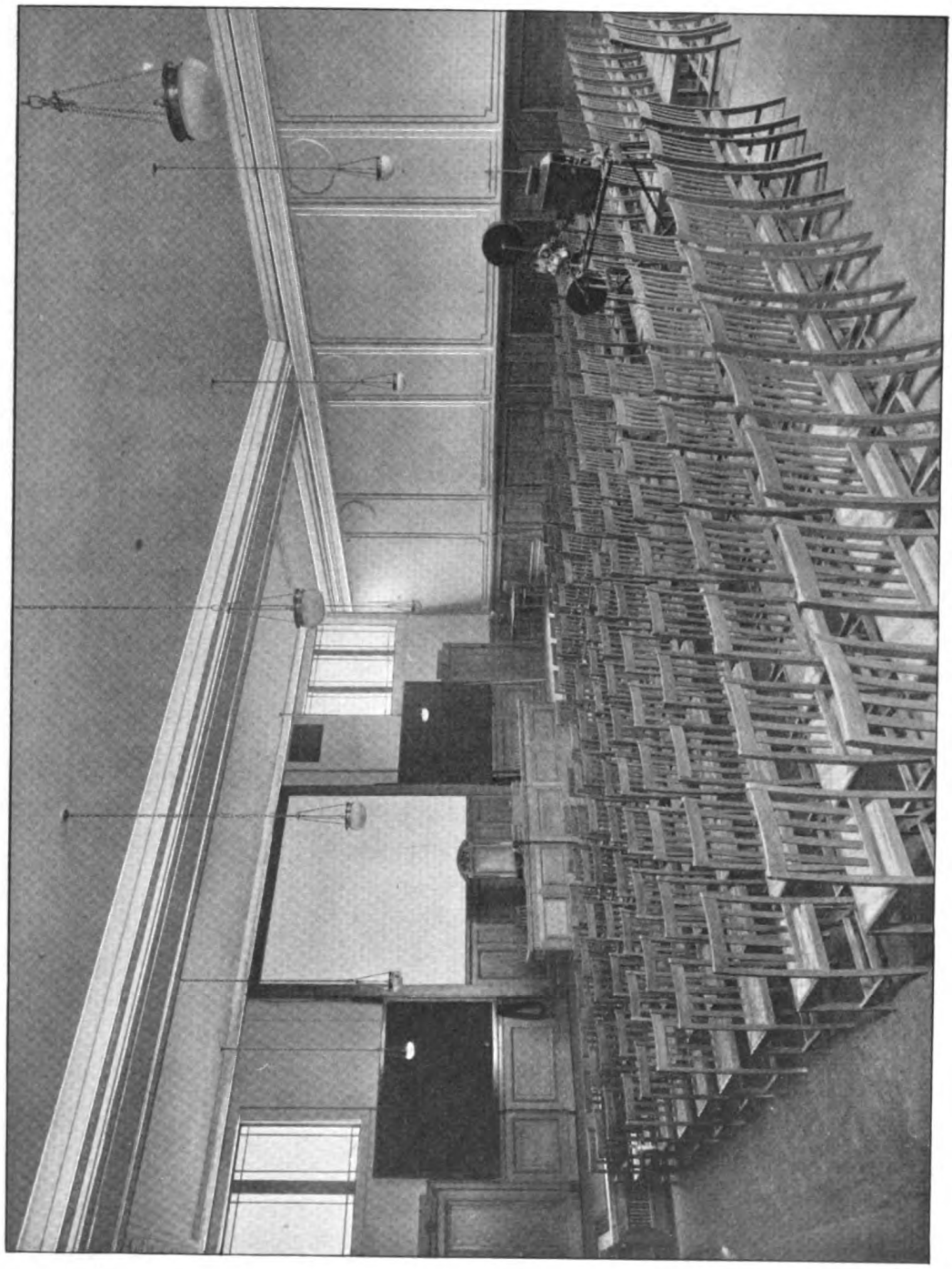
May it please Your Majesties,—

We, the President, Council, and Fellows of the Royal Society of Medicine, desire to convey to Your Majesties, with our loyal duty, our attachment and devotion to Your Persons and to the Throne.

We take this opportunity to express our grateful appreciation of the deep interest which Your Majesties invariably evince in all that concerns the well-being of Your Subjects, as well as in the progress of our profession, whose vocation it is not only to preserve the individual by curing disease, but to improve the health of the nation by promoting hygiene and preventing illness. Our Society was founded in the year 1805, but greater stability and effect were given to its designs by the Charter granted in 1834 by the special grace of His Majesty King William IV, who thereby declared himself, "and his successors if they shall think fit," the Patron of the Society.

It is our pride and privilege to record that each of the illustrious Successors of King William IV has so thought fit, and that

[1] The components of the bouquet were as follows: Bladder Senna (*Colutea arborescens*), Southernwood (*Artemisia abrotanum*), Absinth (*Artemisia absinthium*), Tarragon (*Artemisia dracunculus*), Lavender Cotton (*Santolina chamæcyparissus*), Male Fern (*Lastrea filix mas*), Green Hellebore (*Veratrum viride*), Alkanet (*Anchusa sempervirens*), Sun Spurge (*Euphorbia palustris*), Salad Burnet (*Poterium sanguisorba*), Lavender (*Lavandula vera*), Tansy (*Tanacetum vulgare*), Cabbage Rose (*Rosa centifolia*), Woad (*Isatis tinctoria*), Oak (*Quercus robur*).



ROBERT BARNES HALL.

our greatly beloved, and never to be forgotten late Sovereigns, Her Majesty Queen Victoria, and His Majesty King Edward VII have been, and now your own most excellent Majesty is our Patron.

By a Supplementary Charter graciously granted in the year 1907 by your Majesty's august Father, the Society was empowered to enrol as Fellows the members of several younger societies having allied objects; the scope of the Society was enlarged; its name was changed from "The Royal Medical and Chirurgical Society of London" to "The Royal Society of Medicine"; and it was specifically ordained that female as well as male persons might be elected Fellows and appointed to offices of the Society.

Thus the Royal Society of Medicine now forms a large corporation, embracing within its fold the most important medical societies in the country, and having for its object the improvement of the art and science of Medicine in all its branches by means of debate, collaboration, and research.

Your Majesties, who at all times show very active sympathy in the work of the great hospitals and similar institutions, and in everything that tends to alleviate sickness and relieve suffering, take also, as we are well aware, a keen and enlightened interest in every advancement of medical knowledge directed to the furtherance of these humane ends. Such progress is the single purpose of this Society.

To have been distinguished by the gracious favour of Your Majesties on this auspicious occasion has given inexpressible pleasure to all concerned and is a recognition that reflects the brightest lustre upon the history of the Society.

It is with these feelings of appreciation and gratitude that we very earnestly thank Your Majesties for the great honour you have conferred upon us, and upon the profession at large, by Your presence here to-day.

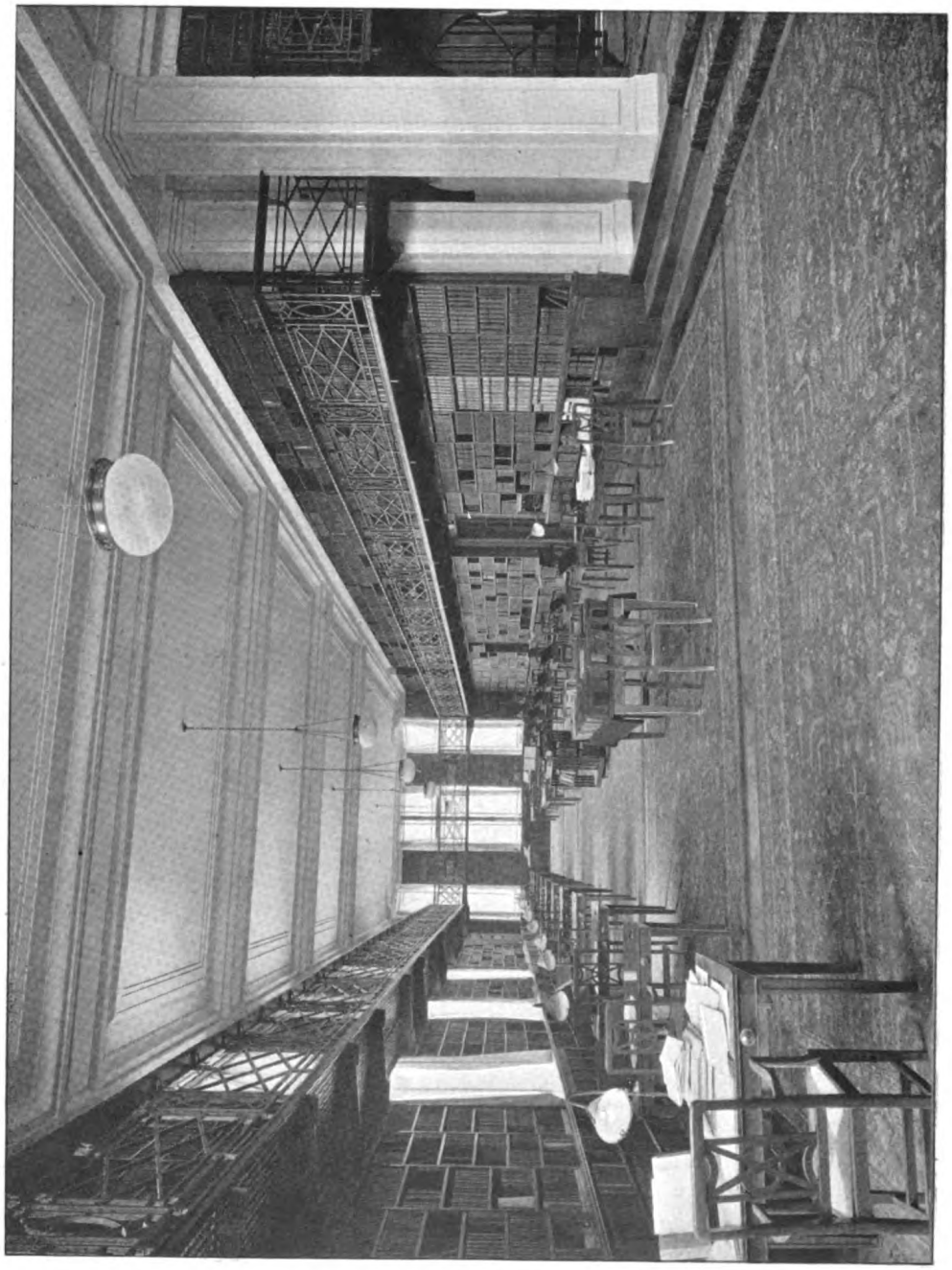
HENRY MORRIS, — **President.**

WILLIAM S. CHURCH,
FRANCIS H. CHAMPNEYS, — **Hon. Treasurers.**

R. J. GODLEE,
NORMAN MOORE, — **Hon. Librarians.**

ARTHUR LATHAM,
HERBERT S. PENDLEBURY, — **Hon. Secretaries.**

J. Y. W. MacALISTER, Secretary.



LIBRARY.

His Majesty's Reply.

His Majesty, in declaring the building open, was graciously pleased to say :—

I thank you on behalf of the Queen and myself for the loyal and dutiful address of the Royal Society of Medicine. It gives me great pleasure to open the fine building which will henceforth be the home of the Society, and which will provide adequately for the increase in your membership and for the extension of your duties since a new and enlarged Charter was granted to you by my father, King Edward.

The importance of the Society's work is now universally recognized, and it is a matter of satisfaction that the needs of the Society have been so generously provided for, and that its varied functions can now be carried on unhampered by lack of space. The health and well-being of the community are safeguarded by the energies of the medical profession. We look to you to fight sickness and disease, and we claim from you an untiring vigilance in this contest, and unceasing efforts to find, by the investigation of the laws of Nature, new means of combating these enemies. Medical science has revealed by experiment and trained observation new securities for life and health during recent years, and none can doubt that the improved public health is mainly due to the discoveries made by the medical profession in this and other countries, to the guidance given by that profession to the civil authorities, and to the sanitary precautions against the spread of disease which they have enforced. It gives us the greatest satisfaction to assist in any way the interests of your noble calling, and the Queen and I will ever watch the progress of your Society with sympathy and hearty goodwill.

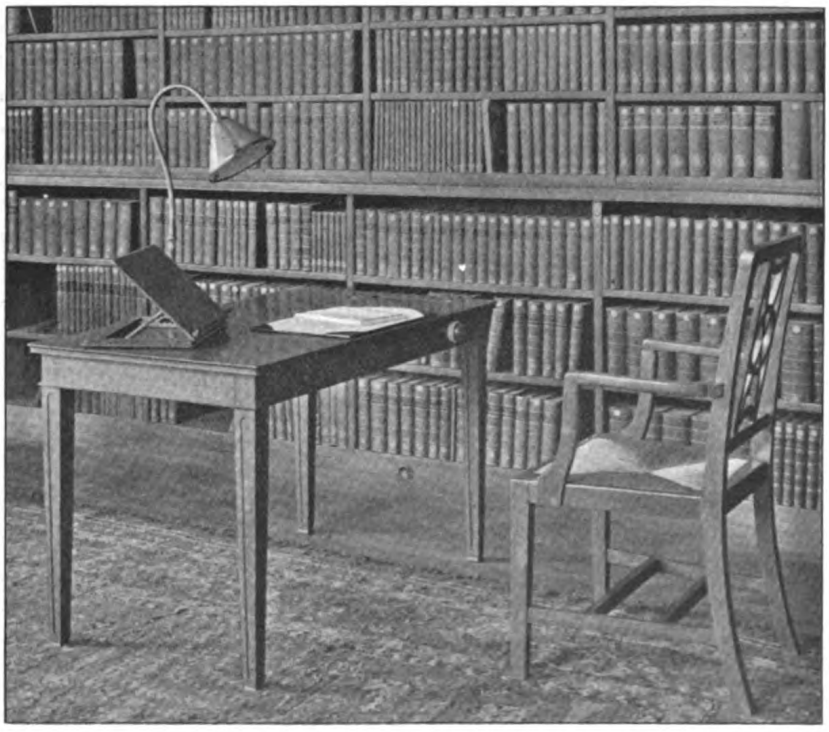
It only remains for me to declare this new building opened, which I do with the greatest satisfaction and pleasure.

The acoustics of the Large Meeting Room proved to be excellent, every word of the President's Address and of His Majesty's reply being clearly heard by everyone in the Hall.

Their Majesties were then conducted by the President and Secretaries from the dais down the centre of the Hall, and proceeded across the Entrance Hall through the small Meeting Room, passing the Patients' Rooms, to the lift. Accompanied by the President, they descended by the lift to the basement, where they were received by the



LIBRARY—PERIODICAL ROOM.



ONE OF THE TABLES IN THE LIBRARY.

Showing the novel Electric Light arrangement.

Hon. Librarians, Mr. Rickman J. Godlee and Dr. Norman Moore, and examined the book-store, which the King had expressed a wish to inspect. The Hon. Librarians explained to Their Majesties the method by which, with the use of iron book-cases and adjustable shelves, it was possible to store the largest number of books in the smallest possible space.

Their Majesties then re-entered the lift, and proceeded to the Library on the first floor, where the following presentations to Their Majesties were made by the President:—

Members of the Council.—Sir William S. Church, Sir Francis H. Champneys, Mr. J. Warrington Haward, Dr. Frederick Taylor, Mr. Clinton T. Dent, Dr. Amand Routh, Dr. R. T. Hewlett, Dr. Theodore Thomson, Mr. H. Lloyd Williams, Sir William Osler, Sir Malcolm A. Morris, Dr. George H. Thompson, Dr. F. W. Mott, Dr. StClair Thomson, Dr. W. J. McCardie, Mr. Richard Gill, Dr. William Milligan, Mr. Arthur H. Cheatle, Dr. G. A. Sutherland, Mr. A. D. Reid, Dr. W. Deane Butcher, Dr. W. E. Dixon, Dr. R. A. Gibbons, Dr. W. P. Herringham, Mr. R. Clement Lucas, Mr. D'Arcy Power, Dr. H. D. Rolleston, Mr. Charters J. Symonds, Mr. E. F. White.

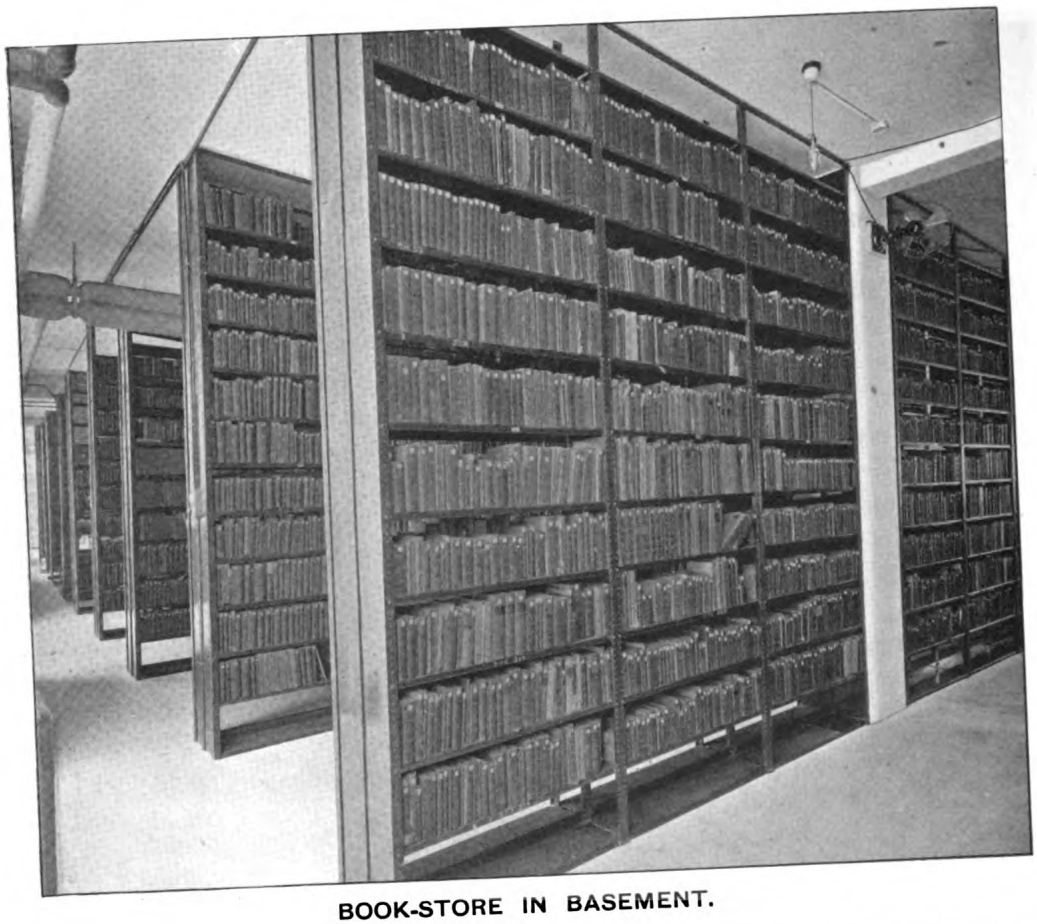
Members of Building Committee not on the Council.—Dr. Herbert R. Spencer, Dr. C. Theodore Williams, Dr. Leonard L. B. Williams.

Mr. John Belcher, the Architect; Mrs. Scharlieb, representing the Lady Fellows; Dr. John Nachbar, Editor of the Society's *Proceedings*.

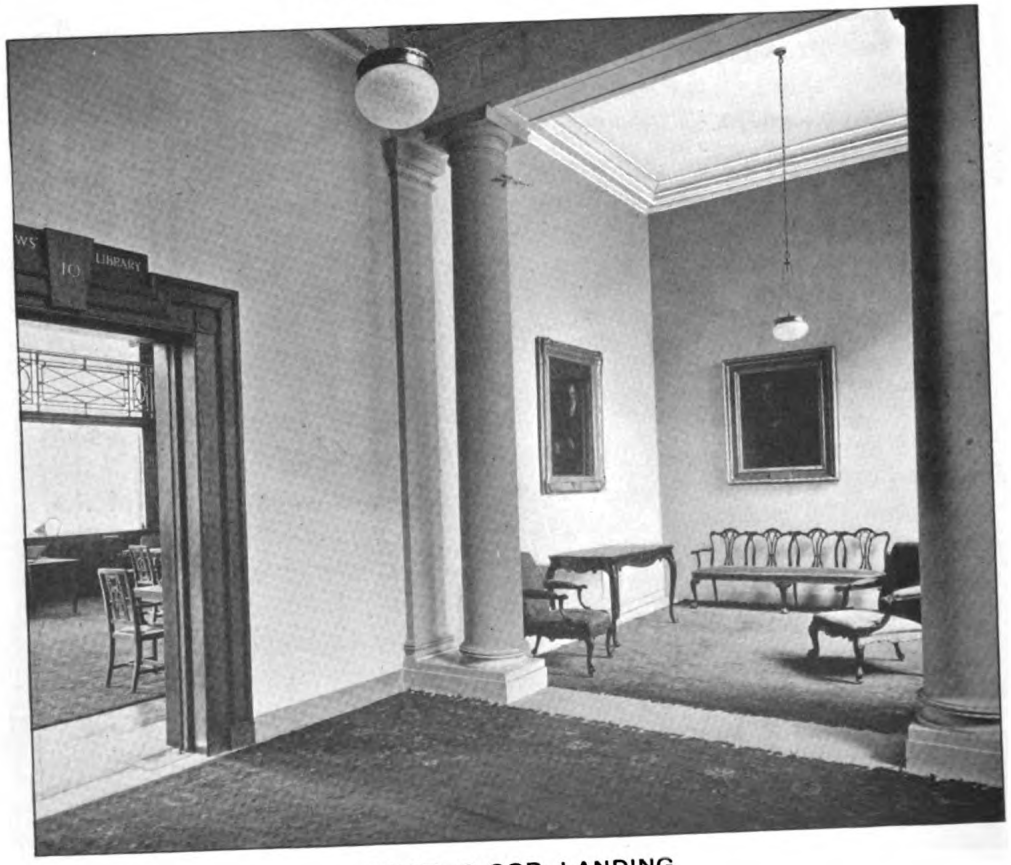
Their Majesties were then conducted round the Library, and were much interested in the electric lighting arrangement (for the reading tables) invented by Mr. MacAlister. The King graciously accepted a model of the apparatus. They also showed much interest in a mounted specimen of the Serpent of Æsculapius and its history. A specimen was on view which was captured in 1911 at Schlangenbad and which has been presented to the Society by Dr. Fortescue Fox.

Their Majesties then signed the Society's Roll on a specially illuminated page, and their attention was drawn to the signatures in other parts of the volume of King William IV, Queen Victoria, and King Edward VII. Before leaving the Library both the King and the Queen accepted from Sir Henry Morris specially bound copies of the history of the Society.

On the second floor, the King and Queen were shown the Council Room, three Committee Rooms, and the Henry Louis Florence Room. Their Majesties, attended by their suite, were then conducted by the President to the Fellows' Tea Room, where tea was served. Among those who had the honour of being present were: Sir William and Lady Church, Sir Francis Champneys, Dr. and Mrs. Norman Moore,



BOOK-STORE IN BASEMENT.



FIRST FLOOR LANDING.

Mr. Rickman J. Godlee, Dr. Arthur Latham, Mr. Herbert S. Pendlebury, Mr. J. Y. W. MacAlister, Sir William and Lady Osler, Mr. Pierpont Morgan, the Lord Mayor and Lady Mayoress, the Mayor and Mayoress of St. Marylebone (Mr. and Mrs. Ernest Debenham), Sir Richard Douglas Powell, Sir James and Lady Reid, Sir Havelock and Lady Charles, Sir Thomas and Lady Barlow, Sir Anderson and Lady Critchett. After tea, Their Majesties, accompanied by the President and the Hon. Secretaries, descended by the lift to the Entrance Hall, where they took leave of the Council.

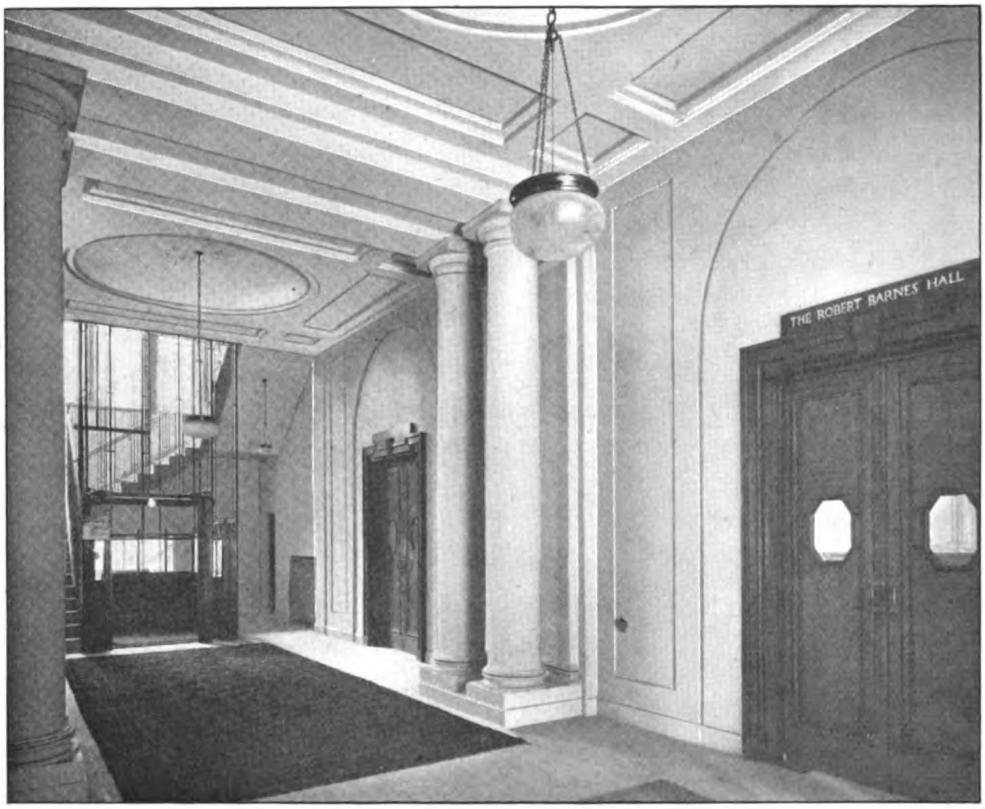
The King, before departing, inspected the Guard of Honour, and Their Majesties finally took leave of the President, Hon. Secretaries, and Secretary after a visit lasting an hour and a quarter, having expressed to all concerned their appreciation of the arrangements made for their visit, and of the admirable way in which the Society's new premises had been built and equipped.

The President's Conversazione.

In order to celebrate the occasion, the President most generously gave a Conversazione in the new house on the following evening, Wednesday, May 22, to which all Fellows and Members of the Society and their Friends were invited. As it was unfortunately impossible to invite all the Fellows to the Opening Ceremony, the occasion was welcomed as affording an opportunity to all to inspect the new building and see it lighted up, as soon as possible after its completion. Over 2,000 were present, and the evening was in every way a great success.

Description of the New Building.

The new building which has been erected at the corner of Henrietta Street and Wimpole Street, on a site occupying nearly 10,000 square feet, consists of a basement, ground floor, and three storeys. The façade is carried out in Portland stone, with a Cornish granite base. The *Architectural Review*, the leading architectural paper, in a notice of the building, states: "It is simple in its parts, but there is a straightforward vigour about it which holds the attention, and it possesses a dignity eminently in keeping with the institution housed within its walls." The special features of the interior are the two meeting rooms with their accommodation for patients, the handsome library with the ample storage room in the basement, and a number of rooms for the councils and committees of the Society and its Sections, which are now seventeen in number.

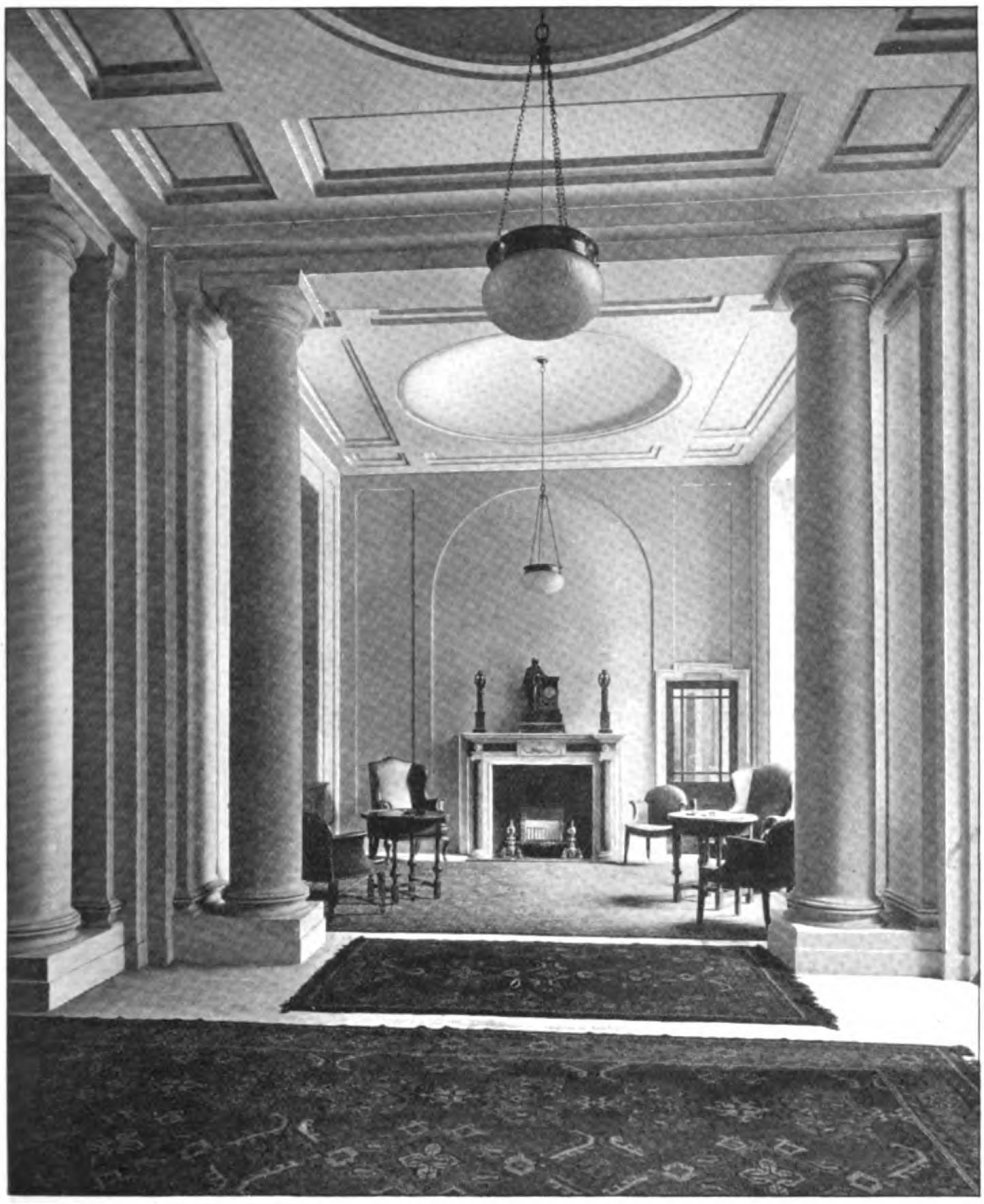


ENTRANCE HALL.

On the ground floor are the porter's lodge, entrance hall, and two meeting rooms; the larger, the Robert Barnes Hall, arranged to seat about 400, and the smaller about 150. Both meeting rooms are supplied with annexes for male and female patients, specially adapted for the examination of cases, and provided with lavatories.

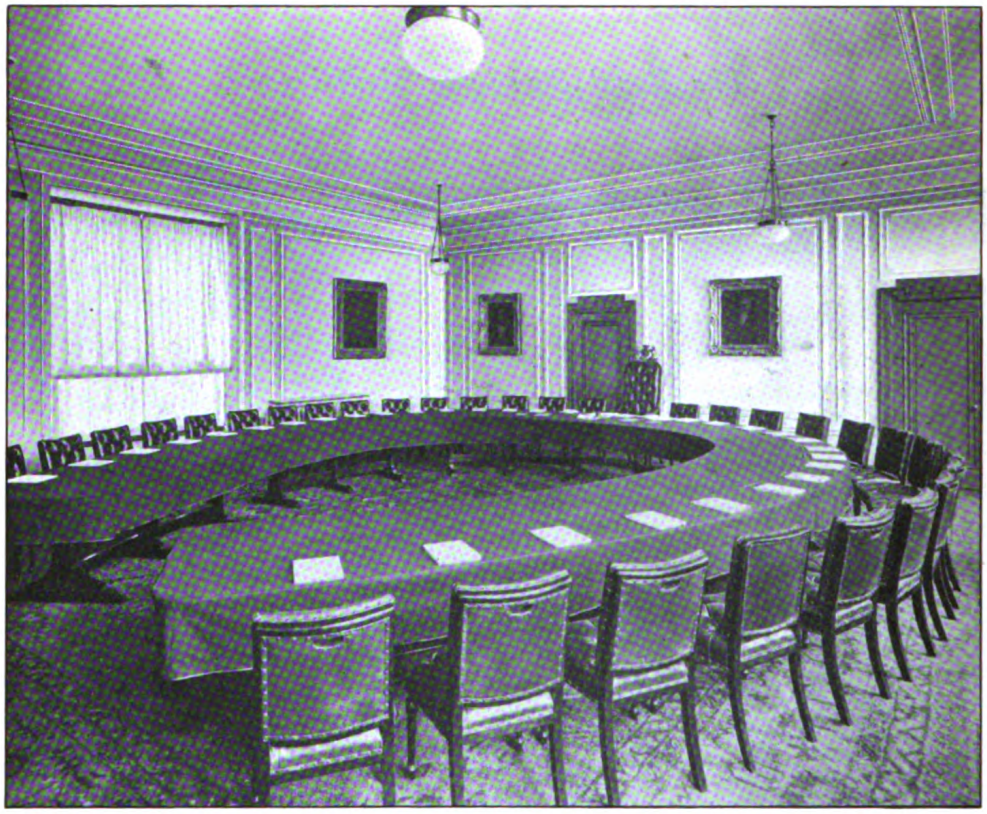
The Entrance Hall, which is L-shaped, contains an Eighteenth Century marble mantel-piece. On the mantel-piece are an Empire clock and a pair of Empire figures with candle holders, presented to the Society by the President.

The first floor is occupied by the Library, the main room of which is 110 ft. long, 28 ft. wide, and 19 ft. high, and extends the whole length of the building on the Henrietta Street side, with a large bay window overlooking Wimpole Street. The shelving extends from floor to ceiling, and is divided by a gallery at a height of 8 ft. From this gallery a Mezzanine Room, with additional tables for readers, opens off, and is well adapted for private study.

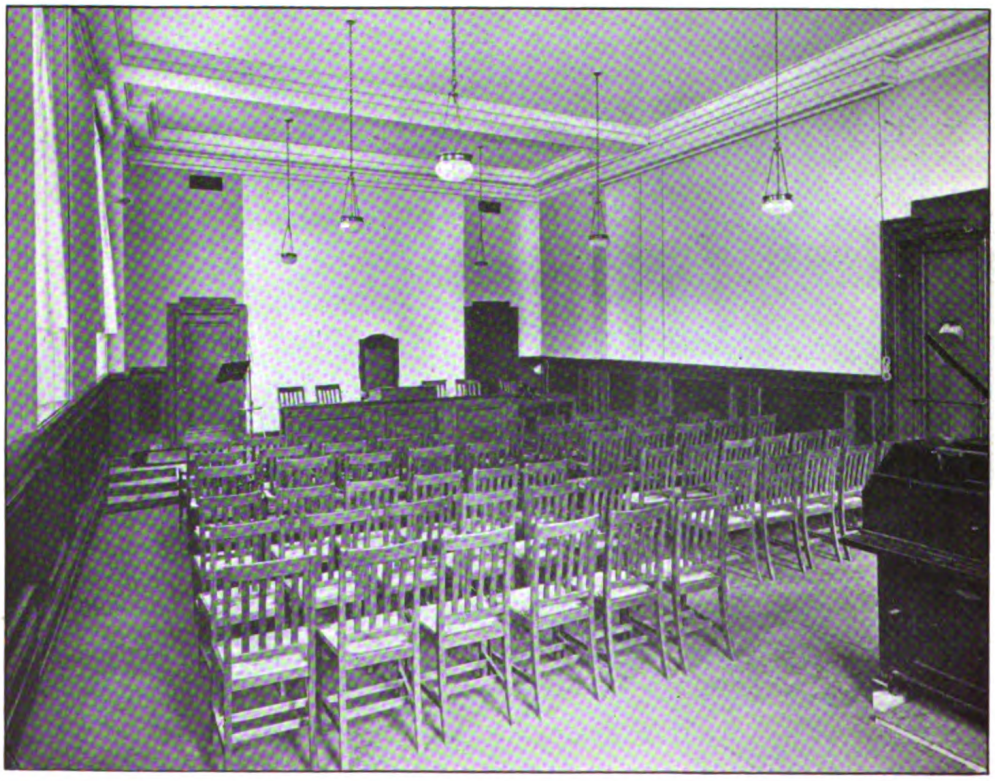


ENTRANCE HALL.

Showing the Eighteenth Century Mantel-piece, and the Empire Clock and Figures with Candle Holders, the gift of the President.



COUNCIL ROOM OF THE SOCIETY.



SMALL MEETING ROOM.

A special feature of the Library is the electric lighting arrangement of the reading tables. This is the invention of the Secretary, Mr. J. Y. W. MacAlister, and solves the problem of effectively getting rid of the usual flexible wires which are a great inconvenience in ordinary table-lamps. The contact is obtained through two legs of the table, in which strong springs are fitted. The spring fits into a counterpart spring socket in the floor, and the contact is produced by a plug in the table. When not in use, the table may be placed in any part of the room, as its own weight is sufficient to force the spring back into the leg of the table.

At the east end the Library extends the whole depth of the building, this portion forming the Periodical Room.

The remainder of the first floor is occupied by the Issue Room, two small rooms for private study, and the Fellows' lavatory. This is fitted up in the most up-to-date manner, with tiled walls and Leyland rubber flooring.

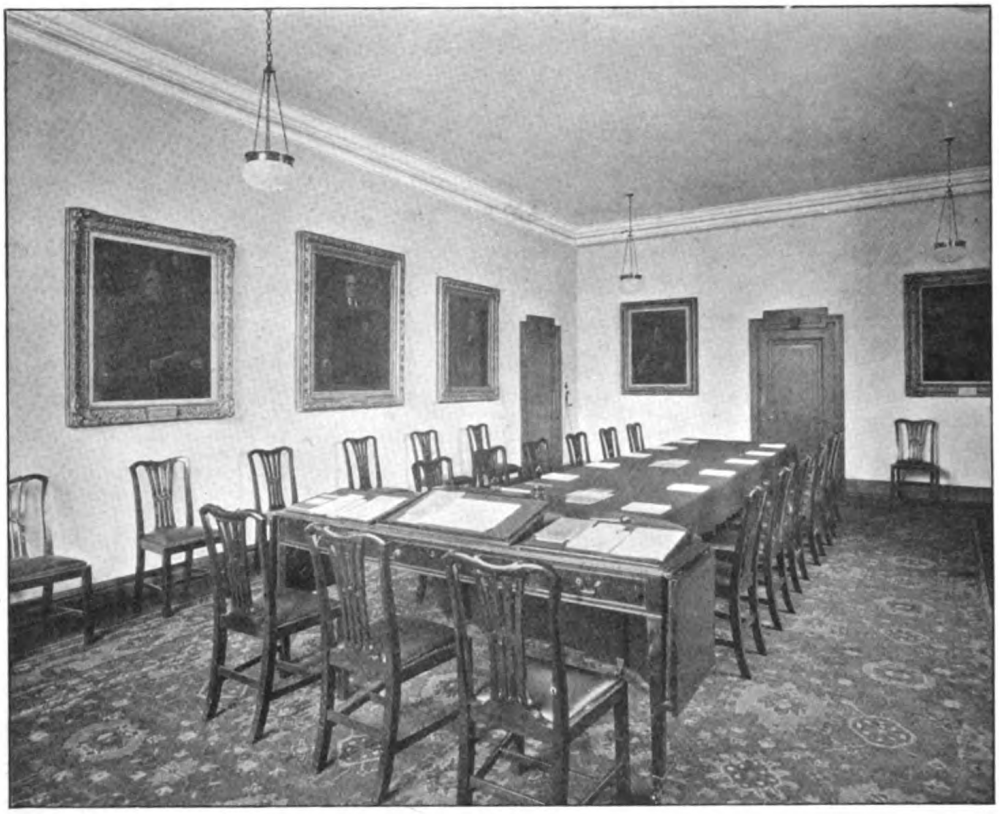
The book-store occupies the major part of the basement. The books are arranged on iron stacks with adjustable shelves, and the system adopted effects a great economy of space.

The Library now contains over 100,000 volumes, and there is storage room for over 200,000 volumes.

On this floor are also provided a large lavatory with dressing rooms for Fellows, also the porters' rooms, with heating chamber, coal-store, &c.

The second floor is occupied by the Council Room, various Committee Rooms, and the Fellows' Tea and Smoking Rooms. The Council Room is a fine room, 30 ft. by 28 ft., with a horseshoe table at which thirty-six members can sit. On the walls hang portraits of former Presidents of the Society, some of them being of considerable value, especially that of Alexander Marcet by Raeburn. Over the chimney-piece is a medallion by John Bacon, R.A., representing Æneas escaping from burning Troy carrying his father, the blind Anchises. For this medallion Bacon was awarded the Gold Medal of the Royal Academy in the year of its foundation, 1768. It was purchased by Sir William Chambers and placed in his house at 53, Berners Street, which afterwards became the home of the Royal Medical and Chirurgical Society. The medallion was removed in 1889 by Mr. MacAlister, and placed in the Society's house at 20, Hanover Square.

At the opposite corner of the building is the Council Room for the various Sections of the Society, and between the two are three Committee Rooms, including the Henry Louis Florence Room.



COUNCIL ROOM OF THE SECTIONS.



CLERKS' ROOM.

The Fellows' Tea and Smoking Rooms extend the whole depth of the building on the Wimpole Street side, and communicate by folding doors.

On the third floor are the Secretarial Offices, a third Meeting Room, Lady Fellows' Room, and the Marcus Beck Laboratory, specially fitted up for the examination of pathological specimens and for other scientific work.

The spacious staircase is a feature of the building and is constructed entirely of ferro-concrete cased with Hopton woodstone. It is exceedingly well lit and contains a large automatic electric lift capable of raising twelve persons at a time if necessary.

The whole of the interior woodwork is in Austrian wainscot oak and the window frames throughout are of steel.

The essential requirements of the Society were enunciated by the Council in June, 1909, in their "Report on the Question of Increased Accommodation," which was issued to all the Fellows of the Society, and considered at a Special General Meeting. In this report the Council stated, that in view of the growing needs of the Library, the great increase in the number of Fellows and Members, and the numerous meetings of Councils and Committees, they were of opinion that it was urgently necessary to provide without delay:—

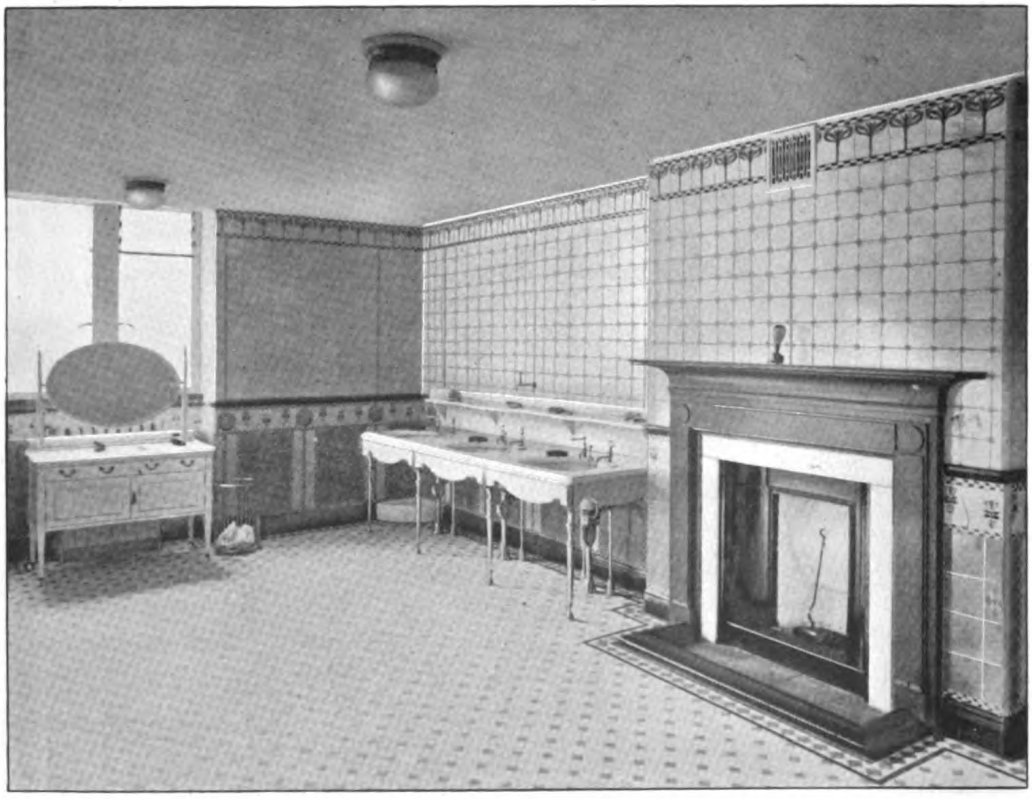
(*a*) Additional reading accommodation for the Library.
(*b*) Additional shelving for 20,000 volumes at once, and space for 50,000 more in the near future.
(*c*) Conference or conversation room.
(*d*) Ladies' lavatory and cloak-room.
(*e*) Room (adjoining meeting room) for examining male patients, with dressing room.
(*f*) Room (adjoining meeting room) for examining female patients, with dressing room.
(*g*) Lavatories for male and for female patients.
(*h*) Smaller meeting room (to seat, say, 100), with conveniences as (*e*) and (*f*).
(*i*) Larger Council room (to seat 50), free from shelving.
(*j*) Smaller Council room (to seat 30), free from shelving.
(*k*) Two Committee rooms, free from shelving.
(*l*) Cloak-room for men.

The Council reported that it was impossible to fulfil these requirements at the house in Hanover Square, and the Society then determined to build a new house on the Henrietta Street site.

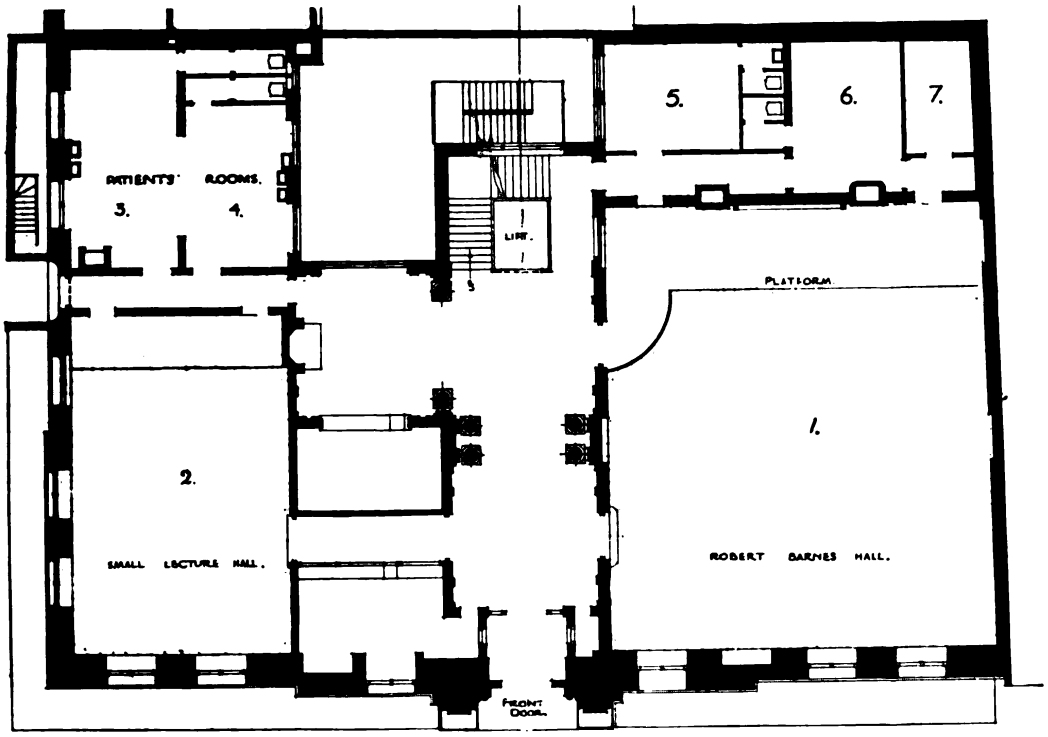
It will be seen that all the requirements have now been met, with additional Committee rooms due to the increase in the number of Sections.



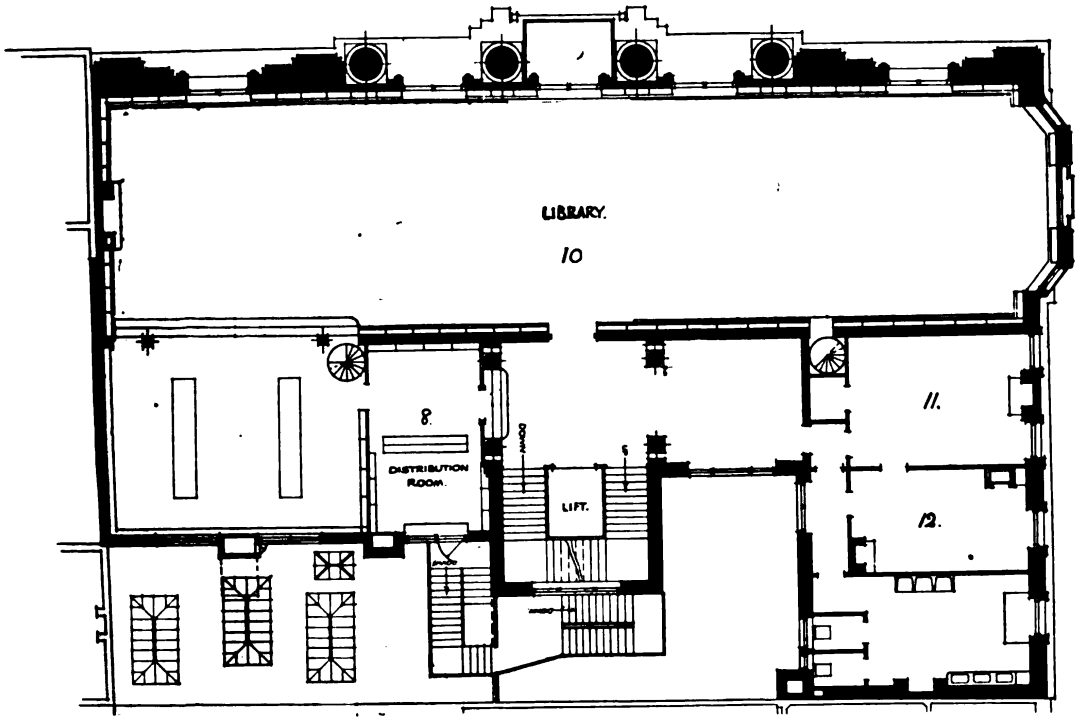
FELLOWS' TEA AND SMOKING ROOMS.



FELLOWS' LAVATORY.

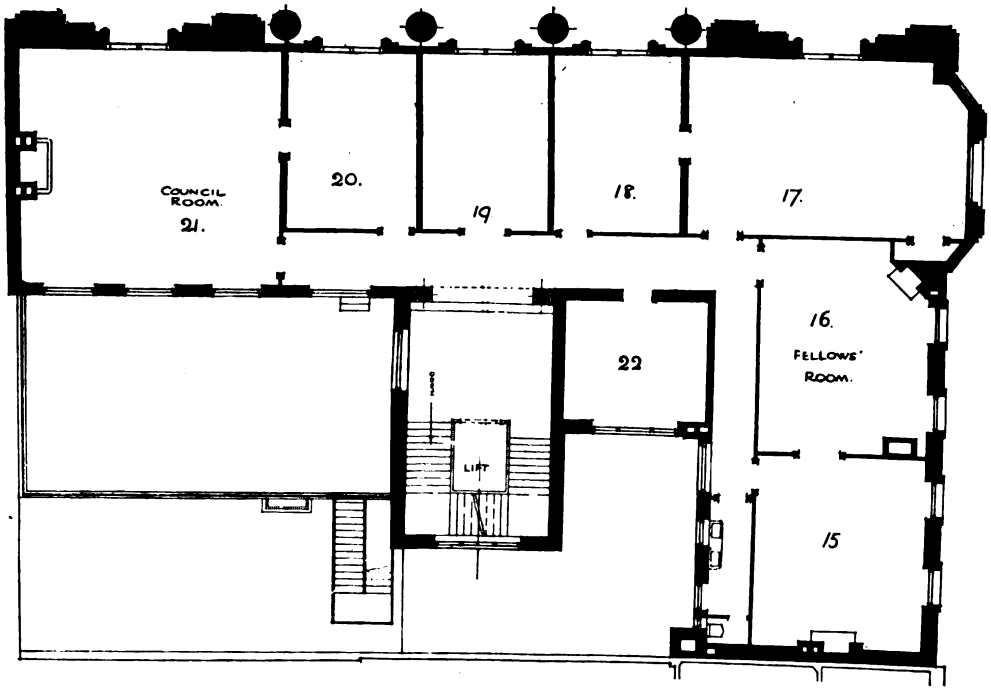


GROUND FLOOR PLAN.



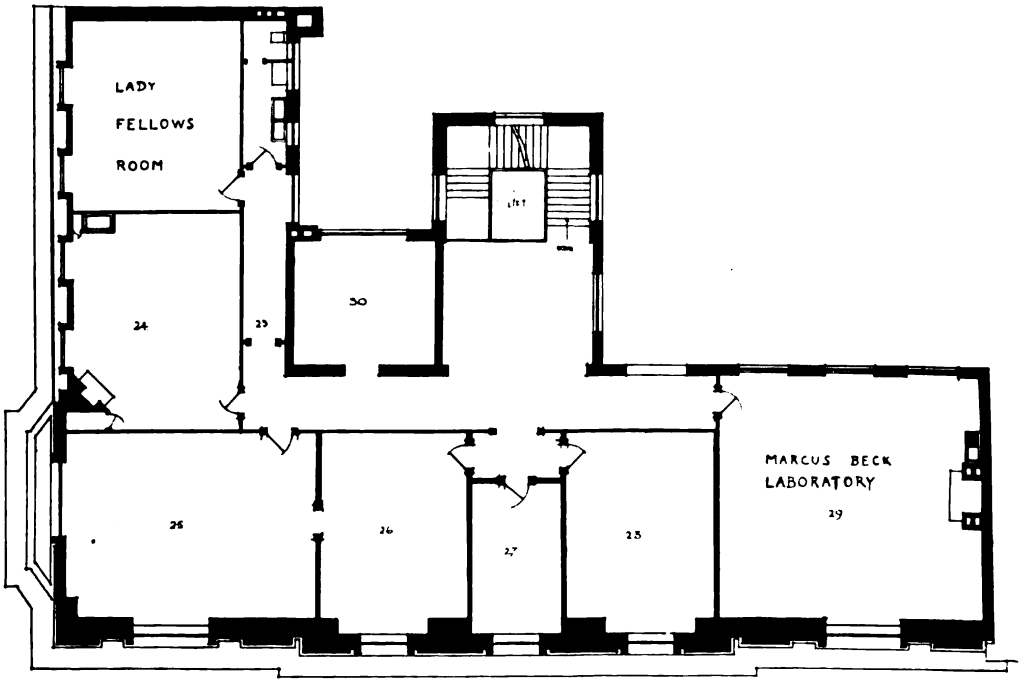
FIRST FLOOR PLAN.

11, Marshall Hall Room; 12, William Allchin Room—two rooms for private study.



SECOND FLOOR PLAN.

15, Fellows' Smoking Room; 16, Fellows' Tea Room; 17, Council Room of the Sections; 18, 19, 20, Committee Rooms; 19, Henry Louis Florence Room; 22, President's Room.



THIRD FLOOR PLAN.

24, Clerks' Room; 25, Spare Meeting Room; 26, Secretary's Room; 27, Cashier's Office; 28, Honorary Secretaries' Room; 30, Editor's Room.

The Royal Society of Medicine.

June 10, 1912.

Sir HENRY MORRIS, Bt., President, in the Chair.

A Discussion on Syphilis, with special reference to (*a*) its Prevalence and Intensity in the Past and at the Present Day; (*b*) its Relation to Public Health, including Congenital Syphilis; (*c*) the Treatment of the Disease.

INTRODUCTORY REMARKS BY THE PRESIDENT.

AT a meeting on November 6, 1911, of the Special Committee appointed by the Council to consider and report on the desirability of having during the session 1912 a general discussion, I ventured to suggest that the subject to be chosen should be the following:—

> "*The ultimate causes of the rise and decline of epidemic diseases; why such diseases are at one time quiescent, at another time vehement and universal. What determines the great cycles of plague, cholera, enteric fever, small-pox, scarlet fever, measles, diphtheria, and other infectious and contagious diseases?*"

It was unanimously agreed that this would form an excellent subject for discussion; but on consideration it was thought that the scope was too wide and would require too large a number of séances. Sir Francis Champneys thereupon suggested that the different diseases might be chosen *seriatim* in different years, and that in this way a series of volumes might be formed of great value. I might also mention that the Council

considered the possibility of holding in a succession of years a series of discussions, of a similar character to that now about to commence.

The subject of the present debate is Syphilis, the disease which that fine old surgeon Richard Wiseman, Serjeant-Chirurgeon to King Charles II, wrote upon under the name "Lues Venerea," and which he defined as "*a venemous contagious disease gotten either Immediately or Mediately from an impure coition.*" Wiseman adds: "*I say Immediately or Mediately, because it is very manifest, that not only the Persons so copulating are infected, but also the children derived from such Parents, and Nurses that suckle those children, and any other child that sucks upon those Nurses, and so forward.*" Then, Wiseman continues: "*It is frequent to mention other secundary ways of the propagation of it; as lying in the same Bed with an infected Person, as lying in the same sheets after them, or wearing their cloaths. . . . Drinking with one so diseased, or sitting on the close-stool after them, are likewise numbered amongst the Causes of Infection.*" And, with a touch of kindly humour, he concludes: "*These are all such convenient excuses for the more Shy and Coy Patients who will not otherwise be brought to confess their distempers, that it is Pity to discountenance them.*"

Syphilis having been selected as the subject of discussion, it was decided *first* that the debate should be arranged with special reference to (*a*) the prevalence and intensity of the disease in the past and at the present day; (*b*) its relation to public health, including congenital syphilis; (*c*) the treatment of the disease. *Secondly*, that at the first meeting separate speakers should open the debate under these three headings respectively, and that each of these speakers should be allowed from half to three-quarters of an hour for his speech or paper. *Thirdly*, that every subsequent speaker (except those who re-open the debate after each adjournment, each of whom will be allowed thirty minutes) be limited to fifteen minutes, and be permitted to treat of the subject at large, but that, as far as possible, he should group his remarks, separately, under the three different headings; and, *fourthly*, that an attempt should be made to arrange the speakers, so that the second day's discussion shall be devoted chiefly to the consideration of treatment, and the third day's discussion mainly concerned with points relating to the disease in its bearing on public health.

Dr. Norman Moore has undertaken to open the "Debate on the History of Syphilis," Dr. Mott on "Syphilis in Relation to Public Health," and Mr. D'Arcy Power on "The Treatment of Syphilis."

From Dr. Norman Moore, with his great bibliographic knowledge,

his recondite acquaintance with old books, pamphlets, and manuscripts, we may expect to hear much that will be interesting respecting the first appearance of syphilis. He will perhaps tell us what evidence, if any, there is that the disease prevailed among the Jews, Greeks, and Romans prior to the Christian Era, and if there is any good ground for the opinion that it was well known on the continent of Europe in the early centuries after the beginning of this era.

Is it certain that the same disease which we know as syphilis, with symptoms of constitutional infection, had existed in various parts of the ancient world?; and, if so, was there a long period of several centuries during which the disease was in complete abeyance, or but slightly, if at all, in evidence? Must we assume a continuity of the virus from very early times, or was there a great development or re-development at the end of the fifteenth century?

Is there anything to support the assertion that syphilis was first imported from the East into Europe on the return of the Crusaders in either the eleventh, twelfth, or thirteenth century?; or are we warranted in believing that it only came to Europe on the return of Columbus, after his first expedition to America and the West Indies, on March 15, 1493? Was the malady which was so destructive to the French army under Charles VIII, before Naples, really syphilis?; and if so, did the disease break out originally in Italy during the siege of Naples, the fall of which was brought about in 1495?; or, on the contrary, was syphilis prevalent in Paris in 1494 and in the few years before that date?

If syphilis existed in very ancient times, was Egypt specially exempt from it? If not, how is it that no trace of its ravages was found in any one of the 10,000 skeletons referred to by Professor Elliot Smith in his communication to the Pathological Society of Manchester, entitled "Disease in Ancient Egypt"?[1]

Another question arising in connexion with the antiquity of syphilis is whether the ancients confounded it with leprosy; whether, as Sebastianus Aquilanus has endeavoured to prove from Galen, Avicen, and Pliny, &c., the pox is only one species of the leprosy?; and whether, as Jacobus Cataneus, a writer almost as early as the rise of the name pox, tells us was possible, transitions from leprosy into pox and from pox into leprosy could and did occur?[2]

[1] *Lancet*, 1909, ii, pp. 1596-7.

[2] Becket, *Phil. Trans.*, 1720-21, xxxi, p. 55.

There is ample evidence that during the Tudor and the Stuart and Commonwealth Periods of English history syphilis was rampant in this country as well as in France and Italy. It is sufficient for proof to quote Shakespeare in regard to the first, and Wiseman in reference to the latter periods.

The word pox reminds us of the frequent use Shakespeare made of it. It occurs at least four or five and twenty times collectively, in fifteen of his plays. He used it as a curse, or an imprecation of impatience or evil. Thus, Iago says to Roderigo, who talked of drowning himself: "A pox on drowning thyself." Sir Andrew in "Twelfth Night," referring to a certain knight who was a celebrated fencer, says: "Pox on't, I'll not meddle with him." In "Measure for Measure," Barnardine in his prison exclaims:—

"A pox on your throats! Who makes that noise there?"

In "Love's Labour's Lost" we find even ladies of quality—ladies in attendance on the Princess of France—making similar exclamations, such as, "A pox of that jest!" In "All's Well that Ends Well" a French lord in a camp near Florence says of a soldier: "Let him fetch off his drum"; and he is anwered by another French lord:—

"A pox on't, let it go, 'tis but a drum."

In "Two Gentlemen of Verona" the servant of one of the gentlemen says to him of the other: "A pox of your love letters." In "Henry IV," in "Hamlet," in "Cymbeline," in "The Tempest," and other plays, there is similar employment of the word, which is equivalent to the "Damn," or "Damn it," of the present day.

This use of the word seems to prove conclusively that syphilis was very common in Shakespeare's day, and that the constitutional and local symptoms of the disease must have been quite familiar to the man in the street and to the ordinary person in society. It is quite obvious from the context of several of the passages in which the word occurs that it was the great-pox and not the small-pox which had given it currency, and to which allusion was made. For example, in "Pericles, Prince of Tyre" the virtuous Marina, the daughter of Pericles, who had been taken captive by pirates and sold to a brothel keeper, is cursed in the following manner by a Pander of the Bawd, for not yielding her honour on the solicitation of the customers: "Now the pox upon her green-sickness for me!"; and the Bawd replies to him: "Faith, there's no way to be rid on't but by way to the pox." In the scene in

the brothel, Lysimachus, the Governor of Mitylene, inquires, "Have you that a man may deal withal, and defy the surgeon"; and in her interview with Lysimachus, Marina speaks of herself as being within a

> "Loathsome stie
> Where, since I came, diseases have been sold
> Dearer than physic."

In "Henry IV," Falstaff says: "A man can no more separate age and covetousness than he can part young limbs and lechery; but the gout galls one, and the pox pinches the other." And lastly, in "Timon of Athens" there abound convincing proofs that Shakespeare knew thoroughly the ætiology, the symptoms of the later stages, and the then prevalent treatment of syphilis. This noble Athenian misanthrope, addressing Alcibiades, says:—

> "This fell whore of thine
> Hath in her more destruction than thy sword,
> For all her cherubin look."

Phrynia hearing this said of her, savagely replies to Timon, "Thy lips rot off"; and Timon retorts, "I will not kiss thee, then the rot returns to thine own lips again." Timon means, "I will not take the rot from thy lips by kissing them"; for, as Dr. Johnson has pointed out, "In former times there was a general impression that the venereal disease, transmitted to another, left the infector free." Further on in the same scene Timon says:—

> "Be a whore still! They love thee not that use thee;
> Give them diseases, leaving with thee their lust.
> Make use of the salt hours; season the slaves
> For tubs, and baths; bring down rose-cheeked youth
> To the tub-fast, and the diet."

The dramatist is here alluding to the practice of treating syphilis at that time either by guaiacum or mercurial inunction. With each method great care was taken to keep the patient warm and to sweat and diet him. Wiseman, about seventy years later than Shakespeare, tells us that in England a hot bath in an ordinary tub was used to make the patient sweat; abroad, a stove, an oven, a cave, or dungeon was resorted to during the guaiacum treatment. Hot baths, Wiseman says, promoted and hastened the effect of inunctions to cause salivation. During the whole period of treatment the strictest regulation in diet, amounting

almost to abstinence, was insisted upon. Hence we see the point and meaning of Shakespeare's reference "To the tub-fast, and the diet." Shakespeare was quite alive to the pains of periosteal nodes, to tendon gummata, to ozæna, to the loss of hair, to the voice changed by syphilitic laryngitis, and to the sallow, withered look of the skin of the face in late syphilis. He speaks of "a pox of wrinkles," and he makes Timon tell Phrynia and Timandra, in language which shows a considerable knowledge of the characters of secondary and tertiary syphilis:—

> "Consumption sow
> In hollow bones of man; strike their sharp shins,
> And mar men's spurring. Crack the lawyer's voice.
> . . . . . down with the nose;
> Down with it flat; take the bridge quite away
> Of him, that his particular to foresee
> Smells from the general weal; make curl'd-pate ruffians bald;
> And let the unscarred braggarts of the war
> Derive some pain from you. Plague all;
> That your activity may defeat and quell
> The source of all erection."

The only passage I know of in any of his plays from which it could be inferred that Shakespeare was referring to the small-pox is the one from "Love's Labour's Lost," which I have previously quoted. In this scene Rosalind says: "O that your face were not so full of O's!" (meaning, I suppose, the circular pits or depressions which are caused equally by syphilis and small-pox); to which Katherine replies, "A pox of that jest."

Shakespeare was not the only dramatist of his period who used the expressions "Pox" and "Pox on it." They are to be met with in Ben Jonson's plays, as, for instance, in "Every Man in His Humour." A century later than Ben Jonson and Shakespeare the same words were still used in the same way by Colley Cibber in "The Careless Husband," a play which Horace Walpole described as "worthy of Immortality."

Wiseman refers to the opinions of Fernelius (1497-1558), who wrote vehemently against the use of mercury. He also quotes from the writings of medical men who were contemporaries of Shakespeare (1564-1616)—Sennertus (1572-1637), Palmarius (1520-1588), Fallopius (1523-1562)—and gives his own personal experience. His notes of the symptoms and treatment of a very large number of cases under his own care, and his comments on them, stamp him as a remarkably careful observer, and a very able clinician, and prove how common the disease must have been, and how familiar Wiseman (1625-1686) was with it in its various aspects, both in its acquired and congenital forms.

To Dr. Mott we look for much important information, and for valuable suggestions respecting syphilis in relation to public health and eugenics, as well as for additional light on the subject of congenital syphilis. Dr. Mott's special experience in neurology and psychiatry will doubtless enable him to add to our knowledge about the insanity due to syphilis. Perhaps he will be able to explain why the syphilitic virus —the spirochætes and their toxins—has such a selective influence upon certain parts of the nervous system, in producing those progressive degenerations which (1) affecting the different conducting tracts of the spinal cord, result in locomotor ataxy; and (2) affecting the frontal and central convolutions of the cerebral hemispheres, cause general paralysis of the insane.

Why is it that the symptom known as Argyll-Robertson pupil is found almost, if not quite, exclusively in persons affected with acquired or inherited syphilis, and as a rule in them only; and are we justified in inferring from this fact that the syphilitic poison is the cause of locomotor ataxy and general paralysis of the insane, in the great majority of instances?

Does the syphilitic virus, as distinct from the soil it infects, vary in such a way that one variety of it is more likely than another to cause tabes and general paralysis of the insane?

In the syphilis of the offspring, are we justified in attributing a certain class of pathological states, such, for example, as the erroneous growth of bone at the epiphysial lines of the long bones, and on the surfaces of the membrane-bones of the skull, to the general intra-uterine malnutrition, due to placental syphilis of the mother?; and in associating another set of pathological conditions, such as skin rashes and fissures, and snuffles, &c., to the specific inherited qualities and proclivities conveyed directly in the sperm-elements, or in the ovum?

As a syphilitic fœtus produces syphilization of the mother, what is the consequence when this same mother bears children to another man who is not himself syphilitic? Will not the children of such a union also be syphilized?

What degree of tendency, if any, is there for the children of syphilitic parents to be affected with acquired syphilis? Or, in other words, is there an absence of susceptibility to acquired syphilis in healthy persons born of syphilitic parents in whom the disease had well-nigh worn itself out before the offspring had been conceived?

Does present-day experience still go to prove that when an infant with a congenital syphilitic history is properly vaccinated, whilst its

skin is clear of eruption, normal vaccinal vesicles develop and take a normal course to their termination?; and does a healthy child born of non-syphilitic parents, if properly vaccinated from the lymph taken from such correct vesicles of a syphilitic infant, entirely escape syphilis?

Is information forthcoming to explain the variations in the frequency and severity of venereal diseases in the armies of the different countries of Europe and elsewhere?

These are some of the problems which have presented themselves to me and which perhaps may be demonstrated and settled by the opener of the debate in this Section, or by some of the subsequent speakers on "Congenital Syphilis" and on "The Relation of Syphilis to Public Health and Eugenics."

To Mr. D'Arcy Power has been entrusted the opening address on the "Treatment of Syphilis." Mr. Power's extensive knowledge of medical and general literature will no doubt lead him, if he should so think fit, to speak concisely, yet comprehensively, on the old remedies and method of treatment; whilst the special arrangements he has made at his hospital for the close observation of patients undergoing the new treatment, together with his discriminating and unbiased judgment of the results, will doubtless enable him to give valuable information upon a subject of absorbing interest at the present time.

That the three factors of the new treatment of syphilis were all discovered between 1905 and 1909,[1] and were made known at intervals of two years of each other, are facts which have given a fresh and profound interest to an old disease of considerable, if undefined, antiquity; and a new hope in the study of the various extensions and ramifications of the disease, which are the outcome of the researches of the last decade or so.

I have thrown together these suggestions and ideas since I was told a few days ago that I should be expected to make some general introductory remarks to-day; but I feel that I should indeed be inflicting an injustice upon the gentlemen who are here to open the debate, and a great unkindness upon all the rest of you, were I to detain you any longer by any words of my own. I will therefore say no more, except to ask for your forgiveness if you think I have trespassed already too long upon your patience.

I now call upon Dr. Norman Moore.

[1] Schaudinn's discovery of the spirochæte in 1905; Wassermann's test in 1907; and Ehrlich's "606" in 1909.

OPENING ADDRESS, WITH SPECIAL REFERENCE TO THE PREVALENCE AND INTENSITY OF THE DISEASE IN THE PAST AND AT THE PRESENT DAY.

DR. NORMAN MOORE: The poem of Hieronymus Fracastorius which has given its name to the group of diseases due to a specific infection which the Society has decided to discuss, was published at Verona in 1530. There was a copy of this rare book in the library of Dr. Mead in Great Ormond Street, where the Hospital for Sick Children now stands, and he one day showed it to Charles Peters, of Christ Church, afterwards Radcliffe Travelling Fellow and Physician to St. George's Hospital, to whom he had previously commended the study of the poem. Peters in 1720 published a handsome reprint of the poem, with a dedication to Mead.

Fracastorius was a physician of Verona, of large practice and great learning, highly esteemed in his own time for his knowledge, both of medicine and of literature.

Lucretius had first shown to the Romans that poetry might set forth a complete system of philosophy, and that the whole order of Nature might be thoroughly and splendidly discussed in verse. He saw the difficulty of the task—yet accomplished it :—

> "Nec me animi fallit Graiorum obscura reperta
> Difficile inlustrare Latinis versibus esse."

Virgil, following him and meditating on his achievement, applied poetry to a simpler purpose—the exposition of the husbandman's art—and, without losing sight of its details, showed their poetical as well as their practical aspect, and thus did he—

> "angustis hunc addere rebus honorem."

When the revival of learning came and all men were eager to show the goodness of their Latin, nothing seemed more natural than to write on every art and science as nearly in the manner of Virgil as was possible.

Dr. Samuel Parr, whom our great-grandfathers believed to have a good knowledge of Latin, thought the "Syphilis sive Morbus Gallicus" of Fracastorius next in merit to the Georgics among such poems of the Arts and Sciences. The poem is in three books and contains 1,344 lines.

Its first lines sum up the generally received history of the origin of the disease :—

" Qui casus rerum varii, qua semina morbum
Insuetum, nec longa ulli per secula visum
Attulerint: nostra qui tempestate per omnem
Europam, partimque Asiæ, Libyæque per urbes
Sæviit, in Latium vero per tristia bella
Gallorum irrupit nomenque a gente recepit."

The disease was unknown before, says the poet; nothing like it had been seen. In his own time it had raged through all Europe and part of Asia and the cities of Africa, and had come into Italy through the grievous French wars, and so received its name from that nation.

The poet asks the Muse of Astronomy, as the goddess best versed in the effects of the stars and other physical forces, how the disease has arisen :—

" Dic Dea, quæ causæ nobis post secula tanta
Insolitam peperere luem? Num tempore ab illo
Vecta mari occiduo nostrum pervenit in orbem.
Ex quo lecta manus solvens de littore Ibero
Ausa fretum tentare, vagique; incognita ponti est
Aequora, et orbe alio positas perquirere terras?
Illic nanque ferunt æterna labe per omnes,
Id morbi regnare urbes, passimque vagari
Perpetuo cœli vitio, atque ignoscere paucis."

What is the cause of this before unknown plague? Was it borne to our world across the western sea, when from the Spanish shore men dared to cross the ocean and to seek for lands in another world? For there this disease is said to be endemic so that few escape it.

The poet enlarges on the obscurity of the origin of disease in fine hexameters, but in a way that reminds one of Johnson's remark on a passage in Milton's "Lycidas": "How one god asks another god what is become of Lycidas, and how neither god can tell."

The second book deals with the treatment of the disease. The third contains the incident which gives it its name. Siphilus, a shepherd, proud of the flocks of King Alcithous which he is feeding, blasphemes the sun god, and the sun god in revenge darts this disease forth, which attacks the shepherd. He was to be sacrificed, but is saved by the intervention of Juno. The poem ends with lines in praise of Guaiacum and of Peter Bembo, to whom the three books are dedicated by the learned and observant Fracastorius. The poem is contemporary evidence of the prevalence in its time of the hypothesis of the then recent origin of syphilis. It was believed, as Dr. Freind points out in his "History of Physic,"[1] to have been imported by Columbus from

[1] "History of Physic," ii, p. 337.

the West Indies in 1492, and to have first attracted attention at the siege of Naples in 1493-4, when it became diffused in the French army.

Is this account true? Does it show that syphilis was unknown in Europe before the end of the fifteenth century? That it was imported from America cannot be asserted without much more evidence than is generally set forth, and the statement may probably be an inference from the date of the supposed first appearance of the disease in the Old World. On this point I may quote a letter which I received from Dr. Elliot Smith on June 3, 1912:—

"Since last I saw you I have seen Dr. Alěs Hrdlička, Anthropologist to the Smithsonian Institution in Washington, and its delegate to the Americanist Congress in London. His evidence concerning syphilis in America is so nearly similar to my experience in Egypt, that I send you notes upon his statements which he gives full permission for you to use as you may think fit. Many thousands of skeletons from all parts of America (North and South) have passed through his hands, but he has not seen a single case of syphilis (or lesion which competent pathologists will admit to be syphilitic) in any one bone which is certainly pre-Columbian. That there was no immunity to the disease is shown by the fearful havoc worked by it among Indians in post-Columbian times. One Indian cemetery (probably early eighteenth century) in Kentucky had over 70 per cent. of the skeletons severely damaged by undoubted syphilis. Moreover, the Indians (who have remedies for all their own diseases and an intimate knowledge of their symptoms) have no remedy for syphilis and are terrified by it in their impotence to deal with it."

The examination of mediaeval and classical writers on medicine has hitherto yielded very indefinite results. The endeavour, for example, of Francisco Lopez de Villalobos in 1498, to identify syphilis with the condition named Saphati by Avicenna in the fourth book of the Kanûn, is altogether unconvincing, and the same may be said of the many other comparisons collected by Dr. Iwan Bloch, of Berlin, in his "Ursprung der Syphilis" (Jena, 1901).

One difficulty of the identification, of course, is that of distinguishing in words some of the commonest lesions of syphilis from those belonging to other diseases. Most of us have known cases in which the rash of small-pox has been mistaken for a specific eruption and the patient remitted to the skin department of a general hospital; and I can easily believe that patients whose rash is due to syphilis may sometimes arrive at the admission room of a small-pox hospital. The very terms *la grosse vérole* and *la petite vérole* and their English equivalents sufficiently

indicate these resemblances, and show how difficult must be the precise discovery of syphilis among the vast collections of phrases of the mediaeval writers, whether European or Oriental. The same applies to the classical authors, both of medicine and of general literature.

No objection on the score of propriety would have deterred Petronius in the time of Nero from mentioning the subject. More than one dialogue of Lucian might have contained some informing allusion as to other parts of the Roman Empire in the time of Galen. The subjects of the "De XII Cæsaribus" of Suetonius might easily have given one or more examples of the disease, while the VIII Satire of Juvenal, "Stemata quid faciunt?" would almost certainly have contained some allusion to physical deformity or heredity connected with syphilis had the disease been well known in his time. To go to more remote times, Terence, if syphilis had existed in Rome under the Republic, would probably have introduced into the dissolute scenes which his spectators enjoyed, some allusions such as those which pleased English playgoers in the Restoration period. No certain references to syphilis are to be found in the general Greek or Latin literature.

To turn from the descriptions of books to the remains of men, Egypt offers a wide field of observation in which indications of the presence of syphilis in the ancient world might be discoverable. By the kindness of Professor Elliot Smith and of Professor Wahby, I was allowed, in two visits to Egypt, to look at the large collection of partly broken-up mummies and of separate bones in the rooms adjoining the museum of the Egyptian Medical School in Cairo; and, being familiar with the fine collection of bones at St. Bartholomew's Hospital, I was not incompetent to make observations on the subject of the presence or absence of syphilitic lesions. I did not see one specimen. In the *Lancet* of August 22, 1908, Professor Elliot Smith,[1] whose opportunities of observation in Egypt have far exceeded those of all other men, and who has made the fullest use of those opportunities, stated that in no case had he seen in bones, teeth, or soft parts of the many thousands of ancient Egyptians he had examined any lesions at all resembling syphilis. He mentioned that all the cases described as syphilis in ancient Egyptian bones up to August 22, 1908, were examples of the destructive work of necrophilous beetles which had attacked the bones in the grave, long after burial. In a letter which he was so good as to write to me on May 14, 1912, and which he permits me to quote, Professor Elliot Smith says:—

[1] *Lancet*, 1908, ii, pp. 521-24.

"Last year I received from Nubia a skeleton of the Middle Empire period (circa 2000 B.C.) with extensive lesions of the humerus, scapula, sternum, and spinal column, which I submitted to Strangeways in Cambridge. He has made a detailed examination (with the help of Dr. Emily Morris) and written a full report for our final Nubian report, from which I quote (and you are welcome to make any use of these quotations): 'Adult Nubian woman (circa 2000 B.C.). Left humerus: Changes in the shaft present all the appearances of a syphilitic node, but might equally well have been produced by any local inflammatory condition of periosteum. Changes in humerus and scapula closely resemble those found in syphilis, but the change in the sternum and spinal column are rather those of a severe chronic suppuration.' Although the node on the humerus closely resembles a syphilitic node and *would in a recent specimen almost certainly be diagnosed as such*, remembering that syphilis is unknown in bones of Egyptians of this date, it would be unwise to suggest that the changes are due to this disease. Last week Strangeways received another suspicious specimen from me (Egyptian: Ancient Empire from Giza Pyramids); if he cares to commit himself, you are at liberty to use the information he supplies."

In a later letter Professor Elliot Smith adds: "I have seen Strangeways, who tells me that the last specimens I sent him are almost certainly *not* syphilitic."

Thus it is clear that no undoubted syphilitic lesions have been discovered in the vast collection of human remains belonging to several thousand years which have come under the observation of so learned and accomplished an anatomist as Professor Elliot Smith.

Three most learned men have considered the question of whether there are any passages in the Greek and Latin writers of antiquity which prove syphilis to have existed in their times. Jean Astruc, Professor at Montpellier, was of opinion that in the classical writings there was not the least syllable that could properly be applied to it.[1] Dr. John Freind, in his "History of Physic,"[2] discusses some passages in mediaeval writers with the learning which he always shows, and concludes that they do not describe appearances really due to syphilis. His remarks are the more valuable because he had read the whole of the mediaeval authors and was thus familiar with the way in which the same passage, with slight verbal alterations, again and again appears in the pages of one, taken from those of another without the least acknowledgment. He was also deeply read in the classical authors, and thus his conclusion

[1] "De Morbis Venereis," Paris, 1740.
[2] "History of Physic," London, 1725.

that this group of diseases was not observed in mediaeval or classical times is an authoritative opinion. The third physician who has discussed the question with great learning, especially in the classical writings, is Van Swieten. He has considered all that Astruc and Freind have said, and has himself studied the ancient authors, and arrives at the same conclusion as Astruc and Freind, that no passages exist which can prove the Greeks, Romans, or Arabians to have been acquainted with syphilis. The subject affords so many opportunities of illustration that it seems unlikely that the greatest linguistic attainments in Greek, Latin and Oriental languages could do much more to discover the early prevalence of syphilis than the reading of Astruc, Freind and Van Swieten.

Our knowledge of the results of this infection has greatly increased in our own times. Thus since the last meeting of the International Medical Congress in London in 1881 the opinion has slowly been established that general paralysis and locomotor ataxy are always to be counted among the manifestations of syphilis. This conclusion seems to open a new possibility of inquiry into the possible occurrence of syphilis in ancient times. If Galen, practising in Imperial Rome, observed or had heard of the mental delusions of general paralysis, or if in any passage he had described the gait of locomotor ataxy or the sudden falling down of its subjects when they entered dark passages, or otherwise ceased to see their feet, then the opinion that syphilis did not exist in early times would have to be altered, and those passages in classical medical writers capable of interpretation in the direction of syphilis, though insufficiently clear to be taken alone as descriptions of its symptoms, must be reconsidered, and might, some of them, become additional evidence in favour of the early prevalence of syphilis. On the other hand, if none of the characteristic nervous affections of syphilis are discoverable in Galen, the opinion that the disease did not exist in Imperial Rome or in early times will be confirmed.

It was the desire to draw attention to this line of investigation which made me willing to accede to the request of the Council of our Society to open this discussion on syphilis with special reference to the history of the prevalence of the disease in the past. The late Henry Pelham, Professor of Ancient History at Oxford, used often to dwell in conversation on the usefulness of the study of Galen in relation to the life of Rome in the time of Marcus Aurelius, and if a scholar should arise capable of the widest illustration of Galen then the question of whether these two most prominent nervous diseases of syphilis or any others of the same origin existed in his time may probably be finally decided.

Taking without criticism the Galenic writings as they were received in the time of Linacre, I may make some few and imperfect suggestions towards such an inquiry. Charles Victor Daremberg, who afterwards made so many contributions to the history of medicine, wrote his thesis for the degree of M.D. in Paris on "Galen's Knowledge of the Anatomy, Physiology, and the Pathology of the Nervous System."[1] Daremberg first states what Galen knew of the nervous centres, how he dissected the brain, what he knew of its membranes, and of the cerebral surface, of the brain itself, of the pituitary body, the brain substance, and the spinal cord. He then shows how far Galen was acquainted with the cranial nerves, the spinal nerves, and the nerves of the limbs. Some notes follow on Galen's physiological notions and on his experiments. The pathological part of the thesis is the least full, but Daremberg dwells upon one interesting case which Galen mentions in three separate treatises. A sophist had loss of sensation in his two last fingers and in half of his middle finger. Galen asked if he had received any blow or wound of the arm. The sophist answered that he had not. Galen then examined his spine and elicited that he had fallen from a vehicle upon a rugged stone and received the blow between the shoulders, and that a violent pain followed, which in time went away, and was followed by a loss of sensation which grew more and more profound as time went on. Galen thought the effect due to injury of the spinal cord, applied remedies over it, and adds: "I obtained the cure of my patient."[2] The patient was interested in his own case and raised a violent discussion between some physicians who did not belong to the Hippocratic School and who had seen him before, and Galen. The patient wanted to know how a paralysis of sensation only could occur. Galen answered that movement being active it required much force to effect it and a severe injury to abolish it, while, on the other hand, sensation being passive it disappeared under the influence of the slightest cause. They were completely satisfied with Galen's answer, but the physician and philosopher wished to put them to confusion, and asked them how they could explain a loss of the power of movement only. They were not able to do so. Galen explained that there are nerves which belong to the muscles and others which go to the skin. That when the former nerves are affected, motion is abolished, when the latter, sensation. Galen demonstrates that it is

[1] "Exposition des connaissances de Galien sur l'Anatomie, la Physiologie et la Pathologie du Système Nerveux," Paris, 1841.

[2] "De locis affectis," i, p. 6.

possible to determine what part of the spinal cord is affected and which nerve. The case shows very well what close attention he had paid to the anatomy and physiology of the nervous system, and that he had made some progress in those observations which indicate its morbid conditions.

I may proceed to interrogate further Galen himself. His treatise on muscular movement shows that he had investigated and considered all the movements of several muscles, while that "De differentiis morborum" shows close observation of the movements of the limbs. The treatise addressed to Thrasybulus on exercise dwells upon the uses of gymnastics. These three books are perhaps sufficient to show that had Galen seen many men walking in the streets of Rome with well-marked symptoms of locomotor ataxy, he could hardly have failed to mention their kind of gait as an abnormal condition. It may be added that the book "De symptomatum causis," which Linacre translated, shows that Galen had seen cases of spinal injury, and had noticed insensibility and immobility below the seat of such injuries, and that their area depended upon the level of the injury. Tremor and palpitation, general convulsion and paralysis of the whole body after an apoplectic fit—all these he had noticed. He had carefully observed the trembling due to old age, and that produced by wine, and that which we should classify under the term rigor. All these may be mentioned as showing how competent he was to observe so marked a peculiarity of movement as that of locomotor ataxy.

In the treatise on the diseases of the mind and their cure, in which he gives so interesting an account of his own education, Galen deals rather with moral affections than with what we commonly call insanity, yet in the chapter on avoiding insatiability he might easily have mentioned magnificent delusions. He dwells upon the folly of luxury and of the unnecessary multiplication of riches, of garments, and of ornaments, but mentions no delusions in relation to them. The six books of commentaries on Hippocrates' "De morbis vulgaribus" do not contain any allusions to nervous diseases of possible syphilitic origin, but the six books "De locis affectis," besides the case already mentioned, contain other allusions to conditions of the nervous system, and full discussions as to the causes and seat of pain. In these general and complicated discussions as to pain, and its meaning in relation to diagnosis, are there any words indicating the observation of the lightning pains of locomotor ataxy? No distinct mention of them is made. Galen had not observed them, while his observations and commentaries

are so often acute that had such pains been common in the patients of his time it is unlikely he would have overlooked them altogether, though he might perhaps have not distinguished them from the periosteal pains with which the sixteenth, seventeenth and eighteenth century observers probably confused them. The interesting discussions in the third book of the "De locis affectis" on affections of the brain, on convulsions, on epilepsy, and vertigo, on hemicrania, on apoplexy, and on affections of the spinal cord, still further show Galen's attention to nervous disease, and suggest that he is unlikely to have seen any of the prominent nervous affections due to syphilis. When, again, he was considering the pulse of paralytics in his treatise on pulses for beginners he had another opportunity of noticing cases of ataxia had they come into his practice. The sixteen books on the pulse contain some remarks bearing on the nervous system, such as the passage on the pulse of paralytics[1] and that on the effect of meningitis of the dura mater on the pulse,[2] but nothing pointing to a knowledge of syphilitic nervous disease. Nor do the books "De crisibus" and "De diebus decretoriis," nor the fourteen books translated by Linacre, "De medendi methodo."

I have perhaps pursued the subject sufficiently far to show that Galen is not likely to have been familiar with any of the nervous diseases most certainly due to syphilis. The conclusion I would venture to draw, if a critical investigation of all his writings should arrive at the same result, is that syphilis did not exist in Ancient Rome, if no nervous diseases due to it were seen by a physician of such keen observation and such wide experience in practice as Galen. If this be so, it is credible that the period of the first appearance of the disease in Europe was in reality as has been so often asserted, the end of the fifteenth century. That the original habitat of spirochæta remains to be discovered is no objection to this view, since at present very little is known of the history of the geographical distribution of such organisms. I hope the Society will forgive me for drawing attention in so superficial a manner to the desirability of investigating thoroughly all the remarks of Galen on nervous diseases, with a view to the consideration of whether the non-existence of locomotor ataxy and general paralysis in his time may not be taken as a strong confirmation of the Egyptian evidence against the existence of syphilis in ancient times in the countries surrounding the Mediterranean.

[1] "De causis pulsuum," Book IV.

[2] "De præsagitione ex pulsibus," Book IV.

OPENING ADDRESS, WITH SPECIAL REFERENCE TO THE TREATMENT OF THE DISEASE.

MR. D'ARCY POWER: I think the Royal Society of Medicine is to be congratulated upon choosing the subject of "Syphilis" for the first general debate in this its New House which was opened on May 21, under those auspicious circumstances which we all beheld.

Syphilis has been so much discussed and its cause and pathology have been made so clear within the last ten years that the treatment has been revolutionized, and it has become necessary to focus our newly acquired knowledge, avoiding rash generalizations on the one hand and an undue conservatism on the other. The field is already so large that it cannot be surveyed satisfactorily by any single observer, and the Royal Society of Medicine has done wisely, therefore, in choosing several Fellows to open this discussion and in allotting to each of them a separate plot. To me has fallen the consideration of modern methods of treatment, partly, perhaps, because I am engaged in general surgery, and so have no special bias towards any particular plan of treatment, partly because, for some years past, I have kept one or two beds in my male and female wards at St. Bartholomew's Hospital for the special purpose of comparing the relative value of the new methods of treatment.

My colleagues at the Hospital have placed me under an obligation by allowing Mr. J. E. H. Roberts to collate the results of the cases of syphilis which they have treated, whilst Mr. Roberts himself has done excellent service in bringing forward the cases upon the present occasion. The Pathological Department of the Hospital, too, under the able superintendence of Dr. F. W. Andrewes and Dr. Mervyn H. Gordon, has given unstinted help in determining the Wassermann reaction in the patients sent to them, so that we have been able to assure ourselves that every patient who was treated really had syphilis, a point of considerable importance and less easy to determine than might appear at first sight.

The modern treatment of syphilis dates from the years 1905-1909. In 1905 Schaudinn discovered the spirochæte; in 1907 Wassermann published his test; in 1909 Ehrlich issued his remedy "606." It is necessary to remember these dates, for they show how recent is our knowledge and how tentative must be the results in a disease which has such long-continued and far-reaching results as syphilis. It is still much too early to arrive at any final conclusion, and for many years to come it will be necessary to reconsider the results, and perhaps to abandon

many of those points which we now consider to be of the greatest importance.

The first advance towards a scientific method of treating syphilis was made when Schaudinn discovered the spirochæte and brought the disease into line with other diseases of microbic origin. Schaudinn was a skilled biologist, the advances of modern tropical medicine were well known to him, and the weight of his authority was sufficient to stamp the value of the discovery, to fix the position of the micro-organism in the animal kingdom, and to point out by analogy the most likely methods of destroying its virulence.

The second step in the treatment of syphilis was also purely scientific. It was made when Wassermann gave a test founded upon the broad principles of pathology. At first, I fear that I was somewhat sceptical as to the value of the Wassermann reaction, and exactly in those cases where I now know that it is of the very greatest help. It was difficult to believe that a young man, seemingly in perfect health and without a blemish, was suffering from a spirochæte infection simply on a pathological report. I had been brought up in the old school which required some clinical evidence of syphilis before a patient is placed upon a mercurial course. I knew and had taught for years that syphilis was a deceitful disease, the signs and symptoms being intermittent, because periods of apparently perfect health are intercalated with other periods when the manifestations are plain, but it was difficult to rely upon a test made by another person, however skilful and assured he might be. Two or three cases, however, convinced me. In these cases the absence of symptoms, with a very doubtful history of a sore, led me to give too sanguine a prognosis and decided me to await further evidence although the Wassermann reaction was positive. Mercury had to be given in each of these cases, and I now have faith in Wassermann's test, especially in the very difficult cases where it is necessary to obtain evidence of syphilis apart from the ordinary clinical signs. We must recognize, however, that the test has its limitations. The reaction is not given by every patient even when there are obvious signs of syphilis; it is not usually positive until five to eight weeks after infection, when the disease has ceased to be local and has become generalized in the body; it is positive in 95 per cent. of cases of "secondary" syphilis and in 75 per cent. of "tertiary" syphilis, whilst it is said to be positive in only 50 per cent. when the disease is latent. Moreover, it labours under the disadvantage of being a laboratory test requiring a skilled worker to carry out the technique. In the future this may be overcome in one or two ways: either the test will be simplified, or we shall educate the next generation

to become more skilled pathologists than we are ourselves. For the present it is necessary to rely upon the report of another person, and this is somewhat repugnant to my surgical instinct.

Knowing the cause and being provided with a test, it is possible to employ a course of rational treatment in a disease, and this is what has happened in the case of syphilis within the last few years. Mercury even in our own times was administered in all cases of venereal disease associated with a sore. The more cautious, indeed, awaited the appearance of "secondary" symptoms: the bolder gave mercury at once if there was a "hard sore" or "Hunterian chancre." But the drug was given for the most part without any system and without any very clear idea of what was to be expected from its use. Too often it was given merely for the relief of syphilitic manifestations and its administration was stopped as soon as these signs disappeared. Little was known of visceral syphilis; still less of syphilis of the nervous system. It is no wonder, therefore, that syphilis was looked upon as incurable, and it actually was so when mercury was given in this haphazard fashion. The remedy further fell somewhat into disrepute, because it was recognized that many syphilitic manifestations disappeared spontaneously whether or not mercury was given. A school arose, therefore, which maintained that guaiacum, iodide of potassium, arsenic, and other drugs were just as efficacious. It was difficult to gainsay these statements so long as the cause of the disease was unknown and there was a prevailing ignorance of its natural history. We know now that all these drugs may be useful adjuvants but that they do not cure, that is to say, they do not destroy the causal micro-organism in the same thorough manner as is done by mercury.

In this country the rational administration of mercury with a view to cure syphilis, and not merely to relieve the signs of syphilis, is due largely to the teaching and practice of Sir Jonathan Hutchinson. We have learnt from him that small doses of the less irritating forms of mercury administered for long periods of time, whether or not signs are present, are more serviceable than large doses given irregularly and for short periods. But as the cause of syphilis was unknown, Sir Jonathan had only his own acumen and experience to guide him, and it was impossible for him to communicate his knowledge personally to any very large number of medical men. His methods, however, came into general use, and even before the discovery of the spirochæte it was admitted that syphilis was curable by those who were prepared to take an infinitude of time and trouble about their disease. There was, however, no test upon which reliance could be placed as to whether or not a patient was cured,

and the answer to the question had to depend upon time and circumstances. It was impossible, therefore, to lay down any definite rule as to the duration of a mercurial course, or when it might safely be stopped. The length of a course was dependent upon the presence or absence of symptoms after the administration of mercury had been stopped for a longer or shorter period. It resolved itself eventually into the administration of mercury with regulated intervals for about two years. If at the end of this period no fresh symptoms appeared within the next three months the patient was thought to be cured. It is needless to say that this empirical method often led to disappointment, and sometimes to disaster. It became clear, too, as our physicians learnt to associate some of the commoner diseases of the brain and spinal cord with imperfectly treated syphilis, that very many cases of the disease were never really cured, although there had been no external signs for many years after treatment had been discontinued. The Wassermann reaction seems to give us the necessary clue to the process of cure, and syphilis can only be said to be cured, in the light of our present knowledge, when the test is negative and the patient remains without symptoms for at least a year after the use of mercury has been discontinued.

The first variation from the methodical and routine administration of mercury by the mouth or by inunction was the use of intramuscular injections. Experience soon taught that it was especially serviceable in those terrible forms to which the term "malignant syphilis" is applied, when mercury was not well tolerated by the digestive tract, and when the patient could not be trusted to follow out his orders. The method of intramuscular injection was reduced to a system by the late Colonel F. J. Lambkin, A.M.S., whose recent untimely death in South Africa we all deplore. The creams of calomel and metallic mercury which he invented give excellent results. Less pain attends their use than when other formulæ are employed, and in no case in my own wards was their administration followed by troublesome symptoms when care was taken to avoid the nerves and blood-vessels of the part where the injections were made. We learnt that it was better to have the creams dispensed in single doses, each in an hermetically sealed tube, rather than to fill the syringe from a larger supply which had been repeatedly heated to render it fluid. It was necessary also to sterilize the syringe by boiling it in olive oil immediately before use. The cream and the oil then mix readily, whereas bubbles are formed when the cream is drawn up into a syringe which has been boiled in water. The routine treatment was identical with that recommended by Colonel Lambkin, viz :—

(1) Four injections of calomel cream delivered deeply into the gluteal region. Each injection consists of 10 minims of cream, the dose containing ½ gr. of calomel. The cream is made according to the formula:—

℞	Calomel		5 grm.
	Creosote Camphoric acid	āā	20 c.c.
	Palmitin basis		100 c.c.

(2) The injections are made at intervals of a week, and at the end of a month they are replaced by a cream containing 1 gr. of metallic mercury in every 10 minims. The composition of this mercurial cream is:—

℞	Metallic mercury		10 grm.
	Creosote Camphoric acid	āā	20 c.c.
	Palmitin basis		100 c.c.

Two weekly injections of this metallic mercurial cream are given.

(3) No injections are given for two months after these six doses have been administered.

(4) An injection of metallic mercurial cream is then given every fortnight for two months—i.e., four injections.

(5) No injections are given for four months.

(6) An injection of metallic mercurial cream is given every fortnight for two months—i.e., four injections.

(7) No injections for six months.

(8) An injection of metallic mercurial cream is given every fortnight for two months—i.e., four injections.

(9) No injections for one month.

(10) An injection of metallic mercurial cream every fortnight for two months.

Colonel Lambkin's method marks a distinct advance in the treatment of syphilis. He regarded mercury as a curative agent and gave it systematically to that end. It is well adapted for the Army and Navy, and it can be employed advantageously in hospital practice if the patient can be interested in his cure and will attend once a week for the injection. It is less fitted for private practice, where the mere mention of a needle prick is often sufficient to frighten a patient, whilst the needle itself is necessarily longer and stouter than that of a hypodermic syringe. Unskilful technique, too, has caused hæmatomata, painful indurations, and, occasionally, deep-seated abscesses. The knowledge that such accidents have happened has also had some effect in limiting the more extensive use of a valuable method.

In private practice, therefore, when a patient has declined to be treated by intramuscular injection, and recourse has been had to perchloride of mercury and iodide of potassium, I have tried to make the mixture somewhat less nauseous than that usually prescribed. I applied to Mr. Langford Moore and Mr. S. Tweedie, the dispensers at St. Bartholomew's Hospital, who recommended the following formulæ after making a large number of experiments. In both prescriptions the metallic flavour of the mercury and the acridity of the iodide of potassium are successfully masked. The first prescription is for those who like sweet tastes; the second is for those who prefer bitters.

(A) ℞ Liq. hyd. perchlor. ... ... ʒi
Potassii iodidi ... ... ʒi
Syrupi ... ... ʒi
Mucilag. tragacanth. ... ... ʒii
Ol. amygdal. essent. (sine HCN)... ... ... mii
Ol. cinnamomi ... ... mii
Aq. chloroform. ... ... ad. ʒviii

M. Fiat mist. Sig., take an eighth part three times daily.

(B) ℞ Liq. hyd. perchlor. ... ... ʒi
Potassii iodidi ... ... ʒi
Tinct. chirettæ ... ... ʒiss.
Elixir glucidi ... ... mxl
Infusi. gentianæ ... ... ad. ʒviii

M. Fiat mist. Sig., take an eighth part three times daily.

I cannot speak from personal experience of the value of the arylarsonates and soamin. The accounts of these remedies did not appear to be very satisfactory, and before I had made up my mind to employ them they had been replaced by "606."

"606," or salvarsan, has been extensively employed in my wards, and I think highly of it as a remedy for syphilitic manifestations, although we have not yet sufficient evidence to prove that it cures the disease. I have always used it, therefore, as an adjuvant to mercury and never by itself or in place of mercury. The present working hypotheses are that salvarsan kills adult and free spirochætes, whilst it has but little effect upon those which do not lie in close relation with the blood and lymph paths, upon the immature forms, or upon those which are in an intracellular stage. Whether or not we choose to employ one of these hypotheses two great facts stand out in connexion with the action of salvarsan in syphilis—it cures the symptoms of syphilis in a shorter time than mercury, and it can be employed as a test for syphilis, because in many cases when salvarsan is given to a syphilitic patient whose Wassermann test is negative the first effect of the injection is to

render the reaction positive, although in a short time it again becomes negative. Salvarsan promises, therefore, to be useful as a test for the cure of syphilis effected by other means.

Much benefit is derived undoubtedly from the use of salvarsan, but it is useless to expect miraculous effects from a single injection. It is necessary to use salvarsan methodically if the best results are to be obtained, and the injections must be repeated at intervals of a fortnight to a month. If they are given too near together time is not allowed for the large dose of arsenic to be eliminated, and the symptoms attending the second dose may be more severe than the first.

The net outcome of our experience with salvarsan has been that it serves as an excellent adjuvant to mercury in the treatment of syphilitic lesions. It has proved especially useful in cases of chronic superficial glossitis, in active syphilitic periostitis, and in ulcerating syphilides of the skin. It has been less serviceable in cranio-tabes, and in cases of osteitis associated with the formation of sequestra, because in these conditions the pyogenic organisms are more important than the syphilitic infection; neither have the results been very satisfactory in cases of syphilitic arthritis, doubtless because many of these inflammations are also associated with a tuberculous infection.

So far as we have been able to ascertain no serious accident has occurred in our cases. We have recognized that we were dealing with a powerful arsenical compound and we have endeavoured to eliminate the more obvious risks. Personally I have not hesitated to use it in private when the patient desired it, and in one case of parasyphilis I administered it—against my own wish, but at the earnest desire of the patient himself—to a gentleman who had suffered for many years from tabes. He had a definite Wassermann reaction, he was nearly blind, and he was unable to walk more than a short distance. I assured him that there was very little probability, on pathological grounds, of his obtaining any good from salvarsan and that he might be made worse. He decided, however, to take all risks, and felt so much relief from the first injection that he determined to have a second. The first injection was given on July 5, 1911, the second on October 4—0·5 grm. each time. He writes on May 8, 1912: "I can now hold my water unless the bladder is overfull from drinking three cups of tea or taking a little sherry, and then I pass it more easily than I used to do before treatment. I have improved in balance since the last injection, as I can stand up on my toes without touching anything; my hearing has remained almost normal since December—that has improved more than anything else—my sight has improved a little. I can see light more strongly, but it is

very little better. My knee-jerks are no better, and my memory, I think, is not so good for names of people and places." To this his wife adds: "His memory is quite as good or better. I am very pleased to tell you what I think of the treatment you gave my husband last year. It has done him a great deal of good and his life is more worth living, though the cure is, of course, not complete. To give you details: Before the treatment I always had to wait for a favourable opportunity. Now his mind is clear and he can grasp and discuss serious affairs at any time. Formerly he could not be left alone at any time. He could not remember to sit still and he could not cross a room without help. His balance is now so much better that he can walk anywhere in the house and garden by himself. He is safe not to fall. I am to tell you he can stand up straight alone and put on his coat by way of proving that the balance is good. His hearing is very much improved. The control of the bowels returned, but I regret to say that since January it has failed, and it appears to be weaker than during the previous six months. The bladder remains as before. Now and then he can pass water without the catheter, but with no regularity. As regards distance: Leaning heavily and distressing himself considerably, he could walk two miles. He can now walk the same distance with ease and not leaning on anyone—simply being guided—and will probably walk another mile later in the day. His eyesight is not improved." This patient seems on the whole to have received benefit from salvarsan, and it will be interesting to discover from similar cases how far this was due to the drug and to what extent it was the result of a firm belief in its efficacy. The positive Wassermann reaction proves that the spirochæte infection was still active, though it was many years since he contracted the disease. The results seem to show that—as might have been predicted—his cerebral functions have improved whilst his spinal nervous system remains unaltered.

The routine of our technique for administering salvarsan has always been the same. It has been given by intravenous injection and the patient has been kept in bed for twelve or fourteen hours previously. The skin has been prepared by sterilizing it with a 2½ per cent. solution of iodine in rectified spirit. The veins are rendered prominent by the application of a fillet and when the veins at the bend of the elbow have been used the patient has been made to grasp a ruler or a staff as in the days of blood-letting. The needle used is that recommended by Mr. J. E. R. McDonagh, which is bevelled to a point on the upper surface instead of beneath, as is usual, and it is provided with a slightly concave plate of metal to allow it to rest more securely on the skin—two small

modifications which make the operation of puncture of the vein much easier. It is unnecessary to give either a local or a general anæsthetic when the veins can be made prominent, as is usually the case. A skin incision is unnecessary, and with a well-directed and sharp needle the pain is momentary and infinitesimal. The syringe and needle are filled with freshly prepared and sterilized salt solution and the vein is punctured obliquely upwards. The fillet is then relaxed and the contents of the syringe are emptied into the vein. This will show whether the vein has been fairly entered, for if it has been missed or transfixed the salt solution will form a bulla owing to the extravasation of fluid into the surrounding tissues. If the salt solution enters the vein freely the syringe is filled with salvarsan solution, which is then injected by syringefuls at a time until the whole pint has been introduced. When all the salvarsan solution has been introduced a syringeful of salt solution is injected to wash out the needle and to free the tissues from any salvarsan which might be adherent to them. Both the salvarsan and the salt solution are kept at a temperature of 105° F. The needle is then withdrawn, a pad and bandage is applied over the prick, the arm is kept in a sling for a few hours, and the patient is allowed to get up when he feels inclined to do so. The salvarsan solution is made by dissolving 0·6 grm. of salvarsan in a pint of sterilized salt solution made from freshly prepared distilled water. The solution is neutralized or rendered faintly alkaline by the addition of a 1 per cent. solution of sodium hydrate. It is necessary to have everything sterile, and it adds greatly to the ease of the operation if the rubber junctions connected with the three-way tap of the syringe are made of such thick rubber with so small a bore as to make it difficult to fit it on to the tap, because the ordinary thin rubber tubing is apt to slip off or become flattened if it kinks whilst suction is being made. When the needle is once fairly introduced care must be taken that it does not slip out of the vein. It is therefore handed over to the sole care of the patient if he is competent to look after it, or, if he is nervous, it must be given in charge of a nurse. If the needle slips out or if the vein is not punctured fairly at the beginning of the operation it is much better to employ another vein rather than to attempt any rectification in the one first chosen.

Our chief difficulties have been to find a suitable vein. Many of the men have been so self-indulgent as to become fat with lax tissues, whilst in women it is by no means easy to make the veins of the arm prominent. In some cases we have used the internal saphenous, in others it has been necessary to give a general anæsthetic and dissect out the vein at leisure.

The sequelæ have been trivial—headache, a rise of temperature to 103° to 104° F., with or without a rigor, vomiting, diarrhœa and stomach-ache, but they have caused no anxiety and the patient has always been well on the following day. In the majority of cases the patients felt no inconvenience at all. Two bad results have come under my observation, and in both an intramuscular injection of salvarsan had been made. In one case there was extensive sloughing of the tissues at the seat of the injection; in the other an œdematous and painful swelling occurred just below the angle of the left scapula where salvarsan had been injected. The swelling suppurated, was laid open, and in due course healed; but it was a long time before the œdema disappeared.

It seems, therefore, that salvarsan is a useful remedy if its limitations be recognized. How far it is useful in the initial stage of syphilis I do not know from personal experience. It is said that primary sores heal much more quickly if salvarsan is given than if it is not given, but the glands which enlarge as the result of the infection remain harder than normal although they have returned to their natural size. The drug is very useful in the secondary and tertiary stages in those cases where the lesions are due more to syphilis than to pyogenic micro-organisms. It is servicable, too, in some cases of parasyphilis, but to what extent and under what conditions I am again ignorant from want of personal experience. We still need more information about the treatment of heredo-syphilis with salvarsan, whether it is best to treat the child through the mother, by means of an intermediary, as a goat or cow, or directly—i.e., the child itself.

Broadly, I do not think that syphilitic lesions are more completely cured by salvarsan than they are by mercury, but the result is certainly brought about more speedily; and as the administration of salvarsan does not prevent the simultaneous employment of mercury, the two remedies may be serviceably used together. But until we have much more evidence and have proved its efficacy in a very large number of cases for a period of several years we shall not be justified in saying that salvarsan cures syphilis.

It is necessary to ask, what are the contra-indications for the use of salvarsan? About twenty-three fatal cases have been recorded, but several of these were moribund at the time of administration, some were associated with a faulty technique, and in others it was clear that the drug should not have been employed. It seems from the evidence at present available that the intravenous injection of salvarsan is a safe method of procedure if care be taken to administer it in a rational

manner and to reasonably healthy persons. The further question then arises, at what period in syphilis should salvarsan be given? The working hypothesis is that the drug destroys the active spirochætes with which it is brought into contact. If this hypothesis is correct the sooner a dose is given after infection the more likely it will be to answer its purpose, for we must assume that the microbes are localized at first to the seat of inoculation and to the neighbouring lymphatics and glands. We should also like to know how often salvarsan must be given and at what intervals, remembering that it is desirable to give as few injections as is consistent with a due regard to complete destruction of the spirochætes. I hope that these points will be emphasized and that we shall obtain answers from those who will take part in the discussion with a larger and more special knowledge than I possess.

What is the best treatment for syphilis in the present state of our knowledge of the disease? Taking for the sake of example the case of a surgeon or of a nurse who is inoculated in the course of professional duty, the wound should be well washed under running water, like a wound obtained in the post-mortem room. It should then be dried and covered with an ointment consisting of 10 grm. of calomel in 30 grm. of lanoline. This mercurial ointment should be gently rubbed into the wound for five minutes and a dose of salvarsan (0·6 grm.) should be given intravenously. The prophylactic action of the mercurial ointment appears to end—at any rate experimentally—within twenty-four hours of inoculation; the salvarsan is said to be serviceable in checking the generalization of the disease even when the seat of inoculation has become characteristically indurated and the lymphatic glands are enlarged. The fact, however, that the lymphatic glands do not return wholly to their natural condition after the administration of salvarsan in early syphilis rather inclines me to distrust the drug as a sole remedy, and should lead one to give mercury in some form or another as soon as possible.

A Wassermann test should be made at an early period after inoculation, although it will probably be negative in the very earliest stages, for as has been stated already, our present knowledge shows that it is usually positive in five to eight weeks after infection; it is positive in 95 per cent. of cases during the secondary stage, and in 75 per cent. during tertiary manifestation, but it is only positive in 50 per cent. of cases where syphilis is latent. Mercury should be given at once when the infection is undoubted, but in the more difficult cases, where the diagnosis is doubtful, it may be withheld until a positive Wassermann's reaction has been obtained.

Preferably the mercury should be given by intramuscular injection, and Colonel Lambkin's formulæ are quite satisfactory so far as I have used them. If, for any reason, the intramuscular method cannot be employed inunction may be employed. But when, as in ordinary private practice, the drug has to be given medicinally, I think the perchloride is better than the grey powder which has been used in England for a long time past. It seems to me that so long as mercury is given in small doses, systematically, and for long periods of time, the exact preparation used does not matter very much. The mercury should be given with regulated periods of rest and a Wassermann's test should be made at the end of each period just before the mercury is recommenced. If the test is negative an intravenous injection of salvarsan may be given with a view to elicit a positive result. If the test still remains negative after this injection the mercury may be discontinued for a further period so long as there are no signs of syphilis, and at the end of a further period of two months the test should be again employed. Marriage may be permitted under these conditions when the Wassermann test has remained negative and there have been no syphilitic symptoms for at least a year.

In this manner we shall avoid giving mercury for a longer time than is absolutely necessary, whilst the drug will be continued until the syphilis is cured, even when the absence of symptoms might seem to make a further mercurial course unnecessary. We shall also have taken a step in advance of Colonel Lambkin, who was obliged to take a time limit with absence of symptoms as a guide for discontinuing a mercurial course because the scientific test had not yet become available.

There are two objections to the use of Wassermann's test as an answer to the question whether or not an individual is cured of syphilis. In the first place, it is purely a laboratory test, and we are consequently at the mercy of the pathologist who makes this examination. This objection is easily surmounted by always employing the same pathologist. The second objection is much more serious. It involves the fallacy that Wassermann's test is an absolute proof of the presence or absence of spirochæte infection, and even in the light of our present knowledge it is certain that we are not justified in making such an assumption, for the figures given above show that the test remains negative in a certain percentage of cases in every stage of syphilis. Clinical experience and the caution begotten of it must still guide our advice therefore, but recent advances in the treatment of syphilis have been so great that we need not despair of obtaining some reliable test even in the immediate future.

OPENING ADDRESS, WITH SPECIAL REFERENCE TO THE RELATION OF THE DISEASE TO PUBLIC HEALTH, INCLUDING CONGENITAL SYPHILIS.

DR. F. W. MOTT, F.R.S.: Allow me to thank you for the honour you have done me in asking me to open the discussion on "Syphilis in Relation to Public Health." If I confine my remarks more particularly to the effects of syphilis upon the nervous system, it is not because I do not recognize the great importance of affection of other systems, particularly the heart and great vessels, in relation to public health, but because others will take part in this discussion who are more competent by experience and knowledge to deal fully with these matters, of which I have had far less experience.

There are no reliable statistics to show whether syphilitic infection is more prevalent at the present time than formerly; however, it is my conviction that severe bone diseases, skin diseases, and visceral diseases, due to syphilis, are not nearly so prevalent as they were when I was a student. But this did not prove that fewer people *pro rata* are infected (although it may be so); for the cause of diminution of severe lesions may be due to an increase of latent syphilis.

It is certain that with the conversion of the rural into an urban population, the more ready mingling of the town and country population, the short military service, and the frequency with which soldiers were syphilized by service in India, and other causes incidental to life in large cities with their armies of professional prostitutes and clandestine prostitutes, the possibilities of a general and widespread syphilization of the race has occurred, since the development of the railway system in England.

The struggle for existence is falling more and more upon the nervous system, and thus it becomes the *locus minoris resistentiæ;* consequently functional neuroses and psychoses are more prevalent. The conditions which favour cerebral and spinal neurasthenia, combined with an acquired widespread racial immunity to severe forms of lesions, due to the reaction of the cells of the body to the syphilitic virus, appear to me to have had a determining influence in the increased production of the late syphilitic manifestations of disease of the nervous system which are usually termed parasyphilis or metasyphilis. They are generally looked upon as degenerative processes of the nervous

system; they are insidious in origin, progressive in character, and comparatively true syphilitic diseases of the nervous system, uninfluenced by antisyphilitic treatment.

It is impossible in England to arrive at any definite conclusions regarding the prevalence of syphilitic infection among the population; likewise it is impossible to arrive at any conclusions relating to the frequency of the incidence of disease of the nervous system caused directly or indirectly by syphilis; still, the experience of general practitioners, general physicians, or nerve specialists, is to the effect that syphilis is by far the most important cause of disease of the nervous system. With the present system of relief at special hospitals, general hospitals, asylums, and infirmaries, a process of selection occurs which interferes with the preparation of reliable statistics regarding the percentage of cases of syphilis that develop syphilitic disease of the nervous system. Yet the importance of the subject is great from the public health point of view, for if the virus attacks the nervous system it is rarely that a complete and permanent cure results even with efficient treatment. I am of opinion that when the roseolar rash occurs, or a secondary eruption indicating spirochætes in the blood, a similar infection of the membranes may occur which may lead to disease of the nervous system. Some degree of paralysis, feeble-mindedness, or functional defect will be left in consequence of syphilitic disease of the nervous system in the great majority of cases, even if they are treated; in many instances in spite of treatment the patient is left with an invalid brain rendering him more or less helpless.

I collected a large number of cases (over forty) in my own experience of syphilitic cord and brain disease about fifteen years ago, and I then expressed the opinion that many of these cases had been cured by efficient treatment. Subsequent experience has taught me to modify my views. The majority of these cases have since died from the disease, or complications arising from the disease, after having been paralysed, and this in spite of treatment.

Statistics have been made on the Continent relative to the incidence of nervous disease occurring in syphilitic subjects. Hjelmann found fifteen to twenty-five cases of disease of the nervous system per 1,000 persons infected with syphilis, excluding general paralysis and tabes. Reumont gives 8·5 per cent., including tabes, and Engelstedt 5 per cent. White and Melville, in the *Lancet* of December 9, 1911,[1] "Venereal Disease, its Present and Future," state: "Dr. Blaschko gives as his

[1] *Lancet*, 1911, ii, p. 1618.

opinion that in Berlin every man who reaches the age of 30 has (on an average) had gonorrhœa twice, and every fourth or fifth man has had syphilis."

In Scandinavian countries, where syphilis has for a long time been a notifiable disease, recognition of the association of general paralysis and syphilis took place long ago; as it was first called attention to by Kjellberg and Jessen. In countries where there is a State control of the hospitals and where everybody must pay for treatment, or if he or his friends are not able to pay, the village or town where he lives must, more reliable data are possible than in this country, especially the Metropolis, where there is no system or organization. When the Insurance Act is brought into force there will be an excellent opportunity of ascertaining the prevalence of venereal disease. I understand that venereal diseases will not be a bar to medical benefits; consequently reliable statistical records can be prepared of a large proportion of the population. Such statistics will give approximately reliable information regarding the prevalence of gonorrhœa and syphilis.

Douglas and Melville point to the fact that the number of recruits annually rejected from the Army for venereal diseases shows a great and steady decrease in syphilis, from the appalling figure of 16 per 1,000 to 1·5; whereas there is a steady persistence of other venereal diseases at the level of about 2 per 1,000 for the last forty years, rising in the last five years to 3·5, and still apparently increasing. By these figures they conclude "a decrease of all venereal diseases to a quarter of its former bulk, the whole decrease having occurred in syphilis. Syphilis," they say, then, "from being a deadly pestilence has become of manageable dimensions, while the inroads of gonorrhœa are increasing." In my opinion this statement, however, requires some qualification, or a false conclusion will be arrived at.

Let us first consider gonorrhœa and syphilis as regards (1) the individual; (2) racial immunity.

An attack of gonorrhœa does not give immunity, and several, even many, infections may occur. The chances of cure are greater, but its re-infection can happen; the opportunities of acquiring it from prostitutes are much greater than in the case of syphilis, for although the influence of the virus remains in the body, it is rarely communicable to others after three or four years have elapsed, even though the infected individual may be suffering from serious symptoms. Gonorrhœa sterilizes, male and female, by inflammatory lesions; it has no influence on the developing ovum; one attack does not give an immunity the same as syphilis.

ATTENUATION OF THE VIRUS OF SYPHILIS AND RACIAL IMMUNITY.

(*a*) A widespread deadly disease produces immunity by killing off all those most susceptible to it, leaving those who are less susceptible or immune to propagate; this presupposes that the body tissues can set up a defensive reaction to the multiplication of the specific organism. Sooner or later the virulence of the disease dies down because there are increasing numbers of the population who either have it in a mild form, a latent atypical form, or possess a complete immunity. Whether a father who has had syphilis and has not communicated it to the mother is able to transmit any immunity to the offspring is a moot point, for it implies the transmission of an acquired character, which to many scientists is a biological heresy. However, there are reasons for supposing that pathological variations of the germ-plasm may result from nutritional disturbances of the sexual cells, occasioned by prolonged toxic conditions of the blood.

(*b*) The more systematic and universal reliance placed upon mercury and less upon iodide for treatment in the primary and secondary stages than formerly, and the continuance of the treatment for several years. This has greatly diminished the virulence of the organism in the body, and may have so modified its virulence that when it passes into the body of another person it produces a less virulent reaction. Experimental observations upon trypanosomes suggest that this inference is justifiable. Thus mercury by its widespread use may have attenuated the virulence of the spirochæte generally in this country.

(*c*) Antisepsis and asepsis, by destroying or withholding the influence of secondary microbial infection, have been largely instrumental in averting some of the serious skin, bone, and visceral lesions, and have aided greatly the defensive reaction of the body against the specific organism.

(*d*) The less consumption of alcohol and diminished prevalence of drunkenness have increased the defensive reaction of the tissues as well as rendered people less liable to infection.

As I have said before, the stress of modern life falls especially upon the nervous system, rendering it more susceptible to pathological, especially degenerative, conditions, and an important question to answer is this: are the late parasyphilitic (Fournier), metasyphilitic (Möbius), quaternary syphilitic affections (Bosc and Hirschl) more prevalent now than formerly? If so, is it due to the widespread existence of racial immunity and latent syphilis? Seeing that both tabes and general

paralysis are incurable diseases and affect especially male adults of civic worth, it is a matter of great social and economical importance to the race to know whether these diseases are on the increase or not. Of 2,000 post-mortem examinations made at Claybury, 500 were general paralytics. About 40 per cent. of the male deaths are due to general paralysis. The riddle is still unsolved, why only a small percentage relatively, possibly 3 to 4 per cent., of persons infected with syphilis should suffer with one of these degenerations termed parasyphilis, but only 15 per cent. of persons suffering with diphtheria develop diphtheritic paralysis. These are frequently cases in which the local infective process was mild, or even unobserved; in that respect, therefore, like parasyphilitic affections, which much more often than not follow mild and even unrecognized primary infections and secondary symptoms.

I have been astonished by the fact that in spite of the promiscuous sexual life led by a number of general paralytics I have never seen a primary sore, nor do I remember to have seen a secondary eruption. Of the 500 cases on the post-mortem table I was struck by the fact that the evidence of severe syphilitic lesions, with the exception of nodular fibrosis of the aorta, was very scanty; in fact, cases diagnosed as general paralysis with marked signs of gummatous lesions on the body or evidence of coarse paralysis, nearly always turned out to be pseudo-general paralysis due to multiple syphilitic lesions affecting the membranes and vessels, involving secondarily the brain. Again, the experience of Krafft-Ebing so strongly supported the view that if there were no syphilis there would be no general paralysis, that he caused to be inoculated with the virus of a hard chancre nine persons suffering from this disease, who had never shown any signs and gave no history; not one of these were infected, for they showed no signs although they were watched for a considerable time. It was concluded they had an acquired immunity. Why is this? Is it because the virus is attenuated or modified and thereby has acquired a special neurotoxic action? Or is there a particular form of spirochæte analogous to the trypanosome of sleeping sickness which causes affection of the nervous system? Or is it because in a small percentage of individuals the cells of the body, *including the cells of the nervous system (normally protected against reactions)*, offer a hypersensitive reaction to the virus? There are many facts which suggest the possibility of a certain form of virus with a neurotoxic action. Thus Babinski remarks that it seems possible that a syphilitic virus may sometimes be endowed with a particular aptitude for attacking the nervous system, and instances have been cited

which show that infection by a particular woman has led to several or even more men, with whom she cohabited, suffering later with parasyphilis. Probably the most striking example supporting this theory of a special neurotoxic virus has been afforded by Brosius, who relates that seven glass-blowers suffered with chancre of the lip, and out of five who came under observation ten years later four suffered with either tabes or general paralysis.

The specific agent, the spirochæte, may exist in some modified form, or it is conceivable that there may be varieties of this protozoal organism, as there is of the malarial parasite or trypanosome. Again, the organism may become modified by the widespread use of mercury. It may thus happen that the virus may vary in different cases of infection. This, however, is speculative, and not only is not supported but also is to some degree contradicted by experiments on animals. We are therefore probably on more certain grounds in attributing the variations of the effects which follow infection not to the variation of the virus, but to the defensive reaction of the individual himself, and we may represent this in the form of an equation :—

$$\text{Symptoms } X = \frac{V}{R} = \frac{\text{Virus}}{\text{Resistance}}$$

If the virus is constant, the resistance must vary according to conditions which lead to racial immunity, family immunity, acquired immunity, and general bodily health.

It appears to me that a race long syphilized is, on account of a racial immunity, more liable to suffer with the late degenerative forms. The interesting description given by Colonel Lambkin of the syphilization of the natives of Uganda shows how severely a race previously free from this disease suffers from malignant skin, bone, and visceral disease. He also points out that parasyphilitic affections are rare; the reason being that the disease has not existed in the country for a sufficiently long time to allow of their frequent occurrence. According to von Halban, tabes is more common in Abyssinia than it is in Vienna; but then syphilis has been in existence in this Christian country for a long time past.

Congenital Syphilis in the Offspring.

It is often asserted that early marriages lead to degeneracy in the offspring. In studying the pedigrees of congenital syphilis I was struck by the fact that the man was very frequently quite young when he

married. The sexual instinct had lead to his acquiring syphilis when young, and he married young and infected his young wife, with the usual result, miscarriages, stillbirths, children dying in infancy of convulsions, meningitis, and hydrocephalus, followed later by children who lived but suffered in various ways physically and mentally. This, to the lay mind, would be all put down to the early marriage and imperfect development of the reproductive organs; but Nature does not make the mistake of exciting an instinct prematurely. I shall now show pedigrees illustrating this fact and others in connexion with congenital syphilis.

My inquiries regarding the results of conceptions in syphilitic parents illustrate the following points: The usual history is either complete sterility, or miscarriages, abortions, stillbirths, children dying in infancy of convulsions, marasmus, meningitis, or hydrocephalus; then there may follow children who are *apparently* healthy, but who in later life develop *syphilis hereditaria tarda*, manifested often by interstitial keratitis, nerve-deafness, bone, skin, and visceral lesions. The children may be stunted in growth and show obvious stigmata of congenital syphilis, in the form of Hutchinsonian teeth, saddle-shaped nose, and linear scarring around the angles of the mouth. At puberty it may be noticed that the genital organs remain infantile in development, and microscopic observations which I have made on the sexual glands in such cases show atrophy or degeneration of the germ-cells; in the case of the male organ an absence of the spermatozoa; in the case of the female a failure of development of the ova and a great diminution in numbers. This infantilism is frequently associated with various grades of idiocy or imbecility. Congenital syphilitic children presenting those well-determined stigmata may subsequently develop juvenile general paralysis, tabo-paralysis, tabes, primary optic atrophy, epilepsy, chorea, hysteria, and meningitis. But it is more common to find *apparently healthy* children born of syphilitic parents subsequently, about puberty or adolescence, developing the various nervous affections mentioned above. It seems as if the virus, as it becomes attenuated, is delayed in its destructive effects, and numbers of family histories I can cite show, as a general rule, but by no means invariably, that as the virus becomes attenuated the conceptions may result eventually in healthy children, who in later life manifest no visible signs or symptoms of the disease. But, as Fournier remarks, the birth of healthy children is "no free pass" for future offspring, and the following cases illustrate this fact:—

F. C., aged 11, suffering with blindness since he was aged 7, was

brought by his mother to Charing Cross Hospital; she said the child had had snuffles at birth. There was no family history of nervous disorder or insanity (fig. 1). Three years elapsed after marriage before a seven months stillbirth occurred, then (1) a girl was born who died with fits at 1 year 9 months; (2) a girl, living, quite healthy; (3) a girl, living, quite healthy; (4) *the patient;* (5) *boy, aged* 9, with paralysis; (6) *boy,* who suffers with fits. The patient when brought to me exhibited no external signs of syphilis on the body and no evidence of visceral disease. There was slight evidence of paresis of the lower face muscles of the right side, and the tongue on protrusion deviated to the right. There was optic atrophy in both eyes, also cycloplegia and iridoplegia. The fifth was a boy, aged 9, and I found him to be suffering with left facial nerve paralysis; the paralysis came on when he was

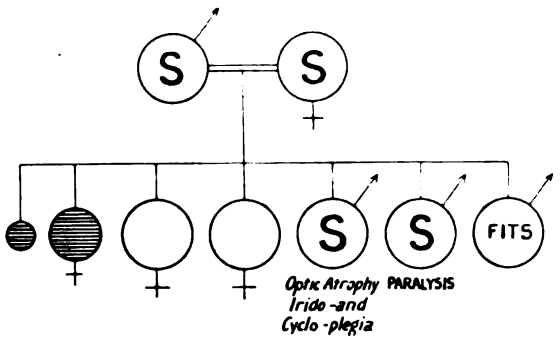


FIG. 1.

aged 6 months. The eye could not be closed, nor the forehead wrinkled; the mouth was drawn to the right, but not markedly. There was no deafness; he could hear a watch equally well in either ear and at a normal distance. This was probably due to a syphilitic affection of the facial nerve. The sight was now becoming defective in the left eye, the disk being pale with a sharp edge, and probably he will become blind like his brother. The practitioner who sent this patient to me treated the mother for acquired syphilis. The history seems to point to the fact that the treatment by mercury for some time led to the birth of two healthy children (2 and 3); it was then suspended, and three children, including patient, were then born, all of whom were seriously affected.

In the following case (fig. 2) there was no history of the mother having been treated for syphilis.

Girl, aged 14, was admitted to Claybury Asylum suffering with juvenile general paralysis, with well-marked signs of congenital syphilis —viz., notched teeth, rhagades around mouth, saddle-shaped nose. History: No insanity, direct or collateral. Father died of an accident, aged 46. History from mother: Mother was married at 20, father at 22. There were twelve children, as follows: (1) dead, 5 months fœtus; (2) dead, 5 or 6 months fœtus; (3) dead, 6 or 7 months fœtus; (4) dead, 7 months fœtus, lived eight hours: (5) born alive, very frail and delicate, ulcers on legs, inflammation of eyes; (6) patient; (7) girl, living, well, aged 16; (8) boy, living, well, aged 14; (9) boy, living, well, aged 12; (10) boy, died of *convulsions*, aged 11 months; (11) girl, died at 8 months of *brain disease* and *club-foot;* (12) boy, living, well (fig. 2). The patient was an intelligent girl and passed the seventh

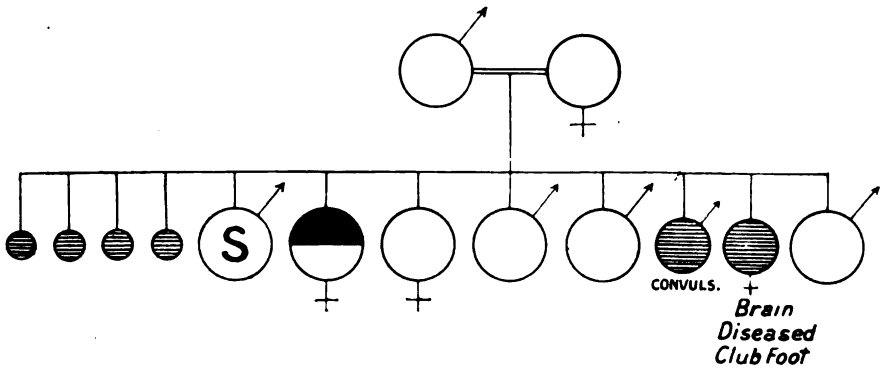


FIG. 2.

standard at 12 years old; developed signs of general paralysis and progressive dementia at 14, and died three years later of this disease. It will be seen that 7, 8, 9 are living and well, then follow two children with nervous affections and death.

As a contrast to the above two cases I may mention the following case, which shows that the defensive reaction of the body apart from treatment plays an important part in subsequent developments (fig. 3).

R. D., a carpet planner, was admitted into Charing Cross Hospital under my care, suffering with well-marked signs and symptoms of tabes. He gave the following history of conceptions following his marriage at 22, which was just two years after he had contracted syphilis with a hard chancre, for which he was treated with mercury for only two months. The first child was born within one year of marriage and is alive and well; he has had six healthy living children,

one of whom died, aged 9; there were also twins. Why was the wife not infected? Had she inherited immunity? These are questions which might well be asked in respect to this case. Healthy children are occasionally born between diseased children; as we have seen above, this may be sometimes accounted for by treatment of the mother.

Hochsinger throws doubts upon a healthy child slipping in between diseased children. Until the introduction of the serum reaction we had no means of knowing whether a child of syphilitic parents was free from taint, for although we are unable to see any external signs of disease yet the internal organs may be extensively diseased, and the following case illustrates this fact most conclusively:—

A. R., male, aged 22, was admitted to Claybury Asylum. He was an able-bodied seaman, but had been invalided on account of fits. There were no external signs of syphilis on the body. There was a

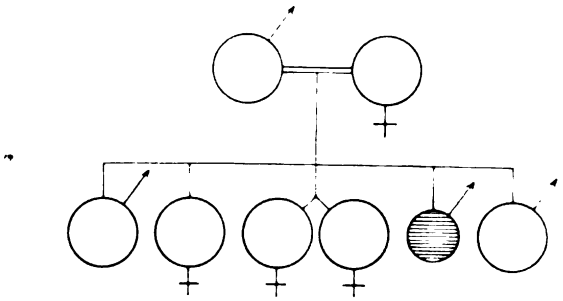


FIG. 3.

history of his father having died of general paralysis in Banstead Asylum, and of a brother, a weak-minded imbecile, being at that time in Caterham Asylum. Whereas the patient, A. R., presented no external signs of syphilis, his imbecile brother had the characteristic nose and teeth. A few days after admission to Claybury Asylum he had a succession of epileptiform seizures; his temperature rose to 108·2° F.; the temperature was reduced by cold sponging, but he never regained consciousness, although the convulsions ceased upon the administration of chloral. The case turned out post mortem to be a typical case of general paralysis. Although there were no external signs of syphilis, the liver showed an extraordinary condition; it weighed 1,200 grm.; the left lobe consisted almost entirely of nodules varying in size from a pea to a marble united to one another by dense bands of fibrous tissue. The right lobe was also nodular in places, and the capsule was here and

there thickened, so that portions of the organ were partially separated. Microscopic examination showed peri-hepatitis and extension of the dense fibrous tissue along the vessels and bile-ducts. There was nodular fibrosis of the aorta, otherwise no signs of visceral syphilis were discovered.

Max Nonne believes that it is not impossible for a healthy child to slip in between two unhealthy ones. He states that within the last few years a large amount of material precisely controlled and obtained from the Engel Reimer division of the Hamburg Hospital (St. Georg) has shown that not infrequently such a thing happens.

I have met with numerous cases in which the mother has had a series of healthy children, followed by miscarriages, stillbirths, and children dying in infancy, followed by syphilitic and parasyphilitic children. These cases often show the necessity of a systematic inquiry of the results of every conception, for the following case of juvenile general paralysis and optic atrophy was shown to me as a case in which syphilis could be excluded, as there was a large, healthy family and no history of syphilis of the parents; yet a systematic inquiry showed clearly that the reverse was the case, and in spite of the denial of the father that he had suffered with venereal infection, and of the mother that she had ever suffered with any signs or symptoms which could be associated with acquired syphilis, the history clearly points to maternal infection after she had had a family.

A. B. was a bright, intelligent girl, who passed the sixth standard of the Board School and gained several prizes. She left school at the age of 13; her periods never came on, and this was the assigned cause of her complaint in the notes received from the infirmary, where she was diagnosed as an imbecile due to congenital brain disease (fig. 4). I took the following notes on the case: She is now aged over 15; she is completely blind in both eyes, she is quite childish, but will talk and answer questions, but in the manner of a little girl aged 6 or 7. She has no delusions or hallucinations, is obedient and now takes her food, although on admission to the asylum she was noisy, crying, and troublesome. She was sent as an epileptic, but she has had no fits while in the asylum. Apparently, from what the mother tells me, she had several fits (like fainting attacks) while in the infirmary. She sits in a chair all day; the legs are rigid and semiflexed, the knee-jerks are not obtainable. She continually fidgets with her hands. I observe only slight tremor of the lips and tongue. The pupils are of medium size and do not react to light. There is primary optic atrophy on both sides. She has never

complained of headache, and there has been no vomiting. She does not respond to the calls of Nature and passes urine and fæces unheeded. She recognizes her friends when they come to see her and talks to them affectionately. Her palate is high and narrow; the teeth show no signs of congenital syphilis, nor were there any stigmata on the body observed by the medical officer on examination; syphilis was not therefore suspected. I interviewed the father and mother. Both said there was no insanity or nervous disease on either side. The mother informed me that she had had fourteen pregnancies. The patient was the next to the youngest living child. Prior to the birth of the patient she had had eight children, all of whom are now alive and grown up and some are married; then she had *two miscarriages followed by twins born dead*, followed by the patient, who had snuffles and a rash on the bottom soon after birth, for which she took her to St. George's Hospital, where they

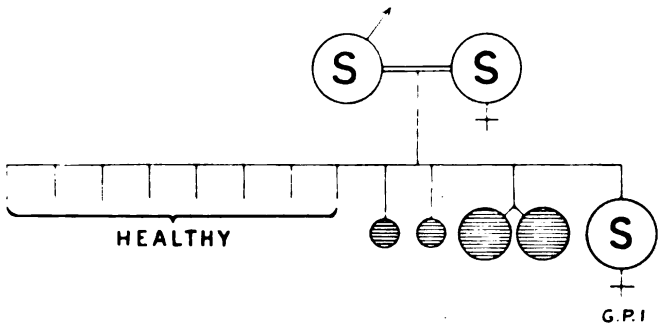


FIG. 4.

gave her grey powders; she did not continue the treatment long. The dementia and paralysis are progressing. This is in all probability one of those cases of the husband acquiring syphilis during the pregnancy of the wife and subsequently communicating the disease to her, with the usual result as regards further conceptions.

Again, a long mercurial treatment of the father, although usually protecting the offspring from congenital syphilis, does not give a positively certain voucher of freedom from taint, as the following case shows: An intelligent professional man acquired syphilis; was treated by eminent authorities with mercury for several years; four years after the primary sore he consulted an eminent specialist as to the advisability of marriage: he was assured that there was no danger to his wife or offspring. He waited a year and married, with the following results: The first two children were born alive but died within a day or two of

birth; the third developed keratitis and otitis with deafness; this child was seen by specialists, who pronounced the affection to be syphilitic; the fourth developed general paralysis and died with characteristic lesions at one of the London asylums; the last two are now bright and healthy children (fig. 5). The microscopical investigation of this case was ably carried out in the laboratory by Dr. Rondoni. The clinical notes and results of this investigation were published in the *Proceedings*.[1]

As an explanation it may be surmised that either he infected his wife by his sperm or that this was a case of spermatic infection by the male without the wife being infected. She had no signs or symptoms of syphilis, but that proves nothing. It is probable that the spirochæte had taken up its abode in the lymphatics of his testicles and had not

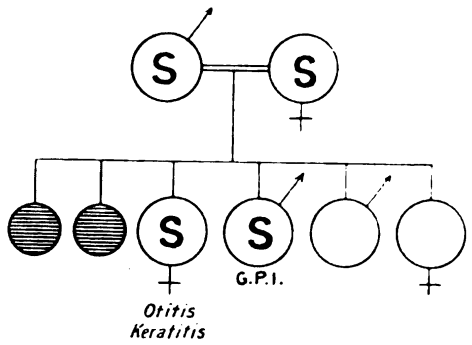


FIG. 5.

been destroyed in spite of the adequate treatment he had received for several years.

Sir Jonathan Hutchinson asserts that "a large experience on this point has led to the conclusion that a man rarely becomes the father of a syphilitic child if an interval of two years has elapsed since the disease was acquired." Now we have reason to believe that the specific cause of infection is a living organism and that the testis is not an unusual location of it; moreover, the living organism may remain latent for a long time, consequently the sperm may be infected long after the primary infection, and this may explain the case referred to; likewise if the ovaries are infected it may explain the fact that although the law of gradual diminution of virulence and risk of transmission holds good, yet exceptions may occur, as the following remarkable case reported by Molénes shows: "A woman, aged 44, was married at the age of 21 to

[1] *Proc. Roy. Soc. Med.*, 1909, ii (Path. Sect.), pp. 101-08.

her first husband, by whom, after the birth of a still living child, she was infected by syphilis. An energetic treatment of husband and wife with mercury and iodide was adopted. In the course of the following years she had six children who all died at ages from 18 to 20 months from symptoms of meningitis. Six years after the death of the husband she married a healthy widower, father of two healthy children aged respectively 16 and 19. She now manifested a recurring syphilitic psoriasis for which she received courses of treatment. Twenty-two years after the primary infection and by her healthy husband she gave birth to a child. This child, just as the former ones, died at the age of 18 months with symptoms of meningitis (convulsions, vomiting, and coma)." (Max Nonne.)

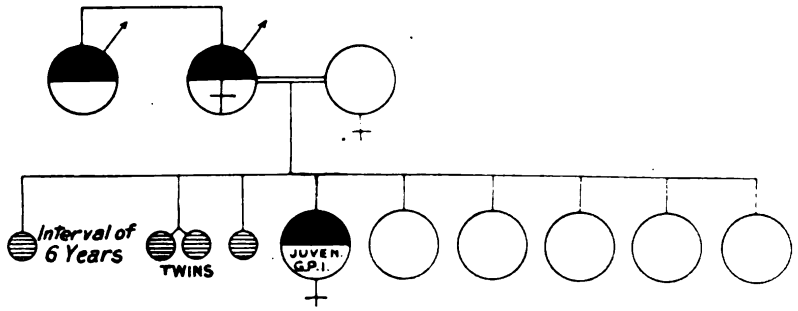


FIG. 6.

The following cases are some selected from sixty cases of juvenile general paralysis, and each presents some interesting feature in the clinical case or history:—

E. E. G., aged 19, single, no occupation, was admitted to Hanwell for juvenile general paralysis. The patient's father was a soldier for seven years, then a carpenter. The mother was aged 24 when married. She said he was a loose-living, drunken man. She was pregnant ten times. The first was born dead at six months. Before her second there was an interval of six years; then a premature birth of twins; the next was born dead at the fifth month. After this the patient was born at full time, and the birth was natural and uncomplicated. She was delicate and had snuffles, but there was no rash on the buttocks. She was a mental defective who could neither be taught to read or write. At 12 years of age she had her first fit. She never menstruated. She became progressively demented and paralysed, exhibiting the characteristic features of the disease in the adult form, except that she exhibited none of the characteristic delusions (fig. 6). A full account

of this case, and the microscopical and other features of the brain, were published in the *Archives of Neurology*.[1]

Probably some or even all of the apparently healthy children who follow this paralytic child would give a positive Wassermann reaction, and have an acquired immunity against the disease. We do not know what proportion of the population may have such an acquired immunity from this cause; it is possible that a very considerable proportion may.

In the next pedigree we have again an example of healthy children being no free pass to the future offspring, as we have a juvenile paralytic developing after the birth of five children who grew up and were apparently healthy. These again may really have an acquired immunity, and would, if they had been tested, have yielded a positive Wassermann reaction.

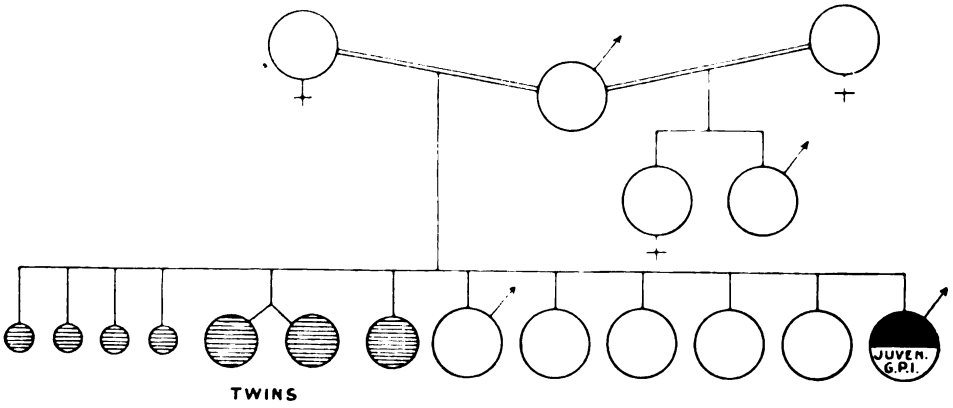


FIG. 7.

F. P. C., admitted to Claybury Asylum suffering from juvenile general paralysis. He is aged 17, but looks like 12. There is an absence of development of the secondary sexual characters (infantilism). The history was that at first he got on well at school and passed the sixth standard. He had no fits, and went to work at a wine merchant's when he left school. Then his memory became bad and he occasionally began to have lapses of consciousness. Subsequently he was admitted to Claybury Asylum, where he became progressively weaker and more demented; he died, and the brain showed all the signs of general paralysis.

I am indebted to Dr. Henry Head for the family history taken at the London Hospital; it is indicated in the accompanying figure (fig. 7). The first four children died in infancy; then came twins who died in

[1] *Arch. of Neurol.*, 1899, i, pp. 250 *et seq.*

infancy; the seventh, eighth, ninth, tenth, and eleventh, living and healthy; the twelfth is the patient. Father married a second wife; there are two children, both living.

M. T., female, age at the onset of disease, 14; died, aged 23, of general paralysis. Family history (from the mother): Parents married at the ages of 28 (father) and 23 (mother). Father was alcoholic and died of general paralysis of the insane; mother has marked tertiary syphilitic sores. Patient was the elder child; she was born three years after marriage. The next pregnancy resulted in a miscarriage at three months. The next and last child is apparently healthy (fig. 8).

T. C., male, age at the onset of the disease, 14; died at the age of 19 of general paralysis of the insane. Family history: Father is in Claybury Asylum suffering with insanity and a history of heavy drinking. Paternal and maternal grandmother of the patient, T. C., were

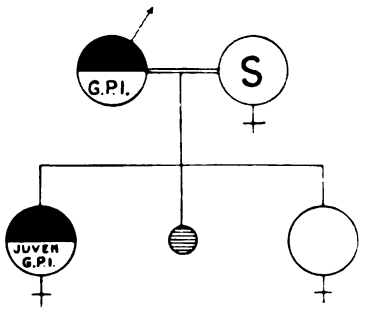


FIG. 8.

drunkards, as are all the father's brothers and sisters. There were eight children in the family; no miscarriages. (1) Died at the age of 2½ months—the doctor said that if it had lived it would have been blind and an idiot; (2) the patient, who was a mental defective of stunted growth; (3) a healthy girl; (4) a boy, aged 14, of stunted growth with typical signs of congenital syphilis—nose, teeth, rhagades; (5) a boy, aged 11½, now healthy; (6) a girl, died of bronchitis when 1 year old; (7) a boy, aged 8, now healthy; (8) a boy, aged 6, not strong (fig. 9). This shows how unevenly the children may be affected by the syphilis of the parents.

E. N., aged 19, was admitted into Caterham Asylum in January, 1897, for general paralysis of the insane and from which he subsequently died. The history of this case points to infection of the husband after marriage and communication to the wife, with the result that syphilitic offspring follow healthy offspring. The mother

gave the following history: Daughter, alive, aged 27; son, alive, aged 24; then followed four miscarriages in succession; an infant which lived only three months; an infant which lived six months; then the patient who, at the age of 14, was able to earn eight shillings a week, so must have been fairly intelligent; a daughter who is now aged 12; then a miscarriage; lastly a frail little girl, aged 8 (fig. 10). No insanity, paralysis, or fits on either side in the family. It is probable that the two last surviving children, if they have not actually suffered from specific disease, have suffered in vital energy.

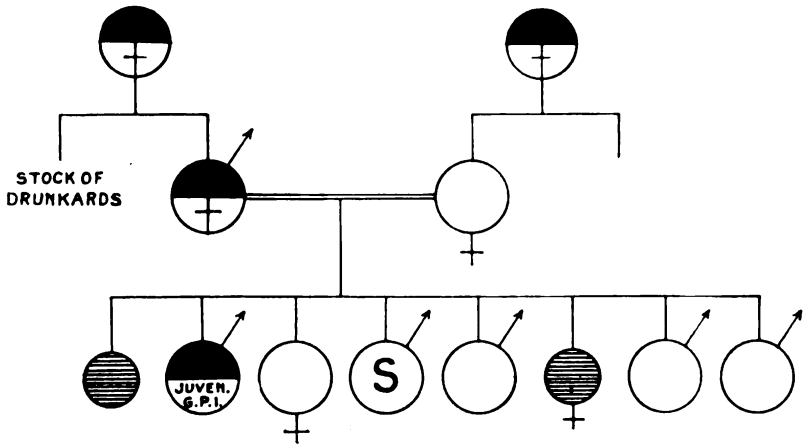


FIG. 9.

E. B., female, aged 14, was admitted to Darenth Asylum for Idiots. History from mother: The child was born at full time; labour normal. The father died of phthisis. The mother is alive, aged 34. Parents married when aged 20. Symptoms of mental disorder first noticed when the patient was aged 12½. She became progressively helpless and demented. The following pregnancies occurred: (1) Miscarriage; (2) miscarriage; (3) child born at full time, lived 15 months and died in convulsions: (4) patient, labour natural; (5) girl who has become blind (fig. 11).

This illustrates the effects of early marriage in relation to syphilitic infection. If the man marries at an early age he, having acquired syphilis, generally infects his wife, because a sufficient length of time has not elapsed for the disease not to be communicable.

The following is not an uncommon cause of congenital syphilis. A great many instances have occurred within my knowledge of married men going to South Africa during the war getting infected; returning

home, they have infected their wives and produced syphilitic offspring. This case shows the same thing: A married man has a healthy son, contracts syphilis while away on a voyage. On his return he infected his wife who did not suffer in any way beyond three miscarriages and giving birth to a dead child, followed by apparently healthy children, although it is probable that all these children would have given a positive serum reaction and have an acquired immunity or partial immunity.

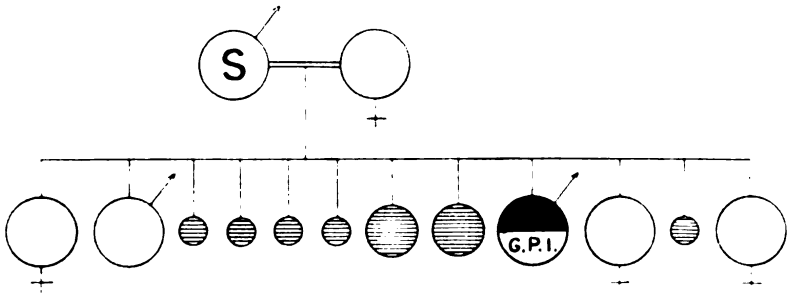


FIG. 10.

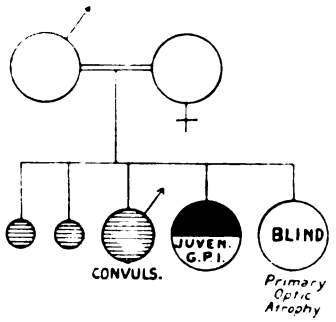


FIG. 11.

General Paralysis in Parents and Offspring.—In about 20 per cent. of the cases of juvenile general paralysis I have found that the father has died in an asylum of general paralysis: seeing that there are a large number of female paralytics (the proportion in the asylums being three males to one female), it is rather remarkable that in sixty cases of juvenile general paralysis I have not met with an instance in which the mother has died of general paralysis. Yet we see that similarity of the mental disease occurs in 20 per cent. of the instances, and according to my experience the father is the parent almost invariably affected. In other forms of insanity due to the neuropathic taint the mother transmits twice as frequently as the father, and daughters are affected twice as

frequently as sons. The difference is this: that general paralysis is an acquired disease due to the late effects of the syphilitic virus, and hereditary neuropathic tendency plays relatively only a small part in the production of this organic brain disease. Now while I do not say that instances may not occur of the mother suffering with general paralysis, and one or more offspring dying later of the same disease, I am of the opinion that as I have not met with them they must be very rare indeed. How can this extreme rarity be accounted for? Clearly neuropathic tendency cannot play an all-important part, otherwise we should expect to have had instances of female paralytics with paralytic offspring; juvenile paralysis occurs with equal frequency in the two sexes as might be expected, seeing that there is a comparative numerical balance of the sexes, and the essential cause is syphilis; unlike the conditions met with in the adult form, the syphilitic virus is liable to affect both sexes equally. The fact that women acquire syphilis much less frequently than men would not account for the fact that I have found no instances of mothers with general paralysis having paralytic offspring. How can the fact be accounted for? Most of the mothers of paralytic offspring have not apparently suffered with any severe symptoms; they have been immunized. Again, a large number of the paralytic women in asylums have undoubtedly been prostitutes, and have therefore become sterile; in connexion with this it may be mentioned that 50 per cent. of the female paralytics dying at Claybury Asylum were found, post mortem, to have suffered with old salpingitis. Moreover, a woman who had been infected and afterwards suffered with general paralysis is less liable to have living offspring.

Congenital Syphilis and General Paralysis in Later Life.—In my Croonian Lectures upon the "Degeneration of the Neurone" (1900), I remarked that it is very probable that some of the cases occurring in adults, in which acquired syphilis can be excluded with certainty, may still owe the disease to congenital syphilis. It is not even necessary, as quite one-half of the cases show, that they should exhibit any external signs of congenital syphilis, for many juvenile patients can be proved beyond doubt, as the following cases I have collected show, to have been born of syphilitic parents. Although themselves manifesting no external signs of syphilis, yet the history of miscarriages, stillbirths, and children dying in infancy of meningitis, hydrocephalus, or of brothers and sisters with well-marked stigmata or evidence of *syphilis hereditaria tarda*, disclosed the necessary proof of the congenital taint. Sometimes no history may be obtainable and there

may be no signs of syphilis on the body; even in some of these cases a definite proof of the possibility of congenital syphilis may be forthcoming by a little investigation. Thus, I was asked to see a patient at Cane Hill Asylum, who was suffering from advanced general paralysis. He was almost speechless, had great difficulty of swallowing, his saliva dribbled from the angles of the mouth, all four limbs were in a condition of spastic contracture, and there was loss of control of the sphincters. There were no signs of syphilis on the body, and the only information obtainable was that he had had a fit at the age of 18; he had married when young, and his wife had given birth to a dead child, and had left him because of his "strange" conduct. He had more fits, and became slowly and insidiously more demented, and died at the age of 28. At the autopsy the most advanced condition of paralytic brain degeneration was found. I subsequently found that his father had died eight years previously of general paralysis in Claybury Asylum. I have once or twice met with instances of father and son being in the asylum together suffering with general paralysis. The case above referred to was one of juvenile general paralysis, commencing in adolescence, but running a slow course. Doubtless the fit at the age of 18 was the commencement of the brain decay, and had lumbar puncture been performed and the cerebrospinal fluid been examined, a lymphocytosis and a positive Wassermann reaction would have been obtained. Just as in the cases of acquired syphilis the onset of general paralysis may in rare cases be greatly delayed, so in the juvenile form there may be great delay in the onset of the parasyphilitic affection. Thus a patient with general paralysis died at Banstead Asylum who had previously been under Dr. Percy Smith at Bethlem Hospital. This woman had characteristic signs of congenital syphilis, but she did not manifest symptoms of paralytic dementia until she was aged 30. She was an unmarried woman, and there was no evidence to show that she could have acquired the disease. Recently Christian Müller has put forward the same hypothesis to explain parasyphilitic disease affecting patients in whom no history of acquired syphilis can be obtained. He described two cases of women (virgins) who were the subjects of well-marked signs of congenital syphilis, and who died of general paralysis at the ages of 42 and 43. The symptoms were not noticeable until a year or two before death.

Tabo-paralysis and Optic Atrophy.—The following case of tabo-paralysis is of interest, as gastric crises at the age of 9 were the first symptoms that brought the patient under observation. P. C., aged 9,

was admitted to Charing Cross Hospital suffering with attacks of vomiting and occasionally diarrhœa. She had well-marked obtrusive stigmata of congenital syphilis, in the form of Hutchinsonian teeth and rhagades. I ascertained that the father had died some years previously at Banstead Asylum of general paralysis; his notes gave no history or signs of syphilis, although the fact of his having had syphilis was clearly demonstrated in his offspring—a not unusual occurrence. The mother had not suffered with any sign or symptoms, but prior to the birth of the patient she had had one miscarriage and two children born dead. The paroxysmal attacks of vomiting had commenced at 7 years of age. She had become progressively enfeebled mentally, and was sent to Darenth, where I again saw her. She still suffered with vomiting and lightning pains; the pupils were unequal, inactive to light and accommodation; the knee-jerks were absent. Both eyes showed

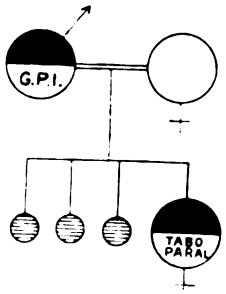


FIG. 12.

well-marked choroido-retinitis and optic atrophy. There was anæsthesia of the chest to light tactile impressions from the third to the sixth rib inclusive. Beyond childish mental enfeeblement there was nothing in her mental condition; there were no delusions, illusions, or hallucinations. Later she suffered with a sudden painless swelling of one knee, and a dorsal dislocation of the head of the right femur. There were no teeth in the lower jaw, and the alveolus was so much absorbed as to resemble the jaw of an old woman. Then she began to have fits and lapses of consciousness, sometimes biting her tongue and passing her urine and faeces under her. She died, after some years' residence in the asylum, of tabo-paralysis, the brain and spinal cord presenting the usual appearances met with in that condition (fig. 12).

Two brothers, sons of a dissolute but clever father, who deserted his wife, came under my notice at Hanwell. The mother gave a history of miscarriages, then two boys were born apparently healthy. One became

blind at the age of 7; the other at the age of 11. They were at the Normal School for the Blind, but were sent away on account of fits. Believed at first to be epileptics, they were treated as such, but a progressive dementia brought them to the asylum, where they subsequently died of general paralysis.

According to my experience optic atrophy in juvenile general paralysis is commoner than in the adult form. A certain proportion of these cases are probably not true optic atrophy, but are the result of syphilitic brain disease.

Syphilitic Lesions of the Nervous System.

True syphilitic diseases of the nervous system in congenital syphilis are nearly always combined; thus we find a generalized leptomeningitis and pachymeningitis, small and large gummata, gummatous neuritis and endarteritis associated in varying degrees. A certain number of such cases have been recorded—e.g., Sir T. Barlow recorded the case of a male infant, aged 15 months, who had weakness of facial muscles and nystagmus. At the post-mortem examination small conical tumours were found in the fourth, fifth, sixth, seventh, and eighth nerves, at their point of exit from the brain stem; these appeared to be of a gummatous nature. There was an associated endarteritis; the basilar and all the vessels of the circle of Willis were extensively diseased; these were opaque, dirty-white in colour, and almost cartilaginous in consistence; the lumen was greatly narrowed by thickening of the interior, and the small arterioles of the pia mater were similarly affected. N. Chiari has recorded a case of endo-, meso- and peri-arteritis syphilitica in an infant, aged 15 months, and Bury, Money, Jürgens, and many others have published similar cases. The following case, which was published in full detail in the "Morison Lectures,"[1] is of great interest, for it illustrates the fact that a typical congenital syphilitic child may attain a fair degree of intelligence, and then, owing to a latent virus becoming active, at puberty suffer from a universal and progressive gummatous meningitis and endarteritis.

E. M. A., female, aged 16, admitted to Claybury Asylum on August 30, 1905; died July 8, 1906. Her mother had three miscarriages, then five children born alive and well, of which the patient was the last (fig. 13). She was delicate from birth; she had snuffles and coryza, and was treated with grey powder; she was undersized, looking about 11 years of age, and had well-marked Hutchinsonian teeth; she must have been fairly

[1] *Arch. of Neurol. and Psychiatry*, 1909, iv, p. 58.

intelligent, as she was in the sixth standard at the Board School. On admission she was thought to be a congenital imbecile suffering from mania. Her conduct had changed; she sang snatches of music-hall songs, and played with dolls like a child aged 6. Although easily excited, she was liked and spoiled by the other patients, who treated her quite as a child. The diagnosis was juvenile general paralysis. For three months before death she had stiffness and rigidity of the neck; she became drowsy and helpless, and there was an internal strabismus of the right eye. At the autopsy a generalized cerebrospinal gummatous meningitis and universal perivascularitis and endarteritis were found; all the arteries of the circle of Willis showed a profound peri-arteritis and obliterative endarteritis; the perivascular and neoplasmic infiltration was universal, it corresponded entirely in its histological characters with a gummatous meningo-encephalitis. At the upper part of the spinal cord the roots

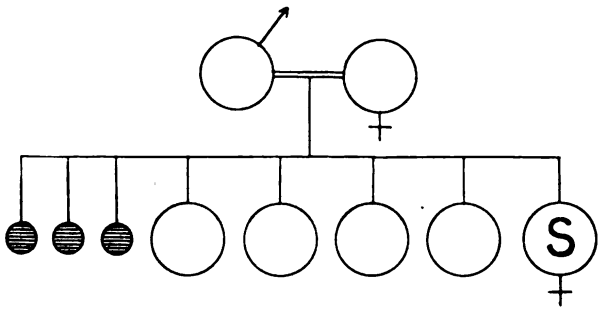


FIG. 13.

were surrounded by an infiltrating exudation quite 3 mm. in thickness. The neoplastic formation consisted of proliferated, branched, and spindle-shaped connective tissue cells, and round or oval cells forming all stages between lymphocytes and plasma cells; there were also macrophages, but polymorphonuclears were conspicuous by their absence; large numbers of the cells were undergoing a granulo-aqueous degeneration. Considering the universal vascular change and perivascular infiltration affecting the vessels of the brain and spinal cord, including the roots, it was astonishing how little had been the destruction of nerve cells and fibres.

Another case of *syphilis hereditaria tarda*, which occurred at Claybury Asylum, has been investigated by Dr. Rondoni. The patient, a girl, was healthy until aged 14; she afterwards became dull and apathetic; suffered with fits (apoplectic), coarse tremor of arms, nystagmus, exaggeration of the knee-jerks, and inequality of the pupils. The family

history obtained was two miscarriages; one boy who lived only 7 months; a boy who lived 15 months; then the patient, who died at the age of 23; then came a healthy living girl; and lastly a girl who lived only 16 months (fig. 14). Rondoni found an old diffuse endarteritis syphilitica with numerous small aneurysmal dilatations, especially of the arteries of the basal ganglia. The arteritis was evidently of long standing, for many of the small vessels in the basal ganglia showed calcareous infiltration and patches of old softening, which can be correlated with the apoplectic fits. The small veins are also affected. Rondoni considers this to be a case of *syphilis hereditaria tarda* of the nervous system similar to the cases of Homén and La Chapelle. The cases of Homén differ only because they were familial (five brothers and sisters). In Homén's cases there were diffuse degenerative changes in the cortical cells without granulation in the ependyma, arterial lesions and softenings in the

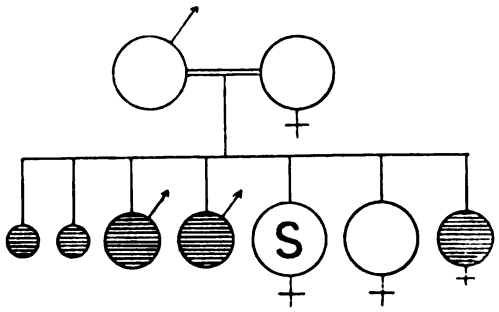


FIG. 14.

basal ganglia, little proliferation of glia, and only a slight perivascular infiltration. Homén's cases were as follows: At the ages of 20, 12, and 20, the disease manifested itself by phenomena of vertigo, headache, disturbances of general well-being, diminished intelligence and weakness of memory, diffuse vague pains in the legs, oscillating gait, and difficulty of speech; the intellectual loss proceeded to an actual dementia; the speech disturbance was rather inability to initiate than to articulate. Spastic conditions in the legs came on, also in the arms; in both situations it progressed to actual contractures. Pupil phenomena and anæsthesia were absent. In all three sisters a certain degree of infantilism occurred.

In both the cases I have related the patients showed a marked degree of infantilism of the generative organs. In view of the results obtained by Kretschner, who has shown that *syphilis hereditaria tarda* is associated with a lymphocytosis of the cerebrospinal fluid, it would

have been interesting if lumbar puncture had been performed in these cases.

Cases have been recorded of syphilitic disease of the brain, the spinal cord, their cavities and vessels (Dowse, Siemerling, Bury, Böttiger, Pick, Hutchinson, and others). The nervous disease of the child began in the case reported by Dowse at the age of 10, Siemerling's at the age of 6, and Bury's at the age of 8. An especially large single gummatous tumour situated in the occipital lobe which had led to erosion of the cranium has been recorded by Hutchinson in a girl aged 16. Hutchinson looked upon the case as one of *syphilis hereditaria tarda*.

As in acquired syphilis so in the congenital form, cases of so-called syphilitic meningitis or meningo-myelitis are in reality not localized to the spinal cord, but affect also the base of the brain and its stem; they are really cases of cerebrospinal meningitis in which the cerebral symptoms are slight and the spinal symptoms obtrusive; consequently, it is not surprising that there are no recorded cases, so far as I can find, of congenital spinal syphilis, although in all cases of diffuse meningitis and arteritis the spinal structures participate in the form of disease of arteries and veins, of circumscribed and diffuse infiltrating gummatous neoplasms, meningitis and neuritis affecting the anterior, and especially the posterior spinal roots. In fact, all the evidence tends to prove that in congenital syphilitic disease of the nervous system multiple combined affections are the rule, and it seems probable that tissues which are undergoing development afford a more congenial soil for the specific organism to grow and multiply in; consequently infection of the central nervous system is especially liable to lead to severe disturbances and loss of function and early death when it is not immediately fatal. Moreover, although other etiological factors—e.g., alcoholism and mental stress—do not directly play a part, yet it is probable that alcoholism, and particularly a neuropathic or psychopathic taint in progenitors, play an important part as contributory factors in the later development of general paralysis, optic atrophy and tabes, also epilepsy, hysteria, and other neuroses of congenital syphilitic children.

Can Syphilis be transmitted to the Third Generation? — The following case lends probability to the assumption that it can. E. H., aged 34, came to Charing Cross Hospital, accompanied by her elder sister. She complains of pains in the limbs; she is very deaf, especially on the left side. She has typical Hutchinsonian teeth. Her sister also has typical notched, peg-top-shaped central incisors, and old keratitis. The sister, a married woman, gives the following history: Her mother

had three premature births, then two children born dead, then one which lived 16 months. She came next, and the patient, E. H., was born a year later. The married sister also informed me that she herself had had but one child, which was a delicate infant; it had snuffles and died at the age of 6 weeks (fig. 15). The patient, E. H., has been paralysed in the left side since early infancy. It was discovered only by her not being able to walk or use the hand. When quite an infant she had a rash on the skin, and the eyebrows came out. Later in life it was noticed she was deaf in the left ear. The left arm and leg are wasted, and the bones smaller. She has no contracture. There is a triceps contraction and marked patellar clonus, but no ankle clonus. This was

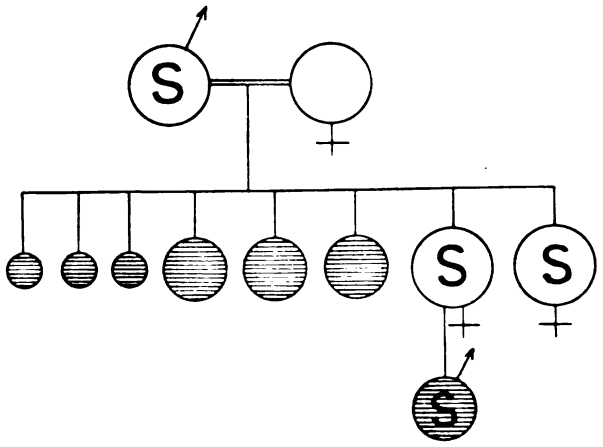


FIG. 15.

undoubtedly a case of congenital syphilitic brain disease causing hemiplegia. The mother came to see me and said that she had never ailed in any way, and I could find no evidence of syphilis on the body. The family history is the specially interesting feature in this case, as showing the effects of acquired syphilis upon the offspring, and also the possibility of transmission to the third generation.

Seeing that Levaditi, Bab, and others have seen spirochætes in the ova, it is possible that the syphilitic contagion may remain in a resting intracellular stage; but when the ovum escapes and is fertilized the syphilitic virus again becomes active, although its virulence is greatly modified and attenuated. This transmission to a third generation is a mere supposition unless we can be absolutely certain that the father was not syphilitic. I could, however, obtain no history of syphilis from the father in the case above recorded. It is no more physically

impossible to admit infection of the segregated germ-cells of the next generation than infection of the sperm-cell. It may be observed that Sir Jonathan Hutchinson is most sceptical of transmission; he says: "Nor have any facts been placed upon record which are worthy of much attention as supporting the belief referred thereto." An excellent critical summary of cases has been given by Dr. G. Ogilvie. In the light of our modern knowledge of syphilis, the spirochæte, or a resting form of it, might have been present in the maternal tissues, and an invasion of the embryo occurred during its development.

When parasyphilis in the form of general paralysis or tabes affects the male, and the wife is not syphilized, the number of living children is not greatly diminished below the normal average. If, however, a married woman is tabetic or paralytic, the reverse is the case. The following cases illustrate some of the points in question:—

G. P., aged 48; occupation, ship's steward. Hard chancre twenty-five years ago, followed by sores on the body and rash. Treated three

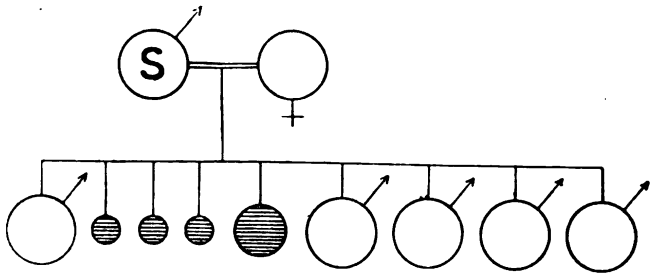


FIG. 16.

months. Married at the time, but away from his wife on a voyage. He acquired syphilis in 1876 and returned home in 1878. His wife, who had had one healthy child, now suffered with a succession of three miscarriages—one in 1878 and two in 1879. In 1880 she became pregnant; child was born dead. A second child was born in 1881 and then followed three more children. All the five living children are grown up and well (fig. 16). The patient, G. P., came under my care for optic atrophy, which came on sixteen years after infection.

B. C., aged 53. A case of tabes with arthropathy and severe gastric crises. She gave the following history: Married at 17; (1) four months after marriage, miscarriage; (2) another miscarriage, six to seven months; (3) a miscarriage, four months (fig. 17). Her husband died of general paralysis, aged 36. She remained a widow four years, married

again, but had no children. She states that she was always in good health till her knee-joint swelled. She was admitted to St. Thomas's Hospital, and owing to the foot beginning to swell, the leg was amputated above the knee.

A. C., aged 57, female with definite syphilitic history, admitted suffering with late arthropathy. Married at 24, no children living, four pregnancies: (1) miscarriage six months after marriage; (2) miscarriage; (3) child born dead; (4) miscarriage (fig. 18). Says she had good health after marriage except for the hair falling out and some fever.

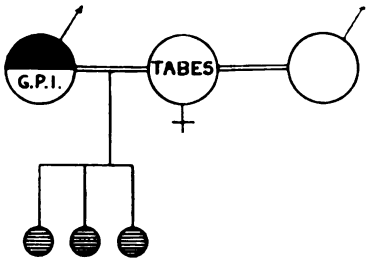


FIG. 17.

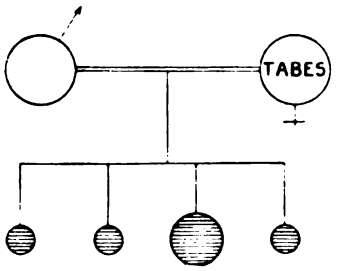


FIG. 18.

E. M., aged 34, widow, charwoman, suffering with tabes. Married at 19, no children, never had any; history of three miscarriages, first being four or five months after marriage (fig. 19). Marks of old syphilitic eruption on the body and a squamous syphilide. Symptoms commenced two years previously with pains in the legs and bladder trouble.

C. A., aged 37, married woman. She was married at 17; an eight months child born lived only two days. She found that her husband suffered with venereal disease. She herself contracted the disease and she had sore throat and her hair fell out; she therefore left him. She married again at 22, had three miscarriages but no living children

(fig. 20). Two years ago the first symptoms of tabes occurred—tight cord round body, rectal crises and bladder troubles, followed by lightning pains.

Many of these "married" women were doubtless unmarried but cohabited with men.

H. W., aged 26, was admitted to Claybury Asylum suffering with early ataxic general paralysis. She is the subject of syphilis acquired before marriage. She was married seven years ago at the age of 19. The husband states that occasionally she has been drunk, but he is sure

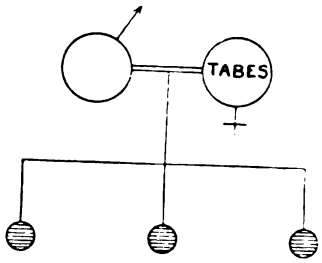


FIG. 19.

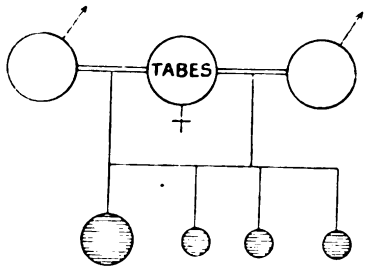


FIG. 20.

her illness was not due to drink. She was an attractive-looking, good and affectionate wife, and came from a very steady, good family. She had suffered with a bad leg, which she always tried to hide from her husband. Scar of a broken-down gumma. The pregnancies are shown in the accompanying figure (fig. 21).

The following cases of two sisters in Hanwell Asylum are of interest as showing the persistence of the Wassermann reaction in congenital syphilis.

L. H. was admitted when aged 26 to Hanwell Asylum in 1889 with congenital syphilis and imbecility. There was evidence of old iritis and depressed bridge of nose. Her sister, A. H., aged 36, was admitted in

1910 to Hanwell Asylum with delusions and aural hallucinations. She has been stone deaf for the past ten years. She has keratitis and iritis. A brother is in an asylum in America. The mother died twenty-seven years ago in Brookwood Asylum of general paralysis.

Twenty-four tabetic women who were married or had cohabited had only 3 living children, 19 born dead and 32 miscarriages. Ten of these 24 were sterile. Now of 54 married male tabetics or tabo-paralytics 151 children were born alive, 75 were born alive and died in infancy, and there were 52 miscarriages or born dead. This shows that a tabetic father does not often infect the wife, and the syphilis from which the male parent suffered has not seriously impaired the number of living offspring.

It is said that women do not nearly so frequently suffer with tabes as men; as in the case of general paralysis there is, however, a correla-

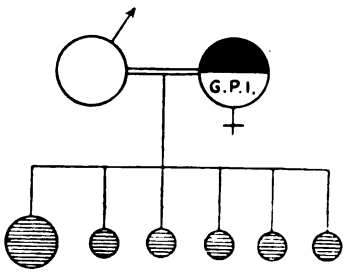


FIG. 21.

A child born dead, followed by five miscarriages.

tion between the frequency of the incidence of syphilis in a class of the population and the incidence of parasyphilitic affections, including tabes. It is said that prostitutes do not suffer with parasyphilis; this is not true. Kron's observations upon 184 public prostitutes showed that 14 per cent. of those syphilitic prostitutes who had reached the age of 25 were tabetic. Although the text-books teach that tabes occurs very much more frequently in men than in women, yet I found on visiting a number of the London infirmaries a large number of women bed-ridden with tabes, and I should estimate that the frequency of occurrence of tabes in women of the poorer classes bore about the same relation as general paralysis—viz., 3 males to 1 female. The reason why such authorities as Erb, who gives 19·5 males to 1 female, and Fournier, 26 males to 1 female, are relatively so high is, I think, due to the fact of

the large private practice they do, so that they draw their data from the upper and middle classes mostly, in which syphilis in women is comparatively rare. Mendel's statistics at his polyclinic in Berlin support this conclusion. Number of patients: 20,539 males, 21,825 females; total, 42,364. Of these there were 725 male tabetics (3·53 per cent.) and 288 female tabetics (1·31 per cent.). There was therefore 1 tabetic woman to 2·7 tabetic men.

Congenital Syphilis and its Effect on the Germ-plasm.

An important question from the racial point of view is this: Does syphilis diminish the vital energy of the germ-plasm prior to conjugation of the male and female germ-cells and cause pathological variations?

It is a known fact that toxins weaken cells, and therefore why not germ-cells? For although segregated in the body, they are of the body and nourished by the same blood and lymph, and there is consequently reason for supposing that the two most potent and prevalent poisons, alcohol and syphilis, may, without killing the germ-cells, diminish their specific vital energy and thus lead to various pathological conditions of the body and especially of the nervous system.

There can be no doubt that syphilis in the parents may lead to infantilism in the offspring, evidenced by arrest of development of the reproductive glands and absence of the secondary sexual characters. A considerable proportion of the male juvenile general paralytics exhibit this manifestation of deficient vital energy of cells without presenting any coarse and obtrusive signs of syphilis. All the cases of juvenile general paralysis and infantilism that I have seen recently give a positive Wassermann reaction.

If syphilis can produce arrest of development of the reproductive organs there is surely no reason why it should not lead to arrest of development of the most highly differentiated and specialized tissues of the body—viz., the cerebral cortex—or cause pathological variations in its structure and functions. Not long ago I was consulted about a pretty, physically well-developed imbecile child. I could ascertain nothing from the family history to account for the condition. Later I saw the father, who was suffering from arrested syphilitic brain disease, and I then ascertained that he had been infected some years before marriage. His wife had had no miscarriages, and there was no evidence to show that she had been infected. This, however, is no proof, I admit, that she had not had latent syphilis.

There is not the slightest question that, if congenital syphilis were not so fatal to infant life, the number of people suffering from brain disease from this cause would be appalling. I have seen but very few cases of syphilitic arteritis, gummata of the brain and meningitis arising from congenital disease as compared with general paralysis, tabo-paralysis, and optic atrophy, pathological conditions which I consider are due to primary neuronic decay. An important question relating to the public health is this: does syphilis contribute to a considerable extent in the production of idiocy? Binswanger, whose statistics have been based upon a large number of idiots, gives 9·5 per cent. as certain and 12·2 per cent. probable of one of the parents. Similar results were obtained by Wildermuth. Ziehen gives 10 per cent. demonstrable, and a further 17 per cent. probable. On the other hand, Bourneville holds that congenital syphilis is an exceeding rare cause of idiocy. Langdon Down found it in only 2 per cent. of cases, and Shuttleworth, among 1,000 idiots at Darenth, only found 1 per cent. of congenital syphilis. Telford Smith found only eight cases with marked evidence of congenital syphilis amongst 580 inmates of the Royal Albert Asylum. Similarly, Brown, in America, only found 1 to 1·5 per cent. of syphilitic origin. An important piece of evidence relating to this question has lately been forthcoming in the examination of the blood serum of idiots by the Wassermann reaction, which tends to confirm the much higher per-centage of the German statistics. I observe, however, that Dr. Dean is taking part in this discussion, and I shall leave him to give his researches in the Serological Institute of Berlin, which certainly support his statement that it is reasonable to think that many cases of idiocy should be classed with that form of syphilis which manifests itself alone by a selective toxic action on the nervous system. It is a pity that no opportunity has been afforded for the examination of the blood of the idiots in the English asylums, for I feel convinced that syphilis plays a more important part in the production of idiocy and imbecility than is apparent from the English statistics I have referred to. Dr. Topley has examined the blood and cerebrospinal fluid of a number of clinically syphilitic or parasyphilitic cases of mine at Charing Cross Hospital. All the cases gave a positive reaction of the blood except one, a case of syphilitic arteritis; after a six weeks' treatment with mercury inunction and iodide a well-marked positive reaction was given. This was paradoxical, but Ehrlich has observed this and termed it "reaction provocative." Another case of ophthalmoplegia externa and interna gave a negative reaction, but the whole of the symptoms cleared up under

treatment. Two cases of juvenile general paralysis—one with infantilism, the other with optic atrophy—both gave a positive reaction of the blood and the cerebrospinal fluid, although there were no obvious syphilitic stigmata. The mothers, when interviewed, said they had never suffered in any way, never had a day's illness; both gave a positive reaction in all dilutions, and this leads me to say that I agree with Neisser as regards Colles's law: the women who suckle their syphilitic offspring and yet do not suffer are like these two women, examples of latent syphilis. The mother is *apparently* immune in Colles's law and the child is apparently immune in Profeta's law. The following facts support this conclusion:—

(1) The researches of Bauer, Engelmann and Rietschl show that all mothers of congenital syphilitic children give a positive Wassermann reaction.

(2) The researches of Knopfelmacher and Lehndorf show that those women who have given birth to congenital syphilitic children within four years react positively in the same percentage as latent syphilis. Consequently, in spite of *apparent* health the mothers of congenital syphilitic children are much more frequently syphilitic than has been supposed.

(3) The passage of reaction bodies to a healthy mother can only be a passive one, and the positive reaction would rapidly disappear from the blood of the mother after birth of the syphilitic child if their source were the syphilitic fœtus, but it may persist, as we have seen, in these two cases for fifteen to twenty years.

(4) Bauer, Wechselmann and Neisser have shown that there may be a positive serum reaction in the mother and a negative of the child.

(5) A series of cases have been published by Halberstädter, Müller and Reichel, in which the child at birth showed a negative reaction, and only gave a positive reaction when there were definite objective signs of syphilis. In these cases we can exclude passage of the reaction bodies from fœtus to the mother.

(6) Neisser's researches upon apes and anthropoid apes afford no support to the occurrence of a true immunity in syphilis.

It is nevertheless a remarkable thing that women with latent syphilis often, as in the case of the two I mentioned, give such a marked Wassermann reaction and have no signs of syphilis. Again, it is remarkable that infants only gave a positive reaction when there were definite objective signs of syphilis.

Many people have syphilis and are never treated at all, and yet

get well; they may show in after-life a well-marked positive reaction. Paralytics give a very marked reaction of the blood and cerebrospinal fluid in nearly 100 per cent. of the cases—97 per cent. at least—and yet the signs of antecedent syphilis are usually slight and often entirely absent. May we not reasonably infer that if the Wassermann reaction is not dependent upon an acquired or inherent defensive immunity reaction, it is in some way closely associated with it?

Of the value of the Wassermann reaction in the diagnosis of general paralysis there cannot be the slightest question. Dr. Candler, my assistant, has published two valuable papers proving this fact. The correctness of the results of his work has been proved by the death and post-mortem examination of a large number of the patients who were examined during life. I show here in tabular form a synopsis of the results obtained by my assistants Dr. Candler and Mr. Mann (pp. 85, 86).

An important distinction of parasyphilis from syphilis of the nervous system is, that syphilis of the nervous system does not give a positive Wassermann reaction of the cerebrospinal fluid except in a comparatively few cases, less than 20 per cent.; whereas general paralysis gives it in nearly every case (97 per cent.) and tabes in about 60 per cent. We may, I think, interpret the marked reaction of the blood and cerebrospinal fluid as evidence of the persistence of the reaction long after the specific organism itself has ceased to be present; for no one has ever discovered the spirochæte in the lesions of general paralysis of the insane. Again, no matter how long after the primary infection (which may be ten to thirty years) the onset of the disease occurs, nevertheless the reaction is intense, in spite of the fact that, generally speaking, the symptoms of infection were mild or even absent. What can this mean but an intense reaction on the part of the body to the specific organism, probably due to an acquired congenital and racial immunity. Schäfer's results show that the time between the primary infection and the onset of the disease is on an average ten years, whether the patient has been treated with mercury or not, which I can confirm; this is a strong support of my contention. Again, we may ask the question whether the metals, arsenic, mercury and antimony, all of which have a specific action in destroying the spirochæte, do not do so by poisoning or weakening the organisms so that they cannot multiply, and thus aid the defensive reactions of the body? But an excessive reaction of the specific organism persisting after energetic treatment with mercury for years suggests an *over-reaction*, a hypersensibility of the cells of the body which will destroy the durability of the complex nerve-cells which

are incapable of regeneration. The probability that the nerve-cells are drawn into this defensive reaction is evidenced by the fact that the cerebrospinal fluid contains the globulin upon which the specific biochemical reaction of Wassermann depends. Normally the brain is protected against nutritional disturbances of the body, causing wasting —e.g., in starvation it hardly loses anything in weight. For this reason I am of opinion that the primary neuronic decay of the nerve-cells in this disease is the outcome of a lack of durability caused by a nutritional disequilibration occasioned by an excessive defensive reaction of the cells of the body in which the nerve-cells participate. If this theory be true, we may expect that those cases of syphilis in which, in spite of all treatment, the reaction persists are more likely to end in general paralysis; particularly if all the additional factors that lead to neurasthenia combine in throwing an extra stress on the maintenance of nutritional equilibrium of the latest and most complex structure in the body, the cerebral cortex. If these premises be accepted as possible, it is to be hoped that the early administration of "606," followed by mercury may in a measure do what mercury alone has in a measure failed to do, as regards averting parasyphilitic affections in later life —viz., it may by rapidly and effectually killing off the spirochætes in the blood and lymph prevent the hypersensitizing of the cells of the body and excessive defensive reaction. It will be of great interest to see how far this hypothesis may be true; at any rate, I expect it will lead to discussion of this important question of latent syphilis and the public health.

I have in conclusion one other proposition to make, and that is the desirability of a Wassermann reaction of all infants born of parents who are syphilitic or who are suspect, whether the infant presents symptoms or not; for we have seen that many die of convulsions, meningitis and hydrocephalus in early life or later develop optic atrophy, deafness, or juvenile general paralysis. Moreover, an estimate of the amount of latent syphilis and its influence in the modification of the effects of syphilis on the race could then be arrived at. The following table gives the results of the Wassermann reaction obtained by Dr. Topley on cases admitted to Charing Cross Hospital under my care recently:—

RESULT OF EXAMINATION OF CEREBROSPINAL FLUIDS.

Case	Diagnosis	Excess of small lympho-cytes	Wasser-mann reaction	CEREBROSPINAL FLUID			
				8 volumes	4 volumes	2 volumes	1 volume
S. H.	Infantile general paralysis of the insane	+	+	+	+	+	+
E. M.	Infantile general paralysis of the insane	Slight	+	+	+	Par	Par
K. C.	General paralysis of the insane	+	+	+	+	+	+
C. H.	General paralysis of the insane	+	+	+	+	Par	Slight
A. W.	General paralysis of the insane	++	+	+	+	Par	–
O. R.	General paralysis of the insane	Slight	+	+	+	Par	–
G. T.	? General paralysis of the insane	–	–	–	–	–	–
R. C.	Syphilitic meningitis	+	–	–	–	–	–
H. H.	Syphilitic meningitis	+	+	+	Slight	–	–
H. H.	Syphilitic meningitis	+	–	–	–	–	–
J. H.	? Syphilitic meningitis... ...	+	+	+	+	–	–
*C. D.	Syphilitic meningo-myelitis ...	+	+	+	+	+	+
J. W.	Syphilitic hemiplegia	+	–	–	–	–	–
†S. W.	Congenital syphilis	+	–	–	–	–	–

* Marked positive result in spite of two doses of "606," followed by mercurial inunction.

† S. W. was the son of A. W. He presented no signs of syphilis, but gave the marked reaction in the blood. It is possible that he may develop general paralysis in later life.

SUMMARY OF RESULTS OF WASSERMANN'S REACTION IN ASYLUM CASES.

Since March 1, 1911, 256 specimens of cerebrospinal fluid (sent from the various London County Asylums for diagnosis) have been submitted to the Wassermann test by Dr. Candler and Mr. Mann. Of these 164 gave a positive result. A number of the patients have died, and including a number of cases that were examined in a previous series of Wassermann tests by Dr. Candler and Dr. Henderson Smith that have also come to autopsy, out of 119 cases we have been able to confirm the result of the Wassermann reaction by autopsy

and microscopical investigation in 116 (or 97·4 per cent.), as shown by the following table:—

CEREBROSPINAL FLUIDS.

		Cases
Positive results confirmed by autopsy and microscopical examination	...	100
Negative ,, ,, ,, ,, ,,	...	17
Negative results on cases found to be general paralysis at autopsy	...	2
Total	..	119

Wassermann's reaction confirmed in 116 (97·4 per cent.) out of 119 cases. No case giving a positive result was found to be other than general paralysis at autopsy. In general paralysis, 100 of 102 cases (98 per cent.) gave a positive reaction.

The Royal Society of Medicine.

June 17, 1912.

Sir HENRY MORRIS, Bt., President, in the Chair.

A Discussion on Syphilis, with special reference to (*a*) its Prevalence and Intensity in the Past and at the Present Day; (*b*) its Relation to Public Health, including Congenital Syphilis; (*c*) the Treatment of the Disease.[1]

Mr. ERNEST LANE: In continuing the discussion opened last Monday I do not propose to deal with the prevalence of syphilis in the past, or its relation to public health, but will confine my few remarks to the treatment of the disease. I expressed my views on the treatment of syphilis at a discussion held by the Surgical Section of this Society in 1910,[2] and I also opened a similar discussion at the Annual Meeting of the British Medical Association last year, since which time my opinions have undergone no material change, and before a meeting such as this I would not venture to go over any ground I had so recently traversed. At my former communication to the Surgical Section of this Society in 1910 I gave my experiences of various arsenical compounds known as the arylarsonates, and expressed my opinion that, though their influence on the symptoms of syphilis was most remarkable, yet it was impossible to advocate the continuance of such treatment owing to the idiosyncrasy shown by certain patients against these preparations. I brought forward a certain number of cases in which double optic atrophy and incurable blindness had resulted from this plan of treatment, and consequently I abandoned it as being dangerous and unjustifiable. Still, the results of the treatment were most remarkable, and many patients left the hospital after ten injections administered once a week, entirely free from all symptoms of disease, and did not present themselves again, notwithstanding instructions to attend promptly in case of any relapse. It will

[1] Second meeting (adjourned from June 10).

[2] *Proceedings*, 1910, iii (Surg. Sect.), pp. 209-48.

thus be seen that I fully realized three years ago that the introduction of large doses of arsenic into the system produced a very marked effect on the symptoms of syphilis, and the treatment by arylarsonates was also, at that time, strongly advocated by Hallopeau and others, and was employed largely in this country at the Rochester Row Hospital by officers of the Royal Army Medical Corps. As I pointed out in that discussion, the insurmountable objection to these arsenical compounds was that they not only deprived the patient of his manifestations of syphilis but occasionally also of his eyesight at the same time. Last year, at the Annual Meeting of the British Medical Association,[1] I expounded my views on the treatment of syphilis with special reference to salvarsan, and as less than a year has elapsed since I read that paper I fear that I have not much to add to what I then said. The gist of my communication was that salvarsan was a most valuable addition to our category of anti-syphilitic remedies, but that it was only an addition, and that its influence on the disease was hardly as striking as had been alleged. I recognized then, as I do now, its extraordinary effect on the symptoms of the disease, but I expressed the opinion then, as I do now, that salvarsan, *per se*, was not the cure that it was represented to be. I have been exposed to considerable criticism as to my early attitude to this treatment, but I see no reason to regret the line I took, which was that its merits were somewhat over-estimated and that the claims originally made on its behalf were not justified.

Having seen many innovations both in medicine and surgery, and having found from extending experience that the claims made for new methods of treatment do not always come up to the sample, so to speak, I moderated the transports of my enthusiasm, and proceeded to give the treatment an impartial trial. At first we were told that syphilis was going to be cured by one injection of salvarsan, and the title of "Therapia sterilans magna" was bestowed upon it. It certainly, as anyone who has employed it must have realized, caused the disappearance of the early symptoms of syphilis with wonderful rapidity, and might have created the impression that it *had* effected a cure. But more mature experience showed that the disappearance of symptoms was only temporary, and relapses were continually met with. Then the number of injections was increased, and two, three, or more, were advocated at short intervals, and now I find cases recorded in which as many as nine injections have been administered in the space of

[1] *Brit. Med. Journ.*, 1911, ii, pp. 673-76.

twelve weeks (a somewhat expensive course for a hospital patient). I consider, therefore, that I was fully justified in criticizing the term "Therapia sterilans magna," but the best evidence that I have not assumed an attitude of uncompromising hostility to salvarsan is the fact that I have treated over 300 cases by this method in hospital and in private practice, and am consequently entitled to speak of it with some degree of authority. Not that I attach so much importance to the number of cases treated in this manner, for without a matured experience in other methods of treatment a fair comparison cannot be arrived at. Like Mr. D'Arcy Power, I do not rely on salvarsan solely, but propose to follow it up with chronic intermittent mercurial treatment for two years or more, according to the blood test. To the devoted disciples of Ehrlich the combination of salvarsan and mercury does not commend itself, and some of them go so far as to cast doubt upon the curative powers of mercury over syphilis. They concede that it abolished symptoms of syphilis, and rendered the disease latent, but there the concessions stopped, so if this is the case we are to conclude that no case of syphilis has ever in the past been cured by mercury. From a somewhat extended experience in the mercurial treatment of syphilis I have no hesitation in saying that it *is* a cure for syphilis if administered for a sufficiently long time, and the reason that cases of tertiary syphilis and of parasyphilis are so frequently met with is that the period over which treatment has been prolonged has been obviously insufficient. I think the doctrine formerly laid down, that mercurial treatment continued for only two years was sufficient to effect a cure, a most pernicious one, and for years past I advocated its continuance for the space of five or even seven years. The duration of treatment will now materially depend upon the Wassermann test, and such long periods as those indicated above will not be necessary in the majority of cases. But when first I drew attention to the inadequacy of the two years' limit the Wassermann test had not been invented, and the prolongation of the treatment for five years or more was only with the view of insuring the patient, if possible, against any of the late manifestations of the disease.

Considering that the salvarsan treatment was only initiated in 1909, and that the method of its administration has been considerably modified since that date, I maintain that no one at the present time is justified in the belief that it is a cure for such a complex chronic disease as syphilis. Until recently a negative Wassermann test was held by many as a proof that the disease was cured, but now in the

presence of a negative Wassermann reaction, what are styled provocative injections of salvarsan are administered, the effect of which is often to render the reaction once more positive and to indicate that further treatment is required. I find it further stated by one of the most enthusiastic advocates of salvarsan that in the late stages of the disease the drug should be used only with the object of eradicating symptoms, and that a cure is only to be expected in those cases in which the disease is attacked in its infancy. This is rather a climb down from the position originally taken up. I see no positive proof that arsenic is any more destructive to the spirochæte than is mercury, and am curious to know whether intravenous injections of mercury would not be found to have an equally damaging effect upon the spirochæte. Sixteen years ago I gave an account of a number of cases of syphilis treated by intravenous injections of mercurial solutions, and from the rapid disappearance of symptoms in these cases, I was led to conclude that the effect of the treatment on the spirochæte was considerably to modify the powers for evil of that organism.

I am of the opinion that salvarsan should not be employed until the surgeon has made himself fully conversant with the technique, and agree with Mr. Power that the intravenous injection of salvarsan is a safe method of procedure *if care be taken to administer it in a rational manner* and to reasonably healthy persons. But I should hesitate in recommending it as a routine treatment, and do not consider that anyone should undertake it until he has had the opportunity of seeing the injection administered frequently by one who has acquired the requisite experience. The slightest mistake in technique may be fraught with the greatest danger to the patient, and might lead the surgeon to regret that he had ever employed it. The simpler the apparatus employed for the intravenous injections the better, and all that is required is a funnel, some rubber tubing with a glass window, a clip, and a needle. There is a contingency to be guarded against, and that is passage of the needle through the distal wall of the vein, and the consequent escape of the solution into the subcutaneous tissues. This happened to me once from the patient moving his arm, and the consequence was a solid œdema of the whole arm which persisted for a fortnight. I should therefore hesitate before handing over the sole care of the needle to the patient, as suggested by Mr. D'Arcy Power, but think it absolutely essential that the needle shall be held in place by the surgeon until the completion of the injection.

I have had two fatal cases at the Lock Hospital, but in neither of

these could I say that death was *not* due to the severity of the disease. In one case the injection of 0·5 grm. was shortly followed by vomiting and extreme collapse, so much so that an immediate fatal result was feared; however, after some hours the patient rallied, but two days later intense jaundice supervened, and death occurred eight days after the injection. It is well known that jaundice does occasionally supervene shortly after the administration of the drug, and it is quite possible that in this case it was an evidence of acute arsenical poisoning.

I also had my attention drawn to another fatal case which did not occur in my own practice, and which can only be characterized as a terrible tragedy. I have mentioned this case in a previous communication, and will give the merest outline of it to this meeting. The subject was a perfectly healthy man, who had contracted the disease two years previously and had been treated with mercury. He was desirous of getting married, but as his Wassermann reaction was positive, he was advised to have an intravenous injection of salvarsan. The injection of 0·6 grm. was followed by slight nausea, and subsequently by pains in the chest, difficulty of breathing, and cyanosis, and he died in the middle of the night, presumably of pulmonary embolism.

When first I adopted this treatment I encountered a fair proportion of cases in which the reaction following intravenous injections was excessive and alarming; in the light of present knowledge I attribute these disturbing symptoms to the fact that I then dissolved the powder in saline solution, whereas now I make use of redistilled water. Through the kindness of my colleague Mr. McDonagh, who supplied me with the tubes, I have had the opportunity of treating seventeen hospital cases with the new modification known as neo-salvarsan. This has the advantage of being perfectly soluble in warm water, requires the addition of no reagent; and appears to be followed by less reaction than the original preparation. In one case some somewhat disturbing symptoms followed this treatment; the case was that of a healthy young man, aged 21, suffering from recently acquired syphilis, the chancre being present and also a slight papular eruption; an injection of 0·6 grm. of neo-salvarsan was given, which was followed by some slight reaction; four days later he was given a second injection of 0·5 grm., which caused no reaction, and five days later he was given a third injection of 0·4 grm.; this was followed by a rise of temperature which for the next three days ranged from 101° to 103° F., accompanied by drowsiness. Five days after the third injection he became distinctly jaundiced, the stools became white in colour, the urine was

markedly bile-stained, and considerable itching of the skin was present. These symptoms had all disappeared by the end of a fortnight, at which time he left the hospital quite free from symptoms. This was not in my opinion a case of jaundice due to syphilis, for though it is a condition which is met with in early stages of the disease, it is not so intense as in this case, and is more transitory in nature. I am rather inclined to ascribe it to the toxic effect of large doses of arsenic upon the hepatic cells. Before quitting this method some allusion must be made to the preparation "joha," which is a recent method of introducing salvarsan intramuscularly: 1½ c.c. of joha are equivalent to 0·6 grm. of salvarsan. Its advantages are that from its limited volume it is less likely to be followed by the extensive necrosis which was occasionally observed to follow intramuscular injections of salvarsan; it is said not to be followed by pain or any toxic symptoms, it does not deteriorate by keeping, and the technique is perfectly simple. My experience of this substance is limited to six cases, in all of which the injections were followed by a rapid amelioration of the syphilitic symptoms, though in one case the patient recorded that following injections into each buttock he was confined to his bed for three weeks with excruciating pains down both legs.

I have dealt at some length with the salvarsan treatment, but as I stated previously, I am not content to rely upon that treatment alone, but follow it up with that form of mercurial treatment which is most suitable to the individual case. A plan of campaign of this nature has the approval of that most distinguished syphilographer Professor Neisser, who in his Cavendish Lecture last year expressed his opinion as follows: "It appears to me beyond all doubt that a surer, more brilliant and more lasting curative effect will follow if we combine the two remedies." I have for years past advocated intramuscular injections of mercurial preparations as the best means of exhibiting that drug, and consider that the insoluble salts are infinitely superior to the soluble, and that of the insoluble preparations the palm must be given to calomel suspended in olive oil.

Mr. JONATHAN HUTCHINSON: With regard to the treatment of syphilis by mercury there has been the greatest controversy as to the best method of administration. Some observers declare that intramuscular injection is superior to all other methods, others pin their faith to inunction, both classes decry the internal use of mercury in the form of grey powder, &c., as being inefficient. In this country, largely

owing to my father's teaching, the vast majority of cases have been submitted to the third or intra-oral method. Now what view was held by its advocates before the introduction of the Wassermann test, which has come as a searchlight on the whole subject? Briefly it was this, the mercurial course should be treated seriously by patient and doctor alike, it should be commenced directly the diagnosis could be made (if possible before the secondary symptoms developed), and it should be continued steadily for about two years. In most cases, provided the patient was abstemious and led a regular and healthy life, the mercury caused little or no inconvenience, and while it was taken it prevented the occurrence of any symptoms (there was a fair proportion of exceptions to this—especially with regard to superficial ulceration of the tongue and throat). Wherever " reminders " of any kind occurred the period of treatment was prolonged, or the form of mercurial varied. Our belief was, that under these conditions a real cure of syphilis was obtained in something like 80 per cent. of the cases. We admitted that in a certain number, even of the most careful patients, this treatment failed to prevent later tertiary symptoms, sometimes of a grave nature, though it is a matter of universal experience that in many of the cases of tabes, &c., the treatment of the precedent syphilis has been obviously inadequate and has fallen far short of the requirements laid down above. But I think it has been generally admitted that in a certain number of cases the syphilitic virus was not destroyed by the internal use of mercury, however prolonged. This is equally true of mercurial inunction and injections, and personally I have never been able to convince myself that there was any decided superiority in either method. It may here be noted that my father and I have published two independent series of cases in which the patients contracted syphilis a second time after the first attack had been treated by a long and steady course of mercury given by the mouth, pointing clearly to a cure having been attained.

Since the invaluable blood test was introduced by Professor Wassermann, a great many patients who had undergone in former years what was then considered to be adequate mercurial treatment (given by the mouth) have been submitted to the test. My own cases have been tested almost entirely by Dr. Fildes, of the London Hospital Medical College. In the great majority of cases the result has been satisfactory, the test being absolutely negative. In some, either those in which the test was faintly positive or distinctly so, I have advised the intravenous administration of salvarsan; in most of these the injections have been

successful in securing a negative result to the test, but in one or two cases even two full doses of salvarsan have failed. I prefer to quote on this subject the results obtained from hospital patients, by Dr. McIntosh,[1] since it is for obvious reasons more easy to carry out the test repeatedly on them, and because Dr. McIntosh's investigation was a wholly independent and unbiased one of 160 patients treated with mercurial pills alone. "Examination during the first year gave 92 per cent. of cases positive. Cases examined at the end of the second year gave 54 per cent. of positive results, while cases treated for over two years reacted positively in 30 per cent." Thus, 70 per cent. were ultimately cured if we accept the evidence of the Wassermann reaction. How does this compare with the results of mercurial injections and inunction? Major Harrison has demonstrated that after seven courses of injection with "Lambkin's mercurial cream" a positive Wassermann reaction was still obtained in no less than 55 per cent., and that of 111 cases treated by various methods 64 were still "positive." The evidence of Continental observers is very much the same; Jesionek and Meirousky found only 60 per cent. of cases with negative Wassermann test after from four to eight courses of mercury, Seligmann and Pinkus only 55 per cent. of cures after four to ten courses. Hence, we may conclude with McIntosh, Pürckhauer and Boas that the exact form in which mercury is administered makes little difference, that "the therapeutic effect is practically independent of the form of administration." Mr. D'Arcy Power, I think, arrived at the same conclusion in his opening address, although he expressed a preference for the use of injections. Mr. Power quoted, apparently with approval, one of the many elaborate systems of treatment with varying numbers of injections interspersed with oscillating intervals. I would urge that there is not the smallest scientific reason to be adduced in their favour. The important point is, when mercury alone is relied upon, to keep the patient under its influence steadily through a long period of time—not less than two years—and the simpler the method of giving it the better.

But Professor Ehrlich's discovery has entirely changed our views, and I cannot agree with Mr. Power that "in the light of our present knowledge salvarsan should not be used by itself, or in the place of mercury." Salvarsan is the only remedy that will speedily render the Wassermann reaction negative, and there is abundant evidence that after two intravenous injections the reaction remains negative in most

[1] McIntosh and Fildes, "Syphilis from the Modern Standpoint," 1911, p. 157.

cases. We may then presume that a complete cure has been effected. Granted that mercury achieves the same end if properly administered in about 70 per cent. to 80 per cent., nothing is more certain than that it takes a long time to do so, that whatever form is employed the Wassermann reaction will remain positive for many months. When one thinks of the terrible possibilities of syphilis, of tabes, general paralysis, &c., it seems obvious that we are bound to employ the most rapid and efficient method of destroying the poison, and to use it at the earliest possible moment. I see that Dr. Mott holds precisely the same view. I maintain, therefore, that in every case of early syphilis we are bound to advise the patient to undergo two intravenous injections of salvarsan, whether they be followed by a course of mercury or not. Dr. Fildes and Dr. McIntosh, from whose valuable work I have extensively quoted, are in favour of relying on salvarsan alone, of course with the subsequent confirmation of the Wassermann test.

My own experience of salvarsan in late syphilis agrees entirely with that of Mr. D'Arcy Power. In cases which have resisted mercury and iodides it has generally proved successful. To the surgeon in past years none have given more trouble than the cases of relapsing glossitis and ulcers of the mucous membrane of the lips, palate, &c. Mercury and iodides may here have ameliorated the condition, or have become quite powerless. In fact, for many of these cases one has ceased to trust to these remedies and relied on local treatment—i.e., some form of cauterization, combined with the removal of all sense of irritation. And yet all these patients give a positive Wassermann reaction, though ten or twelve years may have elapsed since infection, and all kinds of "specific treatment" have been tried. I have seen really brilliant results again and again in such cases from salvarsan injection; often a single injection has completely cured the condition. It should be noted that against true leukoplakia salvarsan fails as all other remedies do. I have long held that whilst leukoplakia may occur in those who have had syphilis the latter has no direct causal relation; the keratosis of the mucous membrane is set up by the irritation of tobacco, and should not even be placed in the vague category of parasyphilitic lesions, since it often occurs in those who have never acquired the disease.

In cases of severe bone-pains and nodes in the early stage of syphilis, salvarsan is as a rule remarkably successful; I have known persistent and intense cephalalgia in the secondary stage disappear like magic after the first injection. In nearly all tertiary lesions, if active or resistant to iodides, &c., salvarsan should be tried and will often cure. It is well

known that congenital cases are, speaking generally, more resistant to salvarsan than the acquired ones. In some, indeed, it appears almost impossible to obtain a negative Wassermann reaction. I am afraid that in interstitial keratitis and deafness due to congenital syphilis the results have been very disappointing, but I cannot speak from much personal experience in this matter.

When the surgeon had only mercury to rely upon there were two main classes of case which resisted his best endeavours, first the relapsing lesions of the tongue and mouth in acquired syphilis, secondly the interstitial keratitis and other symptoms of the inherited disease in young adolescents. So far as my present experience goes salvarsan has enabled the surgeon completely to master the former group of cases (and for this we are profoundly grateful to Professor Ehrlich), whilst unfortunately the second group retains its intractability untouched. It will be noted that Mr. D'Arcy Power records the comparative failure of salvarsan in the treatment of arthritis due to inherited syphilis, a condition often met with at the same period of life as interstitial keratitis. My own experience has been that this form of syphilitic joint disease yields quickly enough to internal treatment with mercury, but it is a very different thing with regard to interstitial keratitis. Here a rapid cure—i.e., in a few months' time—is occasionally obtained, but it is not uncommon for one eye to be attacked whilst the patient is under mercurial treatment for keratitis in the other, thus proving the obstinacy of the disease. In the treatment of what is termed malignant precocious syphilis salvarsan is a great addition to our resources, indeed I think it bids fair to render the term obsolete.

Dr. Norman Moore has dealt gently with our cherished views—or illusions—with regard to the absence of syphilis from Europe before the end of the fifteenth century, and its introduction by Columbus's sailors and others, between 1490 and 1500. We still refer with a certain amount, perhaps, of conceit, to the Old World and the New. With regard to the prevalence of syphilis it seems certain that no distinction can be drawn. In Haiti, in Mexico, in Fiji, in India and China, it appears to be of equally ancient origin—from what we should term prehistoric times. The mercurial treatment of syphilis was familiar in India in the tenth century A.D. That syphilis was unknown in Italy and England before 1490 is shown by the great writers and dramatists of these countries. Shakespeare and Ben Jonson, for example, refer to it frequently, though I venture to suggest that there is no proof that Shakespeare meant syphilis in all the twenty-one references to

"the pox" which Sir Henry Morris has quoted. I would have limited the clear references to syphilis in Shakespeare to about half-a-dozen, but these include the wonderful description of the tertiary symptoms in "Timon of Athens," a description which has no parallel in English poetry until we come to Swinburne's "Dolores" of three centuries later. But whilst Shakespeare and Ben Jonson describe syphilis so well, I believe there is not a single passage in the whole of "Chaucer" and "Boccaccio" which can be strained into an allusion to it. The inference is not to be resisted, there was no syphilis amongst Boccaccio's and Chaucer's countrymen. One of Shakespeare's best allusions to syphilis escaped our President's notice, and I believe is little known; it occurs in the pathetic last speech of the immortal Pistol:—

> "Doth fortune play the housewife with me now?
> News have I, that my Nell is dead i' the spital of malady of France."

And here arises an interesting problem with regard to Shakespeare himself. "Timon of Athens," in which we find such an accurate and poignant picture of the effects of syphilis, was a late play (1608), as was "King Lear," in which occurs the terrible description of the female sexual organs (Act iv, Scene 6, lines 130 *et sqq.*). Both plays seem written by a bitter misogynist. We know that Shakespeare before he wrote them passed through a period of grave mental and probably physical trial; we know also from his own statements in the Sonnets that some scandal was attached to his name. No one has offered a satisfactory explanation of these facts. I venture to suggest that it may be found in Shakespeare himself having suffered from syphilis. The poet often stayed at the Crown Hotel in Oxford; here in March 3, 1605, he stood godfather to the future Sir William Davenant, the son of the innkeeper's wife. It was rumoured that Shakespeare was really the father of Sir William Davenant, who used in later life to boast of this parentage. It is a strange fact that the portraits of Sir William Davenant are strongly suggestive of inherited syphilis in their physiognomy.

Mr. J. E. R. McDonagh: The problem of venereal disease is one of the most, if not the most, important which besets every civilized nation at the present time. The greater part of the general public know little or nothing about this complaint, and therefore care the less, while a few who do know and do care are prevented from airing their opinions *pro bono publico*, for fear of the popular prejudice aroused thereby. The reason of this is twofold.

First, because any venereal affection is looked upon as a punishment for sin and not as a disease; secondly, because the nation's great affairs are managed by persons least suited for the purpose, and because the axiom, "What was good enough for our forefathers is good enough for us," is still maintained with reverence. In no country in the world is scientific progress so hampered as in ours. If any members brought up the subject of venereal diseases for discussion in Parliament it would be probably sufficient to prevent them retaining their seats at the next election. In this country some terrible accident must happen before the authorities responsible can take measures to prevent a recurrence which ought to have been taken before. The sound rule that "Prevention is better than cure" is more often preached than practised. If a man is killed by a train the news is on all posters within a few hours. If thousands die of lingering diseases caused by syphilis no notice is taken. Again, recently a few deaths have been caused by anterior poliomyelitis. Quite rightly there have been numerous meetings to prevent further cases occurring, and the Metropolitan Asylums Board has made the disease notifiable. Hundreds and thousands have died as the result of syphilis, and yet not a hand has been lifted up or a voice raised to stamp out the scourge.

Since prevention is better than cure, on what lines should progress run in trying to combat venereal disease? The following would be useful:—

(1) To teach all boys the dangers accruing to extra-matrimonial intercourse, and that abstinence therefrom does in no way endanger their constitution or detract from their manliness—on the contrary, that it has just the opposite effect.

(2) To advocate temperance and healthy sport.

There can be no doubt that the diminution in recent years of venereal disease in the Army is not due to having abolished the C.D. Act, but to the diminished consumption of alcohol, and to the institution of giving lectures to the soldiers. One would not be far wrong in stating that about 90 per cent. of infections take place while the victims are under the influence of alcohol. The same probably applies to the civil population, but we have no means by which we can positively assert as to whether syphilis is on the increase or decrease.

The problem of prostitution has baffled the minds of men for centuries, and while glancing over their work one cannot but fail

to be impressed with the utter failure which has resulted from the regulations apportioned to that class. State control and medical supervision of prostitutes has in no wise diminished venereal disease, since it has stimulated clandestine prostitution which avoids regulation, and is a potent factor in the incidence of infection. Apart from legalizing a trade which is itself immoral, it is, at the same time, grossly unfair and useless to have laws affecting women only and not men, the latter of whom must have been the prime cause of disease in the former. In this country regulation of prostitution died a quick death, and in France and Germany, where the laws have always been most rigid, the system is rapidly on the wane. If laws of any kind are to be framed they must apply equally to both women and men.

Considering how widespread venereal disease is, and what a danger everyone runs, it must appeal to all sane people that our chief duty is to those who are free and then to those who are afflicted. Saving the free can most easily be done by rendering the affected as free from infection as soon as possible. This means early recognition and immediate treatment of the disease with the best means at hand.

Early recognition of a disease requires a sound knowledge of that disease, and so far as venereal diseases are concerned, we must all admit that the knowledge thereof in our profession is very scanty. The medical man cannot be blamed, because venereal diseases are not taught in the general hospitals, and but few beds are set aside for the purpose. Every general hospital should have its own venereal ward, and systemic courses of instruction should be given by men who devote themselves to that study.

Another step forward which strongly appeals to me is to make venereal diseases notifiable on these lines: Have a central bureau made up of venereal specialists and let every case be reported to headquarters by the medical practitioner in charge, giving all details of the case, without mentioning names. The medical practitioner would then be advised as regards treatment, and facilities made in necessitous cases to have the treatment carried out and gauged by the Wassermann reaction. Just one other point concerning regulation. Under no circumstances is the opinion of a chemist or quack more often sought than in those brought about by venereal diseases. Only too frequently a patient, told by a chemist that his sore is only due to a strain, seeks medical advice when he is well in the secondary stage of syphilis. During the interval he has been a source of infection to others and has greatly lessened his own chance of getting cured. Any medical advice given by

a chemist or unqualified practitioner should be treated as a criminal act, and the offender fined or imprisoned.

The main reason why the time is now so ripe to try to lessen syphilis is because the methods of diagnosis are as perfect as possible, and because we have in salvarsan and neo-salvarsan remedies which, by means of a single injection, render a patient within a few hours non-infectious. As it is during the primary and secondary stages that syphilis is spread, and as the victims are usually young, one seldom meets with a case in which salvarsan is contra-indicated. As mercury takes some time to render a patient non-infectious, this alone compels us to use the arsenical preparations, and renders them superior to mercury.

The use of salvarsan requires skill and precision, the lack of which has led to many erroneous ideas as to its safety. It is a known fact that the loudest decriers of salvarsan in this country have been those who have never given an injection. It is so essentially British to say much on a subject a knowledge of which is non-existent. Salvarsan can be injected either into a muscle or into a vein, the latter for preference, the reason being that a second injection can be given after a few days' interval, which is not the case when intramuscular injections are employed, because it is impossible to estimate how much has been absorbed from the first injection. The whole secret of the intravenous injection is invariably to employ only distilled water which has been redistilled a few hours before the operation, as by so doing the unpleasant symptoms—such as vomiting, rigors, headache, &c.—which used to follow each injection can be wholly avoided; so that it is perfectly safe to give 0·5 grm. of salvarsan dissolved in 200 c.c. of saline at weekly intervals for the first three injections, and at ten to fourteen days' interval for each subsequent one. As each case varies, which makes it impossible to say beforehand how many injections a patient must have to cure him, the number must be regulated by the result of the Wassermann test, which should be carried out at frequent intervals after each injection. Broadly speaking, the average case requires between 2 grm. to 4 grm.—that is, four to eight injections.

Neo-salvarsan, which has now superseded salvarsan, is a condensation product of the latter with formaldehyde sulphoxalate of sodium, which gives rise to a powder which is easily soluble in water and is neutral. The after-symptoms following neo-salvarsan are *nil;* much larger doses can be given and at much quicker intervals, as the drug is more rapidly excreted and less toxic than salvarsan. The course I now follow is to

give either one injection or two injections weekly. I always use as big doses as possible—namely, 0·9 grm. to 1·5 grm.—and am guided as to how many injections to give by repeated examinations of the blood at specified intervals.

The regulation of treatment by means of the Wassermann reaction is of paramount importance; I have done some hundreds of tests since last October, with a view of finding out some rules to go upon, and the following is a résumé of my results, which I published in the *British Medical Journal* :—[1]

Cases with a primary sore and which give a negative Wassermann reaction will give a positive reaction after an injection of salvarsan, which is in the majority of cases most marked about the forty-eighth hour. If only one injection is given, by two months the reaction is negative again, to become positive later when symptoms reappear. Should a second injection be given while the reaction is negative, a positive is produced within forty-eight hours, and as a positive reaction cannot be produced in a patient who has not had syphilis, the occurrence of such indicates that the patient has not been cured. Therefore the injections should be repeated with as short intervals as possible until the reaction is negative, forty-eight hours, the seventh, fourteenth, twenty-first, and twenty-eighth days, after the last injection. In the primary and secondary stages this can be achieved in nearly all cases with from two to nine injections. In tertiary cases it is by no means always possible even with nine or ten injections to obtain a negative reaction, therefore one must be guided entirely by the case in deciding whether a tertiary syphilitic shall be advised to have salvarsan with a chance of a cure, or whether it should be given to abolish symptoms only.

One very important point I have ascertained is that it is the rule for a patient who has had syphilis and has been so far treated with mercury as to have been driven into the latent stage with a negative Wassermann reaction, to give a strong positive reaction after one or two provocative injections of salvarsan, and require four to nine injections before a permanent negative reaction could be obtained. So constant is this that salvarsan can be used as a test of a cure. It shows also that mercury never cured a case of syphilis. The cases which required two provocative injections were cases of arterial syphilis. Cerebrospinal syphilis, syphilitic epilepsy, hemiplegia, &c., usually have very little reagin circulating in the blood, therefore no reliance can be

[1] *Brit. Med. Journ.*, 1912, i, p. 1287.

placed on a negative Wassermann reaction for diagnosis, an examination of the cerebrospinal fluid being far more important. From all this you will see that it is useless to give one or two injections of salvarsan and then test the blood after an indefinite period. If the reaction is negative, it only means that you have driven your patient into the latent stage, from which he may at any future time emerge into the active again.

It is known to all that a woman can give birth to a syphilitic infant without having herself shown any signs of the disease. Once a woman has given birth to a syphilitic infant she is always liable to bear syphilitic children, whether the father is syphilitic or not. Such women not infrequently develop their first syphilitic symptoms as tertiary manifestations at or about the menopause. I showed some cases before this Society two years ago.[1] During the child-bearing period the Wassermann reaction may be negative, even if the mother has just begot an undoubted syphilitic infant which gives a positive reaction. In my experience not more than 70 per cent. of mothers of syphilitic children give a positive Wassermann reaction, therefore the researches of Bauer, Engelmann, and Reitschl, to which Dr. Mott has referred, are not confirmed. If a provocative injection of salvarsan is given to such women a positive reaction afterwards is the rule; therefore, it is important to treat a suspected mother thoroughly.

Congenital syphilis is diagnosed more often than it exists, and the opinion prevails that it is not very common, but the fact is overlooked that an enormous percentage of those that would be syphilitic die before birth, and a very large percentage of the remainder soon after birth.

Before concluding my remarks I would like to mention two points:—

(1) That no two cases of syphilis behave alike to treatment. Of two individuals in exactly the same stage with exactly the same symptoms, one may require four injections to cure him, the other nine.

(2) If salvarsan or neo-salvarsan is prescribed with the object of curing the disease, it is imperative to give the injections as soon after one another as possible, and not to stop until the Wassermann reaction is negative, and to employ as large doses as possible. Because if two injections are given now, and two some months later, it will then be necessary to give continuously just as many as would have been required had the sporadic injections not been given; and also the risk is run of manufacturing an arsenic-resistant breed of spirochæte.

[1] *Proceedings*, 1910, iii (Derm. Sect.), p. 90.

In conclusion, let me state that, having given over 200 injections of neo-salvarsan I find that it is in every way preferable to salvarsan, and its use should be augmented with intramuscular injections of mercury until the Wassermann reaction is negative, forty-eight hours, the seventh, fourteenth, twenty-first, and twenty-eighth days after stopping treatment; also, for the sake of safety, a provocative injection should be given six months or a year later, and the blood tested again on the above-mentioned occasions.

May I now be permitted to draw attention to some remarks made by Dr. Mott and Mr. D'Arcy Power? Dr. Mott says that one attack of gonorrhœa does not give an immunity the same as syphilis. There is no evidence that there is such a thing as syphilitic immunity, either to one's own original or a foreign virus, and the reason why so few cases of re-infection are seen is because so few cases are really cured—i.e., they are syphilitic and so cannot be re-infected. This is likewise the reason why Colles's and Profeta's laws stand; both the mother and the child are immune, because they are syphilitic. If a patient is cured he can contract syphilis again, and further, if all the spirochætes in his body are destroyed except a few in the site of the chancre which are unreached by drugs owing to the non-vascularity of the dense fibrous tissue, these may later wake up and give rise to fresh and general infection. I reported such a case in my book "Salvarsan in Syphilis and Allied Diseases," p. 73. For this reason I have always advocated excision of the primary sore.

Mr. D'Arcy Power referred to the beneficial use of mercurial injections in malignant syphilis. Many people's experience of mercury in cases of malignant syphilis is, that the more mercury they give the worse the patient gets, and I think this is true; I have seen several cases descend almost to death's door in spite of the most varied and vigorous mercurial treatment, which were only saved by a course of Zittmann, a treatment which has unfortunately fallen into disuse. In no instance is salvarsan more urgently called for than in cases of malignant syphilis; small and frequent doses should be employed, preceded, if possible, by a course of Zittmann.

Mercurial injections are most suitable in private practice, because the patient cannot afford the time to undergo inunctions and is too frequently upset by taking mercury internally. Internal administration of mercury undoubtedly has a far greater depressing influence than when given in any other form, and in the majority of cases the depressing action of mercury is a factor which requires great consideration.

In my experience the best mercurial cream is the metallic mercury cream of Captain Adams; it is painless, and as much as 3 gr. of mercury can be given weekly. Calomel is excellent, but pain may be intense, however careful one is.

One other point I should like to refer to. Mr. D'Arcy Power maintains that the salvarsan solution should be kept at a temperature of 105° F. In the case of salvarsan it does not so much matter, but in the case of neo-salvarsan it is most important that the temperature should not exceed 80° F. In both cases the higher the temperature the more toxic is the solution.

It certainly does not detract from the value of a Wassermann reaction, that it has to be done by a pathologist, as there is no reason why every syphilologist should not carry out his own tests.

Mr. P. MACLEOD YEARSLEY said he would like to know, in regard to the action of "606," whether any Fellows with a large experience of this remedy had met with cases of auditory nerve deafness following its use. Last year Alexander, in the *Annals of Otology*,[1] suggested caution in treating by means of salvarsan patients who were already the subjects of auditory nerve deafness. Alexander quoted in support of his contention several cases, and related Ehrlich's experiments on white mice with arsacetin, which he found to produce degeneration of the central fibres of the vestibular nerve. The chief question upon which he (Mr. Yearsley) wished to speak was that of deafness in congenital syphilis. He did not consider that enough attention had been devoted to congenital syphilitic deafness. One authority had stated that syphilis does not cause deaf birth, but the researches of Baratoux and, more recently, of Otto Mayer, inclined one to the opinion that this statement was not true. But further investigation was required on the matter, and Dr. Kerr Love in Glasgow, and he (the speaker) in London, were carrying out researches thereon. Nothing definite could yet be said as to results, though he hoped they would shortly be able to offer some interesting facts. With regard to acquired deafness in congenital syphilis, in ten cases of children thus affected he had found Wassermann's reaction positive in only four. One of these had no other condition but the deafness, three had interstitial keratitis in addition, and of these only one had Hutchinsonian teeth, and one had active nasal syphilis. Of the six in whom the reaction was

[1] See *Journ. of Laryng.*, 1911, xxvi, p. 389.

negative all had old interstitial keratitis, five had old iritis in addition, four had Hutchinsonian teeth, and one showed nodes on the tibiæ. The reactions were done by a well-known pathologist. He did not think that the importance of congenital syphilis as a cause of acquired deafness, sufficiently severe to need special education in a school for the deaf, was realized as much as it should be. In the course of several years' experience in the London County Council Deaf Schools he had found that out of 576 cases of acquired deafness, of which the causes were definitely ascertainable, no fewer than thirty-nine were due to congenital syphilis, or 6·7 per cent. This figure was nearly three times greater than that given for the Paris institutions by Castex, which was 2·5 per cent. The majority of these cases had to be educated not only as deaf, but also as blind, and they furnished some of the most terrible and pitiful instances of the ravages of which the disease was capable. Surely such cases were preventable? and he was very glad to hear Mr. McDonagh enter so vigorously and fearlessly into the question from the public health point of view. It was pointed out some years ago that when deafness occurred in the congenital form of syphilis it did so in those in whom treatment had been neglected in infancy. Whether salvarsan in the first years of life would diminish this serious complication remained to be seen. That ordinary antisyphilitic treatment was of little use in them was shown by the fact that the ear involvement might appear and progress whilst the child was actually undergoing treatment for the eye complications. The only reliable method of treatment of congenital syphilitic deafness was repeated blistering, although he had had a limited number of successes with pilocarpine in the very early stages. What was required was, obviously, the prevention of such cases, and it would be valuable if an expression of opinion could be obtained in this discussion as to the best methods of proceeding to that end. The third sentence in the opening paragraph of Dr. Mott's remarks dealing with congenital syphilis in the offspring indicated the necessity of restriction of the marriage of syphilitics. Reliable data on this matter appeared to be lacking in this country, and Dr. Mott showed that some sort of State control was necessary in order to secure this end. The question was, would notification lead to better control and more efficient treatment of the congenital syphilitic in his early years, and so tend to the reduction of the number of cases of acquired specific deafness, and so, perhaps, even to its disappearance? Those were questions which he would like to see answered in regard especially to syphilitic deafness.

Mr. J. F. Briscoe: There is not a student of the profession who could not profitably discuss this overwhelming malady from the humanitarian point of view. And if some of us here to-day have learnt from years of practice its deadliness to human life their qualifications to join in the debate are enhanced thereby. We are asked as to its prevalence and intensity in the past and the present day. Has not Hogarth left on the sands of time a pictorial record, "The Scene in a Madhouse"? And there is a similar picture in Germany produced subsequently to Hogarth's masterpiece. The former was painted about 1770 and represents eleven mad people. One is closeted in a cell wearing a crown and holding the sceptre of office. Two ladies are passing his cell, at "Bedlam" it was called in those days. Their fans are held up, and one is viewing him through the spokes with a half-hearted sneer. The other figure is a man in another cell evidently aiming for the Papal See. Are not both of these syphilitic general paralytics, may I ask?

I suppose there cannot be any doubt but that there is less external disfigurement of the body from syphilis nowadays than heretofore, although I state this unsupported by statistics. Whether the treatment of to-day is more rapid of execution I cannot say, but undoubtedly we see syphilitic people less deformed externally than hitherto. I mean to say that rat-like eaten noses, carious skulls, and many of the syphilides and gummata on the external surface are less familiar. But as regards the interior life of the syphilitic patient, have we not a problem for thorough investigation? How many worthy specialists are there who can assert dogmatically that a particular disease is due to syphilis? And if we can compare human pathology with that of the lower animals, I am told that at the autopsy of a great white bear which died at the Zoological Gardens a few years ago a large aneurysm was found. I do not suppose he was inoculated with human syphilis or suffered from a comparative malady of the same nature; and we must suppose he was not an alcoholic, although at the same time domesticated animals do receive "nips" occasionally. Formerly, when we did not know the nature of a disease we assumed syphilis and we gave the iodides. To-day we use a test called the Wassermann reaction and apply our remedies accordingly. I have before my mental gaze a married couple where the virus of syphilis had played its part in the breeding processes. The father was the culprit, as is general in well-to-do marriages. He had sown his wild oats to perfection and

had contracted syphilis. He was treated by a well-known London physician for two years and then legally married a lady. The first year of marriage his wife miscarried and contracted puerperal fever. Chloroform had to be administered at the time to clear out a foul uterus, encouraged, no doubt, by syphilitic changes. The third year of marriage the lady again became pregnant, and in her fourth month she experienced uterine pains and hæmorrhage. This was quelled by iodide and mercury treatment, with rest in the recumbent position more or less, till full time, when she was delivered of a boy who had no external marks of syphilis. I may tell you she was attended by a well-known obstetric physician on both occasions. This was the last pregnancy, for the husband subsequently practised extreme precautions. The boy has now grown up and may be styled, conveniently, a "syphilitic warp" with a damaged nervous system. There is a neuropathic history, but has it not been emphasized by his syphilitic father? He is now a "ne'er-do-well" with extravagant fancies. He is not insane in the legal sense, although he is morally unsound. He can speak three or more languages, and would deceive the very elect with his cleverness. In another half decade or so I shall expect to find he is a general paralytic.

It would be wasting the time of this academy of medicine if every Fellow were to discuss in detail the relationship of syphilis to public health, for should we not all travel over the same ground in concord? Thus I have no intention of prolonging this discussion on the relation of syphilis to public health, since there is not a member of the profession who is not more or less conversant with its dire effects. Those of us who have served our apprenticeship in general practice and have become specialists in any department of medicine or surgery can bring to bear in this debate hideous instances of congenital syphilis. Nay, there is hardly a section of the Royal Society of Medicine in which syphilis does not play the principal part. And of the three scourges affecting the human race—viz., this disease, tuberculosis, and cancer—the first takes the lead to all intents and purposes, for have we absolute proof that it is curable in every sense of the word? The Wassermann reaction is an experimental method and an accepted addition to modern knowledge of this disease, but can we all agree that it is infallible and not open to attack by future critical research? It is true facts we want on this great question to-day before we approach the Legislature; whether, with any known remedy we possess we can remove every trace of the syphilitic virus, or if prevention is the best treatment.

I am inclined to think that it is the exception for a syphilized person to be really cured; but in some cases where cure is said to be established the virus may have become a "filter-passer" and beyond the microscope—nay, out of the pale of the Wassermann reaction. Disease undoubtedly cures itself.

We have seen cases of syphilis untreated pass along on an equal footing with cases which have been treated. Again, we know of the majority of patients who would be hideously damaged, mentally and physically, if they were not placed under care and treatment. But let us regard the Hogarth side of the picture, where the virus may have been locked up for years like the tubercle bacillus in hiding. And with a virus let loose from some accidental cause or stress after the tissues have been at peace for many years—here are properties of this infective disease in its tertiary period which must strike every Fellow who treats syphilis with despair. I was talking to a military man the other day, a syphilitic invalid, who had given up ordinary treatment but was now undergoing what he styled "Nature's cure." He had just arrived from Germany after his course of diet and baths. He eulogized this system of treatment without drugs, and remarked very intelligently, and with a good knowledge of what he was discussing, that by sweating, by the kidneys, and by the bowels he would eventually be cured without any chemical administration. He believed this, and I could not convince him to the contrary. He actually cross-questioned me on the action of the iodide of potash and the salt of mercury.

In medical practice we have to confess that recognized forms of treatment are not always sharply defined, so that in treating any disease we must not only have a thorough knowledge of pathology, but we must also be able to explain the ways and means in which a particular remedy acts on the diseased tissue. Short of this, are we not speculative empirics? There are two methods of treating disease —namely, by drug or serum treatment for our practical purposes to-day, or by prevention. The public will assuredly decide in the near future the particular line they prefer. As it is possible to wipe out tuberculosis by cleanliness, so is it possible to cleanse the race of syphilis. Very shortly, no doubt, we shall discover that cancer is preventable. In hospitals for the insane appendicitis is an unusual complaint, and it is not difficult to prove why this is the case.

In summing up the treatment of syphilis it would be idle waste of time on my part to enumerate the drug treatment, for every student of

physic learns this in the rudiments of practice. Circumstances alter the treatment of cases, for while one might treat soldiers and sailors by gluteal injections, one might prefer for other classes of society mouth, inunction, or bath treatment, associated with dietetic and other hygienic measures. If I am attached to any particular line of drug treatment it is the old-fashioned liq. hydrarg. perchlor. and pot. iodid. in combination with cinchona bark, spirits of chloroform, syrup and glycerine, although I have taken up the new line, salvarsan.

As an appendix to these remarks I cannot refrain from alluding to the cruel consequences of syphilis which we observe in our private and public hospitals for the insane. As a specialist in this direction it is appalling to see and live with folk of your own class in life who have fallen victims to gross changes of the nervous system by, perhaps, one sexual act with an unclean woman. They are tabetics or they are general paralytics, and they are incurable. No syphilis, no general paralytics, as we say in the Psychological Association. And it is to Dr. Mott, our great representative pathologist, to whom the whole profession must bow in adoration of his untiring energies in the elucidation of the syphilitic affections of the nervous system.

In conclusion, I excuse myself from taking up your time with these fragmentary statements, knowing too well how you will support an enthusiast who has endeavoured to assist in ventilating the question of a contagious disease which through prejudice is not included in the list of notifiable diseases. May the days yet come when preventive measures will stay the inroads of this vile malady and lessen mental and physical crippledom.

Dr. G. PERNET said he did not intend to read a paper, but would discuss some of the points which had been raised. Dr. Norman Moore's illuminating analysis of Galen's work constituted a further proof, if any such were needed, as to the Columbian origin of syphilis. He did not think there was any doubt that the disease was brought over from America by Columbus, and much of what had been written concerning the prevalence of syphilis in ancient Greece and Rome, and Egypt, could be put on one side. With regard to the antiquity of syphilis in China, a recent Japanese investigator had looked into the original manuscripts which had been examined by a French naval officer many years ago, who was not a medical man by the way, but whose work was constantly quoted. The Japanese authority could find no evidence as to syphilis having existed on the strength of those manuscripts.

With regard to Dr. Mott's extremely interesting survey of congenital syphilis, he was very pleased to find that he used the term "congenital" and not "hereditary." He did not consider that there was any proof that the disease was hereditary; he was of opinion it was not hereditary, any more than were tuberculosis and leprosy. What Dr. Mott said concerning the germ-plasm should certainly be taken into account, and he thought medical men might well take more interest in biological problems. The term "heredity" was often used in a loose and unscientific manner. In Dr. Pernet's opinion we were merely the scenery for the germ-plasm. Colles, of Dublin, in his admirable clinical work, had had the merit of pointing out that syphilis was not contracted by a mother suckling her syphilitic infant. Dr. Pernet considered that syphilis in the child was always conveyed through the mother, and that there was no direct paternal syphilis.[1] The Wassermann reaction was in favour of the maternal origin. A related matter of great importance was the following: It had been laid down, and rightly so on the available data, that marriage must not be allowed for many years after a man had contracted syphilis. But if the disease was not conveyed directly by the spermatozoon to the ovulum, and bearing in mind modern methods of treatment, he thought the interval between the occurrence of the primary sore and marriage might perhaps be shortened, with the Wassermann test as a guide, but that point required careful consideration and should not be decided upon without mature reflection. In his view, the original Wassermann reaction was the best; the test was approximate, not infallible. With regard to treatment, Mr. D'Arcy Power, Mr. Ernest Lane, and others, had gone into it at such length that he did not propose to say much upon it himself. He considered certainly that the use of salvarsan should be followed up by mercury; in our present state of knowledge, at any rate. As a clinician, he was not prepared to be guided utterly by purely laboratory workers, who generally knew very little practically about syphilis. Salvarsan was valuable, though it had drawbacks. In private practice one must be guided by all the circumstances surrounding an individual case. Sometimes these circumstances did not allow of certain methods being employed. He was therefore very eclectic in the treatment of syphilis. No strict line of treatment could be laid down for private practice which would be applicable to every case in a routine way. It had been his intention to insist on the utter inadequacy of the teaching of syphilis

[1] *Vide* Pernet, "Reports of the Society for the Study of Disease in Children," 1907, viii, p. 74.

in London, and in this country generally, as compared with what obtained in the great centres on the Continent. It was high time this should be remedied. When he was doing midwifery "on the list" in his hospital days, students were never told a word as to the dangers of contracting syphilis extragenitally. He had known several cases of digital chancres contracted in this way. Many young men coming up to study medicine—and this applied to nurses too—were quite ignorant of these dangers. It was nobody's business to enlighten them systematically on the point, nor were digital chancres always diagnosed, certainly not in his early days at hospital. He had known of several cases of amputation of finger-ends unnecessarily carried out owing to ignorance. The question of the prevention of syphilis was another matter of importance. He was not in favour of the State regulation of prostitutes. It had never answered in civil practice, leading to all sorts of iniquities, and had failed utterly from the point of view of prophylaxis. Ideas on the Continent on the matter were undergoing a great change. In Paris, for instance, Professor Landouzy and many others were against the regulation of prostitutes, because, though such regulations might appear to be perfect on paper, in practice they were bad, and like a good many foolish laws led to nothing except tribulations and waste of time. There was a tendency to pass too many petty laws. As to the proposal to notify venereal diseases, he did not agree with Mr. McDonagh. He (Dr. Pernet) was distinctly against it. What was the use of making medical men take the Hippocratic oath? It was the duty of the medical profession, and especially of the Colleges of Physicians and Surgeons, to resist such interference with the rights of the sick seeking the aid of the medical man. What passed between medical man and patient should be inviolate. One speaker had referred to prescribing chemists and quacks, but what could be more calculated to drive people into their hands than the notification of venereal diseases? Dr. Pernet was a medical man, not an amateur policeman.

Mr. C. F. MARSHALL[1]: With regard to the treatment of syphilis, Ehrlich's experiments in chemio-therapy are of much scientific interest, but the results reported from the use of salvarsan in human syphilis are premature from the scientific, and ill-advised from the social point of view. Owing to sensational reports of alleged cures, many persons have believed themselves cured after one or two

[1] Communicated by Mr. D'Arcy Power.

injections, have neglected further treatment, and suffered in consequence. In such a disease as syphilis the value of a new drug cannot be estimated till it has been tried for at least ten years. The chief tests of the efficacy of such a drug are its power in preventing tertiary or parasyphilitic manifestations and the transmission of the disease to the offspring. These points have been proved for mercury by the statistics of Fournier, so far as statistics and a life-long experience of the disease can prove them. In the case of salvarsan the time is too short to prove these points. Salvarsan has a rapid cicatrizing action on certain lesions of syphilis of the ulcerative type, but it does not prevent relapses, and is of doubtful value in other syphilitic manifestations. As regards the possibility of aborting the disease in the early stage, it is difficult to prove this either for salvarsan or mercury, for several reasons: (1) It is possible that in some cases syphilis may undergo spontaneous abortion; (2) secondary symptoms may be so slight as to pass unnoticed, and tertiary symptoms may occur after some years; (3) the fact of re-infection is not absolute proof of the cure of the first attack, for the experiments of Finger and Landsteiner have shown that immunity is relative and not absolute. Moreover, it is possible that some cases reported as re-infections were cases of chancriform gumma.

Salvarsan appears to be liable to cause severe toxic effects, sometimes ending fatally. The advocates of salvarsan have attempted to explain away the accidents and deaths after injection in various and ingenious ways; by faults in technique, by the solution being too acid or too alkaline, by administration in hopeless and unsuitable cases, by neglect of the contra-indications, by excessive doses, by injection at too frequent intervals, by the presence of microbes in stale saline solution, and by the effects being due to the disease and not the drug, and so on *ad nauseam*—anything to avoid a verdict of arsenical intoxication. One of the latest ideas is that of Milian, who attributes certain accidents (epileptiform convulsions followed by coma and death) after salvarsan to (1) decomposition of the original dichlorhydrate into a monochlorhydrate and possibly other products, all of which he designates by the name of "para-606"; (2) deficient power of the blood to neutralize the acidity of these products. To avoid these accidents he recommends the injection of a strongly alkaline solution, and also treatment of the patient to render his blood neutral. It would almost appear advisable to breed a special type of patient to withstand the injection of salvarsan with impunity! If this drug is

so easily altered in composition that a slight difference in alkalinity may render it safe or lethal—a difference which depends not only on the drug itself, but also on the condition of the body fluids of the patient—it would appear a questionable procedure to use it in a disease which is curable in the great majority of cases by drugs of simpler and more stable composition, such as mercury and iodides.

No doubt many of the deaths after salvarsan (which now probably exceed 100) were due to faulty technique, to administration in hopeless cases and to neglect of contra-indications, but a certain number are difficult to explain except by arsenical poisoning. In some cases salvarsan appears to have caused acute arsenical nephritis followed by uræmic convulsions, coma, and death. In a case reported by Gaucher, 19 mgrm. of arsenic were found in the viscera in a case of tabes which died after salvarsan.

The majority of observers now recommend prolonged mercurial treatment in addition to salvarsan. The latter is, therefore, reduced to the position of an auxiliary drug, and it may be asked whether an auxiliary drug of unstable composition, liable to cause severe and fatal toxic effects, is indicated in a disease which in most cases is curable without it.

Mercury still remains the essential drug in the treatment of syphilis, and iodide of potassium the best auxiliary drug. Iodides are useful in all stages of syphilis, not only in the tertiary period. Iodide of potassium is superior to iodipin or any of the so-called substitutes for it. Other useful auxiliary drugs are quinine, iron, and sulphur. Arsenic is sometimes useful, in the form of Donovan's solution, but the organic preparations of arsenic are of unstable composition and liable to produce toxic effects.

The majority of cases of syphilis can be treated perfectly well by the mouth. Some authorities regard this method, which they refer to disparagingly as the "pill treatment," as inefficient. But the "pill treatment" includes all forms and combinations of mercury and iodides. Moreover, so long as there is evidence of the active action of mercury on the organism, it matters little in what way it is given. In severe cases, or when a rapid effect is required, inunction is the best method, and it is no doubt useful to begin treatment in other cases with a course of inunction. When this cannot for various reasons be carried out, or when mercury cannot be tolerated by the stomach, injections are useful. Soluble injections are safer than insoluble, which are liable to leave

deposits of mercury at the site of injection, which may become suddenly absorbed after some trauma and give rise to severe mercurial poisoning. Gaucher has reported ten cases of gangrenous stomatitis after grey oil injections, eight of which were fatal, and cases of fatal ulcerative colitis after injections of calomel and salicylate of mercury.

In conclusion, it may be confidently stated that no drug has yet been produced which can replace mercury in the treatment of syphilis.

The Royal Society of Medicine.

June 24, 1912.

Sir HENRY MORRIS, Bt., President, in the Chair.

A Discussion on Syphilis, with special reference to (*a*) its Prevalence and Intensity in the Past and at the Present Day; (*b*) its Relation to Public Health, including Congenital Syphilis; (*c*) the Treatment of the Disease.[1]

Major H. C. FRENCH, R.A.M.C.: I have the honour of re-opening to-day the discussion on "Syphilis in Relation to the Public Health," including congenital syphilis. This is a sequel to other recent discussions on this disease in the various medical societies of London. That it is the first of an important series of debates contemplated in this newly opened building and lasts for four days, is the best test of the importance that attaches to it in the opinion of the medical profession. It is to be hoped, therefore, that some definite recommendations may now be drawn up, and an earnest attempt made to ensure legislative control of venereal diseases—a subject quite apart from the State regulation of vice. These diseases cost the country millions of pounds, they fill many homes with preventable misery, they overcrowd our workhouses, prisons and lunatic asylums with imbeciles, idiots, criminals, epileptics and other insane persons; they sap the vigour of a nation, and if uncontrolled may eventually endanger its very existence.

I pointed out two years ago in an article on the "Control of Venereal Diseases at their Source in Civil Communities,"[2] that Dr. Holland, in 1854, estimated that in the United Kingdom there were at least a million and a half persons infected with syphilis each year. Dr. Mott

[1] Third meeting (adjourned from June 17).

[2] *Brit. Med. Journ.*, 1910, ii, pp. 1766-68.

states that possibly 3 to 4 per cent. of cases infected with syphilis are followed by parasyphilitic affections such as tabes and general paralysis. Further, we know that 11 per cent. of male private and 8 per cent. of male pauper admissions to lunatic asylums are due to this cause. The Wassermann reaction now throws a searchlight on idiocy, imbecility and epilepsy, due to acquired or to congenital syphilis, which were not previously recognized as due to this cause. As the feeble-minded outnumber certified insane persons the part played by syphilis in the causation of this condition requires investigation.

Dr. Duncan Bulkley states that in New York 300,000 cases of skin diseases gave 11·5 per cent. due to syphilis.

Dr. Le Noir, at the International Conference held at Brussels, in 1899, stated that in Paris there are fifteen syphilitics out of every hundred adult men. Fournier, judging by hospital practice, gives 17 per cent. There is no reason to suppose, therefore, that a large city like London, without any control, and with only 100 hospital beds, suffers less from this disease than Paris with 2,000 beds, or Berlin. In both of these smaller cities control and adequate means of segregation exist.

Taking the British Army as the only existing index to the relative prevalence in civil communities, the average number of cases constantly on the Syphilis Register for two to three years and undergoing treatment is probably 3 to 4 per cent. of troops, but numerous other cases have previously suffered, and have been struck off the Syphilis Register. The percentage of freshly contracted syphilis in my experience in the past few years is much higher amongst troops in England, more especially in London and Woolwich, than amongst those abroad. This is easily explained, and is no doubt due to lack of control at the source, as the disease is not as yet notifiable in England. Further, since diseased persons in civil communities are not segregated, disease is spread broadcast. I would strongly advocate that in all statistics the occurrence of freshly contracted disease be differentiated from relapse of pre-existing illness.

As regards relative incidence amongst European armies where control at the source exists in the civil community: In the years 1886-90 the average admission ratio per 1,000 for all venereal diseases was 27 in the German, 51 in the French, 65 in the Italian, and 212 in the British Army. Ten years later, in 1900, it was 17 in the German, 37 in the French, 60 in the Italian, and 93 in the British.

The routine method of treatment in vogue in the French and German armies from 1886-1900 was mercurial inunction.

Regulation of prostitution existed, and still exists in France and Germany, and existed in England from 1864-86, but was abolished in England in 1886, in India in 1888, and in Italy in 1888 by Crispi, when a marked increase of disease ensued in each of these countries. In India, in 1895, the admission ratio for all venereal diseases rose to 537 per 1,000, but as the result of the Cantonment Act, control of disease at the source has again existed since 1897. In India, in 1910, the admission ratio to hospital for all venereal diseases has consequently been reduced to 59 per 1,000. This enormous reduction lowers considerably the totals of venereal diseases for the whole British Army since 1897 (*vide* Table III).

As regards innocently acquired and congenital syphilis, Dr. Bulkley, New York, states "that in Russian villages where prostitution is unknown, syphilis decimates families, and is spread in an innocent manner." Judging by personal experience in India, from 1896 to 1901, this is frequently the case amongst native women. In Indian villages in these years, plague, famine and cholera were prevalent, and a large number of women suffering from venereal disease then arrived in British Cantonments.

Dr. Bulkley has tabulated 110 epidemics of innocent syphilis with a total of 3,000 victims. The common causes of conveyance, apart from heredity, are "nursing, hand-rearing of infants, breast-drawing, accouchements, vaccination, tatooing, household utensils; dental, barber's and other instruments, kissing, pipes, &c." At Philadelphia, recently, there was an outbreak of eight cases of syphilitic chancres of the lip as the result of playing "kiss in the ring."[1] Fournier, in Paris, found that fully 25 per cent. of all females whom he had seen in private practice had contracted the disease innocently and undeservedly. Of the married females, in 75 per cent. of cases the disease was traced to the husband.

Tarnowsky quotes the case of three families with twenty-two children, from whom only one healthy adult survived. This was in the intelligent class of society. At Moscow, Russia, in a period of ten years there were 2,002 births from syphilized parents. Of these, 1,425 (71 per cent.) died. In regard to congenital syphilis, therefore, we are not justified in supposing that in England we stand on a separate pedestal.

As syphilis causes innumerable miscarriages and abortions, it would appear that from a mere population point of view, as well as for the perpetuation of a healthy race, control of venereal diseases at the source has become absolutely essential. This is no longer a subject of purely

[1] *Lancet*, 1911, ii, p. 907.

academic interest, but perhaps the most important social problem of the day. In Chicago, the Church is objecting to marry persons without a medical certificate of physical and mental fitness. It is considered that a large amount of unhappiness causing divorce is due to venereal diseases, and in this country it constitutes legal cruelty.

Alcohol, Syphilis and Insanity.

Alcohol unquestionably plays an important part in conducing to sexual immorality, and if disease is contracted, in perpetuating syphilis and in causing relapse. In conjunction with syphilis alcohol is responsible for 27 per cent. of private and 31 per cent. of pauper male admissions to lunatic asylums in the United Kingdom. Mr. McDonagh's view, however, is not concurred in that the reduction in venereal diseases in the British Army in recent years is due to the growth of temperance (*vide* Table IV). A reduction in alcoholism was non-existent prior to 1907 in India. Venereal diseases, however, subsequent to the Cantonment Act, 1897, have steadily decreased every year until 1910, both as regards admission and constantly sick ratios. It is true that since 1907 there is a slight reduction in the admissions and constantly sick in hospital for alcoholism. A soldier, however, is only admitted to hospital for alcoholism, but not for drunkenness. He loses more pay on admission to hospital—more than the small fines for being drunk outside. Further, in addition to the loss of sevenpence a day incurred by every soldier on admission to hospital, from April, 1904, soldiers also lose service pay, and since October, 1906, they lose proficiency pay when admitted to hospital, either for alcoholism or venereal diseases.

I do not acquiesce in Mr. McDonagh's further statement, "that about 90 per cent. of infections take place while the victims are under the influence of alcohol." In order to arrive at an independent conclusion, I have recently tabulated 461 cases of soldiers admitted to hospital under my care in the past year at Malta, with freshly contracted venereal disease, and find that 203 were total abstainers, 112 belonged to Section B, Army Temperance, or consumed less than 2 pints of beer daily, 46 consumed 3 pints, and 100 consumed over 3 pints a day. When due allowance is made for all these factors, it is not possible on the evidence to arrive at the conclusion that the recent reduction of syphilis at Malta, and of venereal diseases amongst 70,000 British troops in India, since 1897, and in the Army generally, can be fairly attributed

to increased temperance. My view was also held by Sir Alfred Keogh, late Director-General Army Medical Service, and expressed in his introductory remarks in a recent book.[1]

PREVENTION.

Coming to the subject of prevention, it is interesting to trace the history of what has been done in this country. In 1896 the Secretary of State for the Home Department was approached with the object of appointing a Departmental Committee to inquire into the subject of the prevalence and of the treatment of venereal disease amongst the population of England. The Government, whilst admitting the importance of the subject, did not think the time had arrived for taking action in the matter, because there was not then a "sufficiently informed public opinion" on the disease to justify them in so doing. As medical men, therefore, it is our plain duty to enlighten, and if possible to guide, public opinion to a correct appreciation of the situation. Having done this, the responsibility then rests on the public and on the State, who must each work out their own salvation.

As the *British Medical Journal* pointed out in 1899,[2] "It would seem that in consequence of this state of things a considerable number of women do not receive proper treatment in the early stages of syphilis, and seek admission to various homes of refuge in London, administered by ladies who devote their lives to work of charity of this kind.

"The ladies have been so deeply impressed with the extent of the misery caused by these maladies in young women, that they associated themselves into a numerous and influential body, determined to their utmost to secure measures as may be thought best, after due inquiry, to check the ravages of syphilis."

The Council of the British Medical Association then appointed a Committee, and urged on Her Majesty's Government the necessity for appointing a Departmental Committee to inquire :—

(1) As to the extent to which venereal disease prevails amongst the civil population of Great Britain, irrespective of its temporary increase or decrease.

(2) To collect information as to the present arrangements for the treatment of venereal diseases, the distribution of hospitals, and the number of beds available in different places, and to make suggestions as to the more efficient provision for the treatment of the disease.

[1] "Manual on Venereal Disease in Army, 1908" (Sir A. Keogh, Melville, Leishman, Pollock).

[2] *Brit. Med. Journ.*, 1899, i, p. 984.

(3) To collect suggestions and to express opinions as to the means that can be devised for preventing or limiting the spread of venereal disease among the civil population of this country.

The above resolution was also forwarded to the President of the "International Congress on Venereal Diseases," which met at Brussels, on September 4, 1899, urging the Congress to support the resolution by requesting the Governments of the respective delegates to pursue a similar investigation with a view of devising a uniform plan of action to mitigate the spread of this terrible disease among the people of Europe." This was finally embodied among the eight resolutions of the Congress. Further, the Royal College of Surgeons of England, in the Annual Report for the year 1897, contains a statement to the effect that the Council of the College had forwarded an address to the Secretary of State for India regarding the prevalence of venereal disease amongst the British troops in that country. In this address the Council observe: "We therefore express an earnest hope that Her Majesty's Government may take effective means to check the ravages of the disease (venereal) which not only undermines the constitutions of those who contract it in the first instance, but by reason of the many ways in which it may be transmitted destroys the health and happiness of countless persons, and induces in the children of those originally infected diseases of a most formidable character."

As the *British Medical Journal* aptly points out: "The influential body of ladies who have taken up this subject are perhaps the best possible exponents of public opinion in this matter. Only a deeply rooted conviction formed from their observation of the terrible misery this disease causes, would have induced these ladies to have come forward as they have done regarding a question of this kind."

So far as England is concerned the matter died a premature death, "whilst Nature breeds perverse all monstrous all prodigious things," as in the age when Milton wrote.

As regards India, however, the voice of these ladies carried the day. There was a meeting on this subject at St. Martin's Town Hall, London. "Admission was by ticket for which a charge was made, and nine-tenths of the audience were women." This effected the passage of the Cantonment Act, India, which came into force in October, 1897, with such marvellous results and stupendous saving to the Indian Government. India, therefore, must be our principal guide as to the effect of control at the source, as there are some 70,000 British troops always stationed there under more or less identical

conditions. In India, in 1895 to 1897, and previously, there were 3,000 soldiers constantly sick in hospital with venereal diseases. These diseases caused 32 per cent. of the admissions and 42 per cent. of constantly sick in hospital from all diseases. These numbers, in 1910, have been reduced to 500 soldiers constantly sick in hospital, and venereal diseases caused only 10 per cent. of the admissions and 25 per cent. (1909, 20 per cent.) of the constantly sick in hospital from all diseases. This represents a yearly saving to that country of £55,615 when reckoned at one shilling *per diem* for hospital diet, and this quite apart from loss of service. During the same period the annual number of invalids from India to England for syphilis have been reduced from 611 in 1897 to 18 in 1910. A trained soldier landed in India costs the State £100, so that the annual saving in invalids alone is enormous. Whereas, in the United Kingdom in 1910, where no control at the source exists, venereal diseases in an average strength of 108,614 troops accounted for 31·8 per cent. of the constantly sick in hospital for all diseases. These latter figures probably indicate the average prevalence in civil communities in the United Kingdom.

The paragraphs relating to sanitary prophylaxis in India are briefly given elsewhere, and the underlying principles considered in detail.[1] The practical result has been that the Cantonment Act in conjunction with prophylaxis by treatment has effected an enormous reduction in India for all venereal diseases from 537 admissions per 1,000 in 1895 to 59 admissions per 1,000 in 1910. This demonstrates for all time the efficacy of adequate control.

In some British Colonies prophylactic methods already exist as regards control at the source. They support the good results obtained in India. Foreign medical opinion on this subject is given in Appendix A.

Briefly considered, some essential principles in the control of prostitution and venereal disease culled from twenty years' practical experience in many countries are as follows:—

(1) Confidential medical notification of disease on prima facie evidence and medical treatment for short periods in hospital in the early actively contagious stages of disease. The steps taken being dependent on environment and the circumstances of the individual case. As syphilis, five times out of six, is spread by clandestine prostitution, according to the experience of France, Belgium and Germany, such notification by medical men more adequately meets this difficulty.

[1] "Syphilis in the Army," 1907.

Cases (without names) are reported to the health authorities when the exact address is known, and arrangements are then made for the segregation and treatment of diseased persons as in the case of other diseases. This was brought forward two years ago.[1] I have practised it for three years at Malta with marked success (*vide* Table II).

(2) The effectual control of openly practised prostitution by the localization of irreclaimable women into certain areas or streets is essential. Such control of openly practised prostitution is attended by incalculable benefit not only to the women themselves, but also to the civil community, in the reduction of venereal diseases. When control does not exist, or is temporarily removed, disease becomes as frequent among such women as amongst clandestines.

(3) The rigid suppression of souteneurs who act as middlemen and live on the earnings of women, and even marry with this object in view. These men are often criminals of the worst description and levy blackmail on their clients whilst they batten on the poverty of the victims in their clutches. A valuable short Police Bill for the suppression of these men and the white slave traffic has recently passed its second reading in the Commons. This will tend to prevent London being made the clearing house for Europe for prostitutes sent from other countries or who move owing to disease.

(4) The protection of orphan children and minors and the suppression of begging in the streets by children under 12 years of age. This includes the suppression of women souteneurs, who adopt orphan children in order to live on their immoral earnings when, from age or disease, they themselves have lost their charms. No girl under 21 years of age should be allowed to reside in a brothel in any capacity.

(5) The suppression of loitering and solicitation in the streets by women, or men acting on their behalf. This is possible in England under the Town Clauses Act, 1847. If men want prostitutes they must go and look for them where they lodge. This would lessen the work of the police, who are now obliged for hours at night to patrol in large numbers certain parts of the town. London is about the only European city where such a state of things exists. As regards educating the young, it was pointed out in Parliament recently "that a young girl is taught how to find her way to Timbuctoo but not how to avoid Piccadilly Circus." The public display of immorality discouraged in our theatres we encourage by the parade of prostitution in the streets.

[1] *Brit. Med. Journ.*, 1910, ii, p. 1766; and *Lancet*, 1911, ii, p. 1389.

(6) The provision of free voluntary dispensaries where women who do not openly practise prostitution (clandestines) may be treated and reclaimed. These should be open at hours which are suitable to the working classes. Quite apart from the publicity involved, many poor persons cannot afford to wait in the out-patient department of large hospitals for several hours. In addition to lock hospitals for irreclaimable women, I would advocate that sufficient beds be set aside for venereal diseases in every large London hospital and special courses of instruction arranged for students, as in Paris.

(7) Removal of disorderly persons, and measures to prevent the return of evicted persons, and to prevent harbouring of diseased prostitutes.

(8) The control of diseased merchant seamen who spread the worst forms of disease at seaport towns. This is quite feasible under Port Sanitary regulations.

(9) Control of persons seeking medical aid from chemists for venereal disease, who spread their complaints broadcast. Civil practitioners should notify soldiers on the active list seeking medical aid at civil hospitals, as they commit an offence under the Army Act, spread disease, and increase military inefficiency. This is the law in Prussia.

(10) Circumcision of male infants and of all recruits entering the Army with phimosis is strongly advocated on Jewish and Mahommedan evidence and markedly protects against syphilis.

Such measures may not completely deal with this difficult social problem, but they are the condensed experience of practical work as opposed to theoretical considerations. They are the bedrock on which an adequate superstructure can be laid. They do not conflict with public morality, but minimize disease, misery and death.

I have attempted to deal to-day with some of the main principles involved in the control of such a protean disease as syphilis which, octopus-like, has a tentacle fixed on each branch of medical, gynæcological and surgical practice.

That the disease can be effectually controlled in a community is fully exemplified by personal experience of this work in India, Egypt, Malta and England. The finding of a Royal Commission in England in 1870, and also in India, conclusively demonstrates the inestimable benefits derived from control and systematized effort. (Appendix A.)

The figures for Malta (Table II) were not available in November last year, when I attempted to deal with some recent developments in the diagnosis and treatment of syphilis in two Hunterian Lectures at the Royal College of Surgeons of England.[1]

[1] *Lancet*, 1911, ii, pp. 1315, 1389.

The reduction in syphilis effected in recent years amongst soldiers in Malta has mainly resulted from the more thorough control of venereal diseases at their source in the civil community, which has enormously benefited. This has been effected by means of a system of confidential medical notification and more effectual segregation of diseased persons, commencing in October, 1909, and applied equally to both sexes. A Government Ordinance has existed in Malta since 1861, and this was amended in 1898, to make it in effect a sanitary ordinance on the lines of the Indian Cantonment Act of October, 1897.

Amongst troops in Malta, in 1908, syphilis caused 18·6 admissions and 2·33 constantly sick in hospital per 1,000. This was not high, but it has been reduced to one-fourth in 1911—namely 4·3 admissions and 0·42 constantly sick per 1,000, and no invalids for syphilis, gonorrhœa, or soft chancre. This result occurred in a garrison of 7,000 soldiers amongst a civil population numbering nearly a quarter of a million of persons, and a fleet 7,000 strong arriving periodically. Not only was the incidence of freshly contracted disease lessened, but all forms of disease, syphilis, soft chancre and gonorrhœa were also attenuated.

In Table II the actual number of soldiers on the Syphilis Register and the actual number of cases contracted in Malta, with such medical notification in force during my tenure of appointment since September, 1909, are contrasted with the admission and the constantly daily sick in hospital ratios.

In can be observed that in 1910-11 there is a progressive reduction both in the incidence of syphilis contracted in Malta as well as in the admission and constantly sick ratios. The two latter, though partly dependent on the incidence of freshly contracted disease, are more largely influenced by administrative measures plus medical treatment; the sum total being largely dependent on the lubricating oil of individual initiative, on the introduction of measures to meet the frequently changing exigencies of local requirements, and on systematized procedure and centralization of work.

The Naval and Civil Authorities also report the marked reduction of disease; and provided that the immigration of diseased persons from outside areas can be controlled, poverty and its twin sister disease will eventually be lessened by a reduction in the cost of maintenance of poor-houses, prisons and lunatic asylums, in which places syphilis is domiciled even to the fourth generation. What has been done in the Island of Malta can equally be done in the British Isles.

In Malta, several factors have contributed to good results, and in the

case of syphilis they have been obtained with mercury, potassium iodide and local treatment. The results under mercury endorse those which I obtained at Woolwich (Table I) in 1905-09, and endorse Indian experience (Table III).

The marked reduction in syphilis from 1909 to 1911 in Malta, in India, 1898 to 1910, and throughout the Army generally as exemplified by these tables, occurred before the introduction of salvarsan.

A "Syphilis Record Book," introduced in 1904, and modified in 1910, exists in each station, and the particulars from the syphilis case-sheet are entered for permanent reference.

These general principles have recently been applied to gonorrhœa, and a "Gonorrhœal Case-sheet" as an official Army form is now in use. Finally, by means of a venereal surveillance form, cases are followed up on discharge from hospital or on transfer to other stations.

It may prove of interest to trace the origin of methods which, in a modified degree or in a different manner, could be easily made applicable to the control of venereal diseases in civil communities.

In July, 1903, I applied to the War Office for permission to publish some printed matter, the results of investigations in India from 1896 to 1901, and previously, on the control of venereal diseases in the Army. The absolute necessity of entirely revising the collection of venereal statistics, systematic case-taking, and accurate diagnosis was emphasized. Up to this date all cases of venereal sore, whether soft chancre or early primary syphilis, were indiscriminately recorded as primary syphilis. If secondary syphilis followed, then a change to secondary syphilis was made if the case happened to be in hospital. This was confusing and misleading. In September, 1903, therefore, a preliminary tentative scheme was brought into force—"Instructions regarding Procedure in Cases of Syphilis." Under this scheme syphilis and soft chancre became absolutely differentiated.

From January 1, 1904, the scheme was made universal throughout the whole Army at home and abroad, and was later incorporated into Appendix VII, Regulations Army Medical Services.

Under this scheme all venereal sore cases after discharge from hospital are now kept under weekly observation, according to time spent in hospital, for two to four months before the diagnosis of soft chancre, or syphilis, is finally made. This is very important in limiting the local spread of disease arising from relapsed chancres, or a recrudescence of disease. Latterly, I have kept gonorrhœa cases under weekly observation for six weeks from the date of discharge from hospital, with

excellent results in lessening disease both in the case of the soldier and the local civil community.

In 1907, I forwarded three copies of a book ("Syphilis in the Army") to India to the then Commander-in-Chief, to the Principal Medical Officer, and to the Sanitary Commissioner with the Government of India, who all took a keen personal interest in the matter. At the Ambala Conference in India on "Venereal Diseases and Enteric Fever," which later met in November, 1907, a large number of the suggestions were given practical effect and applied to the administrative control of venereal diseases in India, with excellent results, more especially as regards invaliding.

The far-reaching and beneficial effect of the above system is now fully manifest in Army Medical Department Annual Reports. These methods, in conjunction with continued treatment and the active co-operation of Army Medical Officers, have effected not only an enormous reduction in all forms of venereal diseases, but have also effected a considerable reduction in the total of all diseases and in disease indirectly due to venereal complaints, or aggravated by them. Venereal diseases, however, still account for 27 to 30 per cent. of the constantly sick in hospital from all diseases in the Army.

If, therefore, the Legislature in this country is later guided by our deliberations, the administrative system of control existing in the Army can easily be adapted or improved on to meet the special requirements of civil communities in England, and the scourge referred to in the Second Commandment that reaches to the fourth generation may be limited to one or be extirpated in this generation. It is first necessary, however, to get legal control of syphilis in the manner that small-pox and other much less dangerous diseases are controlled.

It is not proposed to discuss whether salvarsan (606) or neo-salvarsan (914) is the crux of the treatment of syphilis, or whether a continuance of mercury is permissible, or a combination of both drugs is advisable. These are problems that in the present find their own solution, and which the riper experience of the future will finally decide.

It is absolutely essential, from a public health point of view, to go to the Legislature of the country with concrete proposals and undivided counsels. Public opinion has been recently educated to the immense importance of stamping out syphilis, and the introduction of salvarsan has given a temporary fillip to public interest. Like a spur, however, applied to a jaded horse, public interest, though stimulated to a gallop, very soon relapses to a jog-trot, but poverty and disease continue for ever.

There are two important Bills now before Parliament indirectly bearing on this subject, one dealing with the feeble-minded and the other with white slave traffic. The notification and control of venereal diseases, however, is of even more importance to the community.

It is essential to recognize that it is not possible entirely to eradicate prostitution, since it is primarily dependent on poverty, and its entire suppression would give rise to much worse evils in the community. It is feasible, however, to limit prostitution, to eradicate disease, and to safeguard minors and orphan children. To ignore the question merely increases underlying evils. Inquiries in Brussels extending over twenty years elicited the fact that amongst 1,523 out of 3,505 women the primary cause of prostitution is poverty.

In summarizing the means at our disposal it is requisite to bear in mind that there are three main principles involved in the prevention and control of venereal diseases.

First: Control at the source which is concerned with prostitution before disease is contracted or spread. This embraces (*a*) medical notification of disease; (*b*) suppression of souteneurs and solicitation in the streets; (*c*) protection of orphan and destitute children.

Secondly: Prevention by medical measures—i.e., prophylaxis by treatment after disease has been contracted. This includes (*a*) provision of hospital beds and segregation; (*b*) professorships at large hospitals; (*c*) instruction of students and the public.

Thirdly: Moral and religious considerations. These are placed last, but are by no means least, and are more applicable to youth and adolescence before disease is contracted. At the Brussels International Conference, in 1899, the consensus of medical evidence was distinctly in favour of revised regulations for dealing with prostitution apart from the State control of vice (*vide* Appendix A). The abolitionists of regulation strongly advocated the notification of venereal diseases, and female inspectors of factories, and reformatories for young girls aged under 21, who adopt a vicious life.

In conclusion, I refer briefly to the main stumbling-block, the ethics of the situation, which are either not understood or the realization of which we shirk.

I quote from Mr. Lecky's "History of European Morals" (vol. ii, pp. 280 *et sqq.*) who in referring to prostitute women says: "Herself the supreme type of vice, she is ultimately the most efficient guardian of virtue. But for her the unchallenged purity of countless happy homes would be polluted, and not a few who in the pride of their

untempted chastity think of her with an indignant shudder, would have known the remorse of agony and despair.

"On that one degraded and ignoble form are concentrated the passions that might have filled the world with shame. She remains, while creeds and civilization rise and fall, the eternal priestess of humanity blasted for the sins of the people.

"The evil rarely assumes such inveterate and perverting forms as when it is shrouded in obscurity and veiled by a hypocritical appearance of unconsciousness. The existence in England of certainly not less than 50,000 unhappy women shows sufficiently what an appalling amount of moral evil is festering uncontrolled." Mr. Lecky further deplores the fact "that an epidemic, which is one of the most dreadful now existing amongst mankind, which communicates itself from the guilty husband to the innocent wife, and even transmits its taint to her offspring, should be suffered to rage unchecked because the Legislature refuses to take official cognizance of its existence, or proper sanitary measures for its repression.

"Infanticide is greatly multiplied, and a vast proportion of those whose reputations and lives have been blasted by one momentary sin are hurled into the abyss of habitual prostitution. A condition which is . . . in no other European country so hopelessly vicious or so irrevocable."

TABLE I.—WOOLWICH, ENGLAND: SYPHILIS IN-PATIENTS.

Year	Average daily strength of garrison	Admissions—actual number	Constantly sick—actual number in hospital daily	Ratio per 1,000	
				Admissions	Constantly sick
1904	5,311	331	56	62·32	10·70
1905	4,966	202	30	41·49	6·14
1906	5,096	129	14	15·12	2·72
1907	4,702	87	13	18·50	2·71
1908	5,666	53	6	8·75	1·28

1904: Grey oil intramuscular mercurial injections for in-patients and out-patients.

In 1905-08 I instituted mercurial inunctions for in-patients as a general rule in early stages, and reserved mercurial grey oil injections for out-patients.

TABLE II.—SYPHILIS: MALTA COMMAND.

Year	Average daily strength of garrison	Admissions—actual number	Constantly daily sick in hospital—actual numbers	Ratio per 1,000 admissions	Ratio per 1,000 constantly sick in hospital	Number of soldiers on Syphilis Register	Number of cases contracting syphilis in Malta
1908	6,030	112	14·02	18·6	2·33	158	89
1909	6,392	125	12·41	19·6	1·94	171	87
1910	6,769	83	9·21	12·3	1·36	160	35
1911	6,686	29	2·86	4·3	0·42	146	11

(1) Confidential medical notification of venereal diseases was instituted in October, 1909.

(2) In 1912, to May 31, only five cases of syphilis of very mild type (attenuated) contracted in Malta amongst 7,000 troops.

(3) Treatment in 1908 and to September, 1909, intramuscular injection of grey oil.

(4) In September, 1909, I instituted inunctions of ung. hydrarg., B.P., for in-patients in the early stages in ordinary cases and reserved grey oil injections for out-patients.

TABLE III.—INDIA: SYPHILIS.

Year	Strength	Admissions—ratio per 1,000	Constantly sick—ratio per 1,000	Invalids sent home	Deaths
1895	68,331	86·8	8·84	321	15
1896	70,484	97·7	10·47	448	16
1897	64,531	106·2	11·65	611	26
1898	65,397	88·2	9·51	547	20
1899	67,697	71·9	7·54	417	16
1900	60,553	62·5	6·59	344	15
1901	60,838	58·3	6·03	355	8
1902	60,540	49·9	4·85	286	24
1903	69,613	46·7	5·04	190	12
1904	70,413	49·5	5·45	175	18
1905	70,994	35·7	4·80	75	13
1906	70,193	27·7	3·55	120	11
1907	69,322	22·2	3·02	76	5
1908	68,522	15·8	2·37	59	3
1909	71,556	16·3	2·23	26	2
1910	72,491	14·5	2·01	18	1

(1) Control at the source (Cantonment Act) began October, 1897. The above figures deal with unquestionable syphilis.

(2) From 1895 to 1903 inclusive all venereal sores, whether "soft chancre" or early primary syphilis, were included under a heading *Primary* Syphilis. I do not include them, as soft chancre was not differentiated.

(3) Loss of service pay for venereal disease began in April, 1904.

(4) Loss of proficiency pay for venereal disease began October, 1906.

(5) From 1904 to 1910 syphilis and soft chancre are absolutely differentiated in British Army returns.

TABLE IV.—INDIA.

Admissions and Constantly Sick Ratio per 1,000 for "*all Venereal Diseases*" and *Alcoholism* contrasted.

Year	VENEREAL DISEASES		ALCOHOLISM	
	Admissions	Constantly sick	Admissions	Constantly sick
1898	303·5	27·97	3·3	0·12
1899	250·4	22·55	3·1	0·12
1900	231·3	21·19	3·7	0·14
1901	211·5	18·35	4·4	0·18
1902	209·6	17·60	4·4	0·16
1903	187·3	16·67	2·4	0·11
1904	200·4	17·63	3·1	0·14
1905	154·3	15·37	2·8	0·13
1906	117·4	12·32	2·4	0·11
1907	89·9	10·61	1·3	0·05
1908	69·8	8·85	1·0	0·04
1909	67·9	8·52	0·9	0·03
1910	58·9	7·79	0·5	0·02

(1) Control of venereal diseases at the source commenced October, 1897, Cantonment Act, India.

(2) Service pay lost for admission to hospital for venereal diseases or alcoholism from April 1, 1904.

(3) Proficiency pay lost for admission to hospital for venereal disease or alcoholism since October, 1906.

(4) Note the yearly reduction of venereal disease from 1898 to 1910. Alcoholism remained *constant* from 1896 to 1906. From 1907 to 1910 the reduction is mainly due to (2) and (3).

APPENDIX A.

In regard to systems of regulation of prostitution in foreign countries:—

At the International Meeting at Brussels in 1899, the question before the meeting was: Have the systems of regulation actually in force had any influence upon the frequency and dissemination of syphilis and venereal disease?

Dr. Barthélemy, one of the medical chiefs at St. Lazare Hospital, Paris, said: "It was impossible to apply inspection properly without registration. Free prostitution meant unrestricted syphilization of the people." He denied "that registration made women professional prostitutes, as they were so before. Disease was due to immorality, not to regulation; that passion was an eternal and imperative factor in human life, and that regulation was a means of combating its attendant evils. If it had not yet succeeded, that was not a reason for abolishing but for improving it." He recommended "gentler" methods.

I understand that the old prison hospital at St. Lazare, Paris, is to go, and a hospital built for diseased prostitutes.

Professor Fournier "would not trust to statistics, but appealed to common-sense. An infected prostitute was safe only when she was shut up." He stated that the opponents of regulation (abolitionists) minimized the appalling deformities and dangers of syphilis.

Professor Lassar (Berlin) agreed with Professor Fournier, and considered "that an infected prostitute was a focus of disease which it was the duty of Society to remove."

Professor Neisser (Breslau) shared Professor Fournier's opinions and was opposed to the views of the abolitionists.

The Italian doctors drew attention to the very serious increase of disease in Italy on the abolition of Cavour's system by Signor Crispi in 1888. The same thing occurred in India in 1885 when fifteen of the principal hospitals were closed as an experiment. The experiment being unsuccessful, the hospitals were re-opened until 1888, and then closed until 1897. During this period the increase of disease was appalling. Control again began in October, 1897.

Dr. Bortavelli (Milan) supported regulation.

Professor Oltramana (Geneva) said that the question had lately been put to the vote in Geneva, and regulation affirmed by a majority of two to one. He considered "that the success of regulation depended greatly upon the competence and carefulness of the physicians to whom the work was entrusted."

Professor Holst (Christiania) said that, since the abolition of regulation in Christiania, syphilis had increased 25 per cent., and clandestine prostitution had increased.

Professor Sturmer (St. Petersburg) supported regulation, and said "that unregistered prostitutes came to the dispensaries in a frightful state."

Professor Petersen said that regulation worked well in Russia, and moved "that supervision and inspection are of the greatest importance in preventing the spread of syphilis."

Major (now Colonel) Macpherson, R.A.M.C., produced a diagram to show the curve of venereal incidence in fourteen regulated and fourteen non-regulated towns in Great Britain. The results closely approximated before and after the period in which the Contagious Diseases Acts were in force, but during the period separated widely in consequence of the lowering of the rate in the regulated towns.

Dr. Kromayer (Halle) exhibited a number of tables to show "that the Contagious Diseases Acts in England had a distinctly beneficial effect upon the incidence of syphilis, but had not affected the frequency of soft sores or gonorrhœa."

A Royal Commission in 1870 reported on the working of the Contagious Diseases Acts of 1864, 1866, 1869, and expressed the opinion that the worst forms of the disease had been much reduced amongst the lower classes of prostitutes and that the women had also been benefited in an indirect manner.

"The Acts have purged the towns and encampments to which they have been applied of miserable creatures who were mere masses of rottenness and vehicles of disease." The report further says, "We are satisfied from the evidence that the frequent examination of prostitute women is the most efficacious means of controlling the disease." The Contagious Diseases Acts did not die a quick death in England, as suggested by Mr. McDonagh. They were in force from 1864 to 1886, and their repeal was mainly due to political reasons (*vide* "Cooper's Syphilis," 1895).

To reduce the number of prostitutes the abolitionists of regulation at the International Conference at Brussels in 1899 directed their efforts against immoral literature, alcohol, dancing rooms, and the stage. They recommended the increased care of neglected children, the provision of female inspectors of factories, more employment for women, and the notification of venereal disease.

Dr. Jullien, Paris, produced tables to prove that venereal disease occurred more commonly amongst unregistered prostitutes and that syphilis was most common in women between the ages of 17 and 22. The age of women is an important factor in contagion. Dr. Blaschko, Berlin, pointed out "that the unregistered women were the younger women who were always the most dangerous. It was not Regulation that rendered the courtesan less dangerous; it was Time."

Dr. Sperck, the great Russian specialist, found "that the amount of syphilis conveyed by the registered women was proportionate to the recruitment of their ranks by healthy women. These were soon infected themselves."

There was a strong consensus of opinion "that instead of placing the whole trade of prostitution under police control, they would put the entire administration on a purely medical footing and make the inspection itself an appendage and continuation of the hospital treatment. The action of police should only be admitted where patients failed to continue their attendance, or in the case of women denounced as centres of infection. The work of examination should be in the hands of venereal specialists. Early and effectual treatment was the goal to be aimed at. Two things are therefore essential—namely, improved medical education, to ensure a supply of competent doctors, and diffused information, that is, protection by knowledge."

There were eight resolutions made by the Conference; two of them appear to be essential as a foundation on which to build:—

First, that the Governments of each country should appoint a Commission charged to ascertain the prevalence, means of treatment, and prevention of venereal disease.

Secondly, that the statistics of disease should be drawn up in all countries on a common basis.

Major T. W. GIBBARD, R.A.M.C.: My remarks will be confined to the treatment of syphilis and especially to the use of salvarsan and neo-salvarsan. For nearly two years we have been investigating salvarsan at the Military Hospital, Rochester Row, with the object of ascertaining whether it was a drug which could usefully be introduced into the Army as a remedy for syphilis, and if so, the best method of proceeding so as to obtain the greatest total benefit from its use.

I will not waste your time by detailing the immediate effect of salvarsan on syphilis, because it has already been shown, in innumerable publications, that it acts much more rapidly than mercury, and that the rapid disappearance of *Spirochæta pallida* from local lesions, as well as the behaviour of the Wassermann reaction after its use, proves it to be specific and not a symptomatic remedy. I propose, instead, to devote some of the time at my disposal to a comparison between the subsequent progress of as many of our salvarsan cases as we have been able to follow up with that of a number of patients who were treated exclusively with mercury, in order to demonstrate to you my reasons for recommending the routine use of salvarsan in the Army.

We are particularly well situated for following up our cases of syphilis in the Army. Every soldier who contracts this disease is registered and a special case-sheet, of which I show you a sample, is made out for him. To whatever station he is transferred this syphilis case-sheet accompanies him and is there kept by the medical officer who is charged with the treatment of venereal disease in the station. The soldier is examined at regular intervals and his progress, with any treatment administered, is recorded on the case-sheet. I have taken advantage of this system to follow up the cases of syphilis we have treated with salvarsan. Many of our patients have been transferred to other stations, but every month I have addressed an inquiry to the medical officers in charge, in the form I now show you, as to clinical relapses, and especially as to subsequent cranial nerve disturbances, while every three months I have requested that a sample of blood serum be sent to Rochester Row for the Wassermann test. Progress of such cases as have remained in the London District has been carefully watched at Rochester Row. We have taken the greatest pains to exclude any bias in favour of salvarsan, and I have no reason to suppose that the medical officers who have kindly furnished reports on our patients in other stations have acted otherwise. Many of our patients were unfortunately transferred to the Army Reserve soon after the treatment and were not under

observation long enough to be used in this comparison. A large number also had previously been treated with mercury and must be excluded because most of them were especially severe cases in which mercury had failed to prevent frequent relapses, so they were not average cases. A sufficient number of patients who had received no previous treatment remain, however, to institute a fair comparison.

The particulars regarding the clinical behaviour of the exclusively mercurial cases whose progress I propose to compare with that of our salvarsan cases were obtained from a number of syphilis case-sheets relating to patients who received not less than nine weekly injections of mercurial cream in the first course, then rested for not more than eight weeks, had six mercurial injections in the second course, and so on—cases, in fact, which had been treated thoroughly with mercury. Except for this stipulation, the syphilis case-sheets of these mercurial cases were chosen at random.

The information regarding the Wassermann reactions given by patients treated with mercury was supplied by Major L. W. Harrison, R.A.M.C., from the results of tests he has carried out on the sera of patients who have been treated at Rochester Row, Aldershot, Bulford, and Woolwich.

The clinical results we have recorded up to date have been collected for me by Lieutenant A. S. Cane, R.A.M.C., and are as follows: Excluding 10 cases which were treated with one subcutaneous or intramuscular injection of salvarsan, 65 fresh cases of syphilis were treated, 49 with one or more intravenous injections of salvarsan, and 16 with an initial intravenous injection of salvarsan, then nine weekly injections of mercurial cream, and, lastly, an intravenous injection of salvarsan. These 65 patients were observed for periods ranging from twelve to twenty-one months, and nine relapsed. Forty other cases were treated with salvarsan and observed for periods ranging from nine to twelve months; 6 of these relapsed. Fifty-seven other cases were treated, 5 with one or more intravenous injections of salvarsan only, and 52 with an initial intravenous injection of salvarsan, nine weekly injections of mercurial cream, and lastly an intravenous injection of salvarsan. These 57 patients were observed for periods ranging from six to nine months and none relapsed. Altogether, therefore, out of 162 patients who were treated from the outset with intravenous injections of salvarsan, either alone or in conjunction with mercury in the way I have mentioned, and were subsequently observed for periods ranging from six to twenty-one months, 11

relapsed. The average period during which the relapse cases had remained free from active signs of syphilis was seven months. In comparison with this, out of 102 patients who were thoroughly treated with mercurial injections and observed for six to twelve months, 85 relapsed; the average period during which the relapse cases remained free from symptoms being 4·2 months.

Twenty-three cases of primary syphilis were treated with intravenous injections of salvarsan, and observed for twelve to twenty-one months; none of them showed secondary symptoms. Out of 10 others who were similarly treated and observed for nine to twelve months, 2 developed secondaries, while out of 23 others observed for six to nine months none developed secondaries. Altogether, therefore, out of 56 primary cases treated with salvarsan, and observed for six to twenty-one months, 2 subsequently developed secondary signs, the average interval between commencement of treatment and commencement of secondaries in these two cases being six months. I may mention that the *Spirochæta pallida* was demonstrated in each of these 56 cases previous to the commencement of the treatment. In comparison with this, out of 23 primary cases treated exclusively with mercury, 21 developed secondaries within an average period of 1·8 months.

Roughly, therefore, the incidence of clinical relapses within six months was twelve times as great in the exclusively mercurial cases as in the salvarsan, and the incidence of secondary symptoms when treatment commenced in the primary stage was twenty-four times as great in the purely mercurial series as in the salvarsan.

Good as these clinical results of salvarsan treatment are, I think that better will be shown in future, because many of our patients received one or two intravenous injections and no other treatment, and we expect better results from the intravenous injection of salvarsan in conjunction with mercurial injections. Out of 93 patients treated in this way and observed for six to twenty-one months, 3 only have relapsed.

As regards the Wassermann reaction, Major Harrison's latest results show that out of 83 cases treated from the outset with salvarsan, and tested seven to fifteen months after suspension of treatment, 15, or 16·8 per cent., were found to be positive to the original test, which in his hands gives 96·1 per cent. of positives with untreated secondary cases. One hundred and three cases tested four to seven months after suspension of salvarsan treatment gave 18 positives, or 17·4 per cent. It is impossible to compare these results with those obtained after

exclusively mercurial treatment in a manner which is fair to salvarsan, because there has been no opportunity of testing the sera of the mercurial cases at such long periods after suspension of treatment. It is obviously unfair to compare them, as a distinguished critic of salvarsan has done, with results obtained immediately on the termination of the fourth, or any other course of mercurial injections. The longest period during which mercury is suspended in the first two years of a soldier's treatment is after the fourth course, when as a rule a rest of four to six months is allowed. Forty-two sera were tested at the end of this period and 24, or 57·1 per cent., were positive. Out of 289 sera which were tested only three months after the termination of two years' regular treatment no fewer than 123, or 42·5 per cent., were found to be positive to the original test.

Judging by the Wassermann test, therefore, when salvarsan is used either alone or in conjunction with nine mercurial cream injections, the result is better than after two years' exclusively mercurial treatment by regular intramuscular injections.

We have had 5 cases of reinfection. In each of them the sore was on a fresh site, and occurred within the incubation period of a primary sore from the date of exposure to infection. They may possibly have been chancriform gummata, as Mr. Marshall suggests, but it is not our experience to find so many *Spirochætæ pallidæ* in gummata as were found in each of these fresh sores.

As regards safety, we have given 43 intramuscular or subcutaneous injections, and 1,435 intravenous injections of salvarsan, some of them to patients who were very debilitated with syphilis, but no untoward incident has occurred. In connexion with the subject of death after salvarsan, I should like to mention, however, that a few weeks ago, a policeman suffering from secondary syphilis was admitted to Rochester Row, and subsequently transferred to St. George's Hospital, where he died of large white kidney. It is quite possible that if we had not examined him carefully, in accordance with our usual practice, we should have given him salvarsan, and this remedy would have been blamed for his death.

We have had no cases of cranial nerve disturbance after any of our injections. One of our patients developed right-sided hemiparesis four months after an intravenous injection of salvarsan, but I considered that his symptoms were due to syphilis, and not salvarsan. He was therefore given further calomel injections and salvarsan, and recovered in two weeks. Another patient (a medical man) came to us six months after a subcutaneous injection of salvarsan, which he had received in India.

He was suffering from facial paralysis, which I considered to be due to exposure while motoring. As some of the salvarsan injected in India had just been removed by incision, and I did not think that the original course of treatment had been adequate, I recommended another course of salvarsan. Three days after the first intravenous injection he wrote to me to say that the paralysis had completely disappeared.

For the reasons I have mentioned I would recommend the routine use of salvarsan in the treatment of syphilis, and would withhold it only in the presence of those contra-indications which have been insisted upon by Ehrlich and others who are qualified by their experience of salvarsan to speak on the subject. At the same time, I hold that salvarsan should be administered only by those who are fully conversant with the details of preparing and injecting it. I may add that we are now training a class of medical officers at Rochester Row in the necessary technique, and I understand that salvarsan will not be issued officially to any military hospital till there is a medical officer there who is fully acquainted with this technique.

I consider the early recognition of syphilis of the very greatest importance to the success of salvarsan treatment, and may say that every medical officer is taught both on entering the Service and subsequently when undergoing his Captain's promotion course, how to use the dark-ground apparatus, how to prepare a specimen to send through the post for dark-ground examination, and how to obtain a specimen for the Wassermann test.

I have never thought that a single course of salvarsan injections is sufficient to guarantee a cure. On the contrary, I hold most strongly that subsequent examination cannot at present be dispensed with, and that this examination must be serological as well as clinical. I cannot say what test we shall eventually accept as an indication that the patient is cured, but I think it probable that we shall adopt the provocative injection recommended by Professor Ehrlich and Gennerich, and now advocated by Mr. McDonagh.

We have so far obtained the best results with one intravenous injection of salvarsan followed by nine weekly injections of mercurial cream, and lastly an injection of salvarsan, but are at present building up a series of cases to whom we are administering three fortnightly injections of salvarsan, and during the same month four intramuscular injections of calomel ($\frac{3}{4}$ gr.). It is much too early to compare the results of this procedure with those of other plans we have tried.

Professor Ehrlich very kindly sent me a generous supply of neo-salvarsan, and we are investigating it on the same lines we adopted with

salvarsan, but I cannot say how it will eventually compare with the older remedy. Its immediate effect is certainly as rapid as that of salvarsan, and as it is ready for administration when dissolved in fresh distilled water at room temperature, it is much more convenient to use.

As regards reaction, such as rise of temperature, vomiting, &c., we recently compared salvarsan with neo-salvarsan in this respect. Sixty-two patients were injected with salvarsan and sixty-two with neo-salvarsan. Out of these, in eleven salvarsan and five neo-salvarsan cases there was a temperature over 100° F.; in eleven salvarsan and three neo-salvarsan cases vomiting occurred. It is hardly necessary to say that we invariably use freshly distilled water in the preparation of these remedies.

It is possible to give neo-salvarsan in larger doses than salvarsan, and the dose can safely be repeated at shorter intervals. Major Beveridge, R.A.M.C., has very kindly investigated the question of the excretion of neo-salvarsan for me, and has so far not been able to detect arsenic in the urine later than the third day after an intravenous injection of neo-salvarsan. This probably explains why it can be repeated so soon. With regard to the repetition of neo-salvarsan, Dr. Schreiber in a recent letter to me mentioned that he was not now inclined to administer maximum doses of neo-salvarsan more frequently than every eight days except in the case of very powerful patients. This advice is repeated by Professor Ehrlich in his latest circular in which he recommends that the maximum dose should be 1 grm. to men, the interval between injection of such doses, six or eight days, and the total in one course, not more than 5 to 6 grm.

Sir GEORGE SAVAGE: I fear I have nothing new to contribute to this discussion beyond my personal experience. I therefore hesitated before consenting to take part, but perhaps it is fitting that as a senior in my branch of medicine I should in some small way contribute to this most important comparison of the experience of many men. My part is to trace the relationship which may be found to exist between syphilis and mental disorder. This may be considered from several points: The effect produced by the acute poisoning, by the moral effect, and by the chronic degenerative changes produced by constitutional disease; it must also be considered in its acquired and its inherited forms. I recognize and accept the work of Mott as representing my belief in the material pathology. My old teacher Wilks was the first to recognize that syphilis attacked different persons in different ways, and that there was a visceral syphilis. He pointed out that the persons who suffered from the tertiary or constitutional

disease often had escaped the earlier symptoms. Wilks was anxious to know whether certain poisons affected the nervous systems of those who were neurotic by heredity, and at his request I made some investigations at the fever hospitals as to whether those patients who were by heredity neurotic were more delirious during fevers than the others. I failed to get evidence to support this idea. Mott has, however, shown that in alcohol those who suffer in their viscera do not suffer in an equal degree in their brains, and that the alcoholics in an asylum are generally free from visceral degeneration. There is therefore this interesting parallel between the alcoholic and syphilitic person. Another point of interest is that the general paralytics in asylums are not so neurotic by heredity as are the rest of the insane patients. My experience has been that whereas 30 per cent. of the certified lunatics have a clear hereditary taint, not more than 10 per cent. of the general paralytics come of insane stock.

The effect on the nervous system seems only slightly to depend on the treatment. I have seen all the forms of nervous disorder which depended on syphilis in cases which have been thoroughly treated with mercury in the earliest stages and with mercury and iodides for later complications. Whether the latest forms of treatment will prevent severe constitutional symptoms remains to be seen. If Wassermann's reaction is infallible, then I have met with instances in which all history of syphilis was denied but where symptoms indicated the disease. Syphilis rarely acts as the only cause of mental disorder, alcoholic excess, strain, or injury being commonly associated causes. In Tuke's "Dictionary of Psychological Medicine" I have published a table of the relationships which may exist between syphilis and insanity. I modify this as follows: (1) Insane dread of syphilis; (2) insane obsession as to the results of an attack of syphilis; (3) delirium and delirious mania following syphilitic fever; (4) mental decay or premature dementia following constitutional syphilis; (5) syphilitic neuritis, such as optic neuritis, leading to delusional insanity with ideas of persecution or suspicion; (6) syphilitic disfigurement giving rise to ideas of being noticed, jeered at, or suspected as being sources of a general epidemic of syphilis; (7) inherited syphilis with sensory defect and retarded or defective mental development; (8) inherited syphilis leading to idiocy or epilepsy; (9) inherited syphilis causing juvenile general paralysis; (10) constitutional syphilis causing the general paralysis; (11) constitutional syphilis causing locomotor ataxy with mental symptoms; (12) constitutional syphilis leading to senile dementia, with or without paralysis, hemiplegia, aphasia, and the like. In this plan may be seen

the various mental and physical disorders which are met with in asylums as the result of syphilis. I shall not elaborate the cases, but it is important to recognize at least three groups. First, the very rare one in which syphilitic fever gives rise to mania of a delirious type. Such cases must be looked at as parallel to the cases of mental disorder following any specific toxic agent and must be treated as such, more attention being paid to the febrile state than to the specific causes. Next there are the cases in which the dread of syphilis or its results overpower all reason, so that either syphilophobia results or ideas arise that the results of the disease make the patient a social danger or one conspicuous and to be avoided. Last, the great and varied group of cases depending on structural degeneration, always vascular in part.

And now I pass to the more special point of interest—namely, my belief in relationship to the connexion between syphilis and general paralysis. In my earliest lectures in the seventies I distinctly said I could trace no relationship between the diseases; a similar opinion was held by Clouston, of Edinburgh; since then we have both changed our views. In my case the process of education was as follows: I met with cases in which there were ptosis, external strabismus and mydriasis, and gummata were diagnosed; in some cases I followed up the cases and made post-mortem examinations, only to find arterial degeneration, but no evidence of any gummata. In some such cases recovery took place, but in others the recovery was only apparent, the patient reappearing as a typical case of general paralysis of the insane. Then I saw several medical men whose history I knew, as far as syphilis was concerned, and after many years they developed general paralysis of the insane. These facts naturally impressed me so that I took greater pains in investigating the previous histories of the general paralytics. I soon found that the great majority admitted slight venereal sores many years before, and now I am convinced that nearly every case of general paralysis of the insane has had syphilis. I own to the difficulty which is met in the semi-savage tribes into whom syphilis has been introduced, and who almost all suffer from the disease, and yet among whom very few are general paralytics. I believe syphilis to be the dominating cause, but that alcohol, prolonged nervous strain or injury, may be the contributing or exciting causes.

A very practical question arises in insurance cases. Can general paralysis of the insane arise as the result of injury alone? A brain whose general nutrition has been affected by alcoholic excess may rapidly degenerate and lead to dementia after a head injury, and I believe the same may be said of the syphilitic brain. Rapid mental

deterioration may follow from a direct external injury or from an internal one, due to a so-called epileptic or congestive attack. The enormous number of general paralytics admitted to asylums gives evidence of the great importance of studying the various relationships existing between the diseases with the hope that, attacked at its root, one may sooner or later be able to counteract or cure it. It is certain that some strains of the syphilitic poison are more virulent than others, for whereas but a very small number of those infected with syphilis develop general paralysis of the insane, there are instances of women who not only give rise to syphilis, but in a large proportion these sufferers also later have general paralysis of the insane. Besides true general paralysis of the insane there are many cases of patients who, having suffered from syphilis, exhibit well-marked evidence that there is brain disorder which leads to mental weakness but does not produce general paralysis of the insane. These cases of parasyphilitic brain disease have been variously named, some being called pseudo-general paralytics. As a rule their one constant symptom is progressive mental weakness, resembling in many ways the mental disorder which arises from progressive arterial degeneration. It interested both Gull and Sutton to consider if with progressive arterio-capillary fibrosis changes there were to be met any definite mental disorders. They, as far as I recollect, could not detect any regular evidence of special mental trouble in cases such as those of Bright's disease. This has led me to insist on the special form of decay which is met with in general paralysis of the insane which is not confined simply to vascular changes.

A rather interesting feature of the special nature of the disease occurs in its relationship to memory. The degeneration due to alcoholic excess is marked by loss of memory, whereas in general paralysis of the insane very often the memory will last far into the second stage of the disease. In the pseudo-general paralytics I think the memory fails more than in the true cases. Allied to these cases I have been struck with what have been described as cured cases of general paralysis of the insane, or of cases in which the disease has become chronic or arrested.

I have seen a good many cases in which the history of syphilis was clear, then has followed some local intracranial nerve trouble, which has passed away with treatment, yet the patient has never become normal. He may remain chronically restless, or he may be merely a mental cripple, and in such a state he may live on for many years. I have seen a good many instances of men who, having contracted syphilis after middle age have developed some of the restlessness and exaltation met with commonly in general paralysis of the insane, and yet have

only passed in the end to senility. I used to call these cases of senile general paralysis of the insane, and now I rather look upon them as cases of accelerated senile decay.

I shall not enlarge on syphilitic epilepsy, as my experience of this is limited, but I have seen a certain number of patients in whom, with epilepsy developing in mature life there has been a distinct syphilitic history.

Having now considered the results of acquired syphilis I turn to inherited syphilis. First, I have met with cases of general paralysis of the insane in early manhood in cases in which I felt I could exclude the personal acquirement but in which there was a history of parental syphilis. Thus, these seem to form a connecting link between ordinary general paralysis of the insane and the juvenile variety. My experience bears out that of many observers that in juvenile general paralysis there is always a history of parental disease. In a certain number of children of syphilitic parentage I have thought some of the cases of so-called meningitis which have ended fatally were really allied to the cases of juvenile general paralysis of the insane. I am not one who is fully convinced that nothing acquired by a parent can be transmitted to the children, and I have been astonished at some of the histories of children who were morally imbecile in whom vice had been the most marked trait in a parent. I know it is almost impossible in such cases to exclude the effect of surroundings. I have already spoken of children with well-marked evidence of inherited syphilis in whom some sensory defect was present, and in whom mental deficiency was present. Deafness, with disease of the middle ear alone or associated with some eye trouble have been present in several children who were morally defective. It is difficult to gauge fairly the effect of inherited syphilis in producing backward and feeble-minded children, but I have met with families of defectives in whom there was a clear history of parental syphilis. In some of these children there were some of the stigmata of inherited syphilis. I have known healthy children, however, who have been begotten by fathers who later have died of general paralysis of the insane. The danger to the offspring depends on the proximity of the begetting to the development of mental disorder. I have known a child begotten in the early excited stage of general paralysis who was idiotic, whereas children begotten by the same father years before were perfectly normal.

So far, then, I have given as my experience that inherited syphilis is the cause of juvenile general paralysis of the insane and may be the cause of moral, intellectual, or physical deformity or defect. Now, as

to idiocy. I was at first surprised to find that the experience of men like Dr. Caldecott, of Earlswood, and Dr. Turner, of Colchester, showed that syphilis seemed to play a very small part in the production of idiots. Not more than 1 or at the most 2 per cent. of the idiots have any specific history. It has struck me that the many miscarriages occurring with syphilis may have stopped some potential idiots. I cannot pass over the fact that I have come across some cases of what Clouston called postponed idiocy (what, in fact, may be dementia præcox) in which there has been inherited syphilis. In these cases there is commonly some neurotic heredity as well, so that I can say that in certain cases in which inherited syphilis is combined with neurotic heredity a tendency to early and premature mental decay may occur.

To sum up, then, syphilis may act as a direct acute poison, producing acute mania. It may lead to mental disorder without any traceable material brain change in syphilophobia and certain obsessional disorders. It may, by inheritance, give rise to juvenile general paralysis and some forms of mental and moral weakness. It may lead to very marked disease with progressive mental degeneration assuming a specific form in general paralysis of the insane, or a less special form in pseudo-general paralysis or progressive dementia. In my belief there is not only arterial disease but some particular disease in general paralysis of the insane affecting the nerve elements, and at present no anti-syphilitic treatment is of any avail when true brain syphilis is established.

Dr. DOUGLAS WHITE: I propose to utilize the short time at my disposal in directing your attention to the social aspect of syphilis. In this aspect it cannot be wholly isolated from other forms of venereal disease, which, equally with it, form a standing menace to our civilization, both in this and other countries.

In a former paper, Colonel Melville and I attempted to analyse the present state of affairs in this country; to show the extent of the prevalence of venereal disease, and to sketch the principles and methods by which it should be combated. While there are valid moral objections to any form of State regulation of vice, such as prevails in most Continental countries, yet the final condemnation of such methods, in the eyes of practical men, lies in its total practical failure to lessen the inroads of the disease which it seeks to prevent. (There can be no doubt that, although in the French and specially in the German Army, such diseases are less prevalent than in our Army, yet in the general civil population the prevalence is greater there than here.) The main causes of this failure are plain. Just as any regulative system becomes

severe and relatively efficient, in just such a proportion vice tends to avoid regulation and becomes clandestine; so for instance, in Paris, there are now only forty regulated houses, as compared with 200 forty years ago; in the meantime clandestine vice has more than doubled in volume. Besides this, the most dangerous age from the point of view of disease occurs at 18 in women, at which age very few, if any, women come within the purview of regulation.

We arrived at the conclusion that no method was available for the elimination of such diseases but to offer extended facilities for treatment at the general hospitals and dispensaries throughout the country. I lay stress on the general hospitals, because special or lock hospitals will not meet the requirements; the average patient who acquires these diseases does not wish to wear a label which all may see.

At the present time it can hardly be said that serious attempts are being made to deal efficiently with the mass of venereal disease at the general hospitals. Its vast importance would justify the raising of such patients to a position of special consideration at the hospitals, as is now done in the Army, to the no small advantage of both the patients and the medical staff; but in the hospitals these diseases occupy the place of dishonour rather than that of special attention. I do not mean that no attempt is being made to cope with them, but the effort is not concentrated and systematized. Nor can it be denied that much is left to be desired as regards the teaching of the students. Here also more systematic education is required, and that more particularly with regard to practical treatment, the administration of injections and various forms of special medications. Specialization should be encouraged, and posts created for specially qualified persons.

The advent of Erhlich's specific not only further justifies, but urgently demands, a radical reconstruction of our whole hospital outlook on syphilis; for by means of this, if we could get hold of all cases of primary and secondary syphilis, the disease, at least *as a contagious disease*, could be eliminated in a very short space of time. This, it may be observed, holds true even if we adopt the less optimistic views on the efficacy of salvarsan.

By some people the present conditions are regarded with some degree of complacency, because there is some evidence that syphilis has somewhat decreased in the general populace in the last half-century; this in turn gives rise to the hope that the disease, if left alone, will gradually eliminate itself. I believe such complacency to be without justification, and I wish to place the facts shortly before you, as they are found in the official returns.

The returns of *infants* dead from syphilis under one year do show a considerable diminution—about 33 per cent. in the last twenty-five years. This is so far satisfactory; it is very probable, indeed, that syphilis in infants was over-diagnosed in the early days; but this would hardly account for the decrease. (It is noteworthy that of infants who die of syphilis, the males preponderate by 5 to 4 over females. This, however, is only in consonance with the general fact that in the first year of life males die off more than females, and in the same proportion. There are 4 per cent. more males born than females: by the end of the first year of life this male preponderance is eliminated. In this process syphilis takes only its proportional share.) At the present time 1,200 infants under one die of syphilis annually in England and Wales.

The death-returns for the *total* population show a somewhat similar result; but for our purpose this return is most misleading. First it includes the infants, which form three-fourths of this whole return. Secondly, it does not include tabes, general paralysis of the insane, aneurysm, or other diseases than tertiary visceral deposits. The number of adult deaths thus included amount to 450 a year. This small number is no doubt tending to diminish.

But in the past decade there has been an increase in the number of deaths due to aneurysm, tabes, and general paralysis. From aneurysm more than 1,000 die annually, from tabes about 600, and from general paralysis of the insane about 2,400. This makes 4,000 in all from these three complaints, while the official returns from adult syphilis only show 450. If now, all these are taken together, we find that, for the decennial period, adult deaths from syphilis have increased in number, and this increase has just about kept pace with the increase in population.

If the figures for aneurysm are to be trusted as indicative of previous syphilis, they are still more startling, for the death-rate per million living from this cause has been absolutely steady for thirty years past. Now an adult death from tertiary or quaternary causes represents syphilis contracted on an average fifteen years previously. So from the last decennial return the obvious inference is that there was no diminution in the incidence of syphilis during the years 1885-95.

(Taking the adult deaths from syphilis as a whole, the proportion of the sexes is almost exactly three men to one woman, and this I believe to represent pretty accurately the proportional incidence of syphilis among men and women.)

As against the above inference that syphilis in adults is not decreasing, we have the returns which show the number of recruits rejected per annum for syphilis. These show a pronounced diminution. They

are now only one-fifth of what they were twenty-five years ago, and one-tenth of what they were fifty years ago. It is difficult to believe that these figures do not indicate some true decrease in the general incidence of syphilis. They are taken at a time of life when the incidence of syphilis among men is at or near its maximum (19-20), and the class from which recruits are drawn is neither too high nor too low in the social scale to be reckoned as typical. Two considerations, however, will tend to lower the significance of these figures. First, syphilis was almost certainly greatly over-diagnosed in the older days, recruits being often rejected on account of skin affections which were then, but are not now, regardly as syphilitic. Secondly, recruits have gradually learnt that it is useless to present themselves for enlistment while suffering from obvious disease.

How far these points tend to discount the significance of the figures it is impossible to say. On the whole, considering the conflict of evidence, we must admit that we do not know whether syphilis is decreasing in the civil population or not; anyhow, there is no ground for an easy optimism.

The severity of the complaint, specially of its earlier stages, has declined in the last half-century. I well remember how an eminent surgeon, examining me on the subject, laid his hand on my shoulder and said, "My boy, you don't see syphilis now as we saw it forty years ago." Yet it may be doubted whether its social and economic effects have decreased in a similar ratio; nay, the opposite may be the case; for its ultimate disastrous effects to the community are as bad as ever, while its primary and secondary manifestations are less alarming to the individual and call with less urgency for the immediate treatment which they equally require.

Now as to the present incidence. By the use of Blaschko's figures for Berlin, combined with the recruiting figures for both countries, I concluded that London probably has a total annual incidence in venereal diseases of not less than 200,000. Of this total we should be about right in putting 40,000 to the credit of syphilis. Considering the relation which London holds to the rest of the country in the Registrar-General's returns of deaths from syphilis, I do not think we shall be far out if we say that in the whole of Great Britain and Ireland the annual incidence of syphilis is about 130,000. If this is even approximately true, then the matter is worthy of the immediate attention of the profession. What are we to do?

If all fresh cases of syphilis are to be treated with salvarsan, as they ought to be, then probably every bed devoted to this purpose could

accommodate 100 persons in the year. This would mean 400 beds for the purpose, distributed over London. For so important a purpose this sort of demand ought to be easily met. In the past, it is true, difficulty has been experienced in the practical working of wards for venereal patients; perhaps this might be avoided by partitioning such wards into cubicles, which would be both pleasanter for the better class of patients, and also inhibitive to the more aggressive sort. The period of stay also in the hospital would be too short for the development of a disorderly spirit. The first need, therefore, is for increased hospital facilities.

If, however, any such effort is to be a success, it is necessary also that the public should be educated to the dangers of venereal disease. The daily Press vigorously prosecutes campaigns against tubercle and cancer, of the method of propagation and cure of which we are comparatively ignorant. Of syphilis we know both how it is propagated and how it may be cured. Its importance to the community I believe to be greater than either of the others. The Press could do a great deal in assistance of such a campaign. But more than that, we need systematic teaching of young adults. Lecturers ought to be sent round to teach the students at secondary schools, public schools and universities the unmixed advantages of chastity, and the dangers of promiscuity, in such a way as would conduce both to the raising of morality and the prevention of disease in the future parents of the race. By such means men and women would at least learn to come for cure on the first suspicion of disease.

If, then, as I urge, our true policy be simply that of attracting people by every means to their own cure, it follows that we must set our faces against anything which may savour of coercion. Any coercive detention would frighten away the very people whom we wish to attract; and as far as syphilis is concerned, coercive detention would be needless, since active infectivity is so rapidly destroyed by salvarsan.

Lastly, I would urge the necessity of obtaining statistics of venereal disease, in order to ascertain our present position and our future progress. Notification without names is the only practicable method; full notification would simply lead to concealment.

Whether it would be possible to obtain the necessary data without the assistance of the Government I cannot say. It would certainly be difficult to do thoroughly except through a Government department, but a start might be made by getting the hospitals to give annual returns.

I strongly suggest that our knowledge of this disease is far enough advanced, both as regards its cause and its cure, to justify the leaders of the profession in setting out on a vigorous endeavour to apply that knowledge to the inhibition of this continuous national scourge.

Mr. F. E. FREMANTLE remarked that most of the points he had intended to make and the evidence he wished to bring forward had been covered by the better facts which had been submitted by Dr. Douglas White and Major French, figures which at the present time could scarcely be beaten for their comprehensiveness. He hoped it was fully realized how indebted the profession was for the magnificent work on this subject by their colleagues in the Royal Army Medical Corps; the work they had done in this connexion stood out as a great example in preventive medicine, one from which, as in other departments of public health in the past, much could be learnt for use in the public health of civil life. With regard to statistics, Dr. Douglas White had already shown that the figures published were of very little value, and he (the speaker) would again emphasize the fact that out of the 1,685 deaths recorded in the Registrar-General's Returns as due to syphilis in 1909, 1,238, or over 75 per cent., were in persons under 20 years of age. It was obvious, therefore, from what was known by clinical medicine, that by far the greater number of deaths from syphilis which occurred over 20 years of age were not notified on the Register as such, and that the figure 1,685, as representing the deaths from syphilis in 1909, was very misleading. It was an important matter when thinking of the way the figures could be improved. To take another analogy, the Prussian Government in 1900 decided to endeavour to obtain more accurate and fuller statistics on the subject, and accordingly they issued questions to all medical men, but received replies from only 63 per cent., which gave a total of 40,902 patients; and yet by looking at the figures they concluded that there were probably half a million cases of first attack in Prussia in the year, which showed they had good evidence for believing that the return given was very small in proportion to the number of cases which they suspected to exist. It brought one back to what one often felt in connexion with public health, that the official returns were not to be relied on in certain cases, and that it was necessary to take a less definite but more comprehensive survey from the general opinion of the medical profession as a whole.

It had often been said that the question of disease must be separated from the moral question, and in considering causes that was certainly the case. But when considering means of prevention, the two questions must be brought together; for although syphilis was often conveyed in other ways, it was obvious that it was most frequently conveyed through the medium of prostitution, and prostitution must, therefore, be primarily attacked. There was a history available which went back to the earliest days, and it was of interest from the historical standpoint

to note that the Jewish Code, in Leviticus xv, contained an early, if not the earliest known, mention of venereal disease; and though presumably that disease was not syphilis the restrictions which applied to one applied also to the other. It is important to note that the distinctive feature of the Israelitic religious law was the discouragement of irregular sexual intercourse. It attempted to work against the tendency to such irregular intercourse by moral methods; and if there were time he would be able to show that throughout history the really effective control had always been by way of general moral influence, which had greatly reduced the amount of prostitution and other irregular intercourse. He did not suggest that any educative methods or means of exercising moral control would stamp out prostitution; but education of public opinion along this line must find a prominent place.

The absolute failure of all suppressive measures was well known, whether in the form of registration or of weekly medical inspection. It was well to tell those who sometimes suggested that those methods had not been properly carried out, that in Berlin, where the police system was very complete, they had absolutely failed, insomuch that when they were trying to suppress brothels the police nevertheless had to deal with 16,000 charges of *Kuppelei* every year. That was sufficient cause for putting such measures aside in this country, even though earnest people suggested a return to them. It was only forty-four years ago that the Royal Colleges of Physicians and Surgeons together brought forward evidence to the Government, in considerable volume, in favour of the extension of the Contagious Diseases Acts. But in 1882 there was a popular movement against them, and the Acts simply died of inanition, and were finally repealed in 1886. We had now, therefore, to return to the methods of attraction—to attract people to reveal themselves and be treated.

Was it possible to get any further forward in the way of notification? New York City had recently adopted a measure of notification, primarily as a result of the report of Dr. Hermann Biggs, General Medical Officer to the Department of Health in 1911. He represented the necessity of increasing the amount of accommodation for cases of venereal diseases; a vote for £11,000 was passed for making such provision, and hospital plans had been approved since February of the present year. Resolutions were at the same time adopted, requiring notification to the Department of Health. In addition to compulsory notification by officers of institutions, official and charitable, there was to be voluntary notification by all physicians, excluding the name and address of the patients. Still, both compulsory and voluntary notifications must

include particulars as to the age, sex, nationality, marital state, and as to the disease, the character, stage, and duration of the infection and, if possible, the date and source of infection. The promotion of voluntary notification was sought to be increased by the provision, for those who notified on a proper form, of free bacterial examination of discharges and the free provision of vaccines. And circulars were to be distributed on the subject. He asked those who proposed notification what they would gain by a system such as this. There was compulsory notification, but the majority of cases were left to be notified voluntarily. He believed the number so derived would have no known proportion to the actual number, and one would not know what provision to make for those whose cases were not voluntarily notified. It was of interest to consider the analogy of the notification of tubercle in this country. Provision had been made for the notification of tubercle, but those who moved about amongst general practitioners knew there were many doctors who refused to notify cases of tubercle. And surely that would be infinitely more the case with syphilis or other venereal disease? It was easy to see what attitude would be taken by a wealthy man of business desirous of consulting a medical man for syphilis, when his prosperity largely depended on his good reputation. Such a man might have indulged once, through no exceptional fault of his own, and would fear even the remote danger of the fact being known through his visit to the physician: surely he would do his utmost to prevent even anonymous notification, and would probably feel it safest to consult an unregistered practitioner. The result of such a measure must be to create fear in such people, and to some extent to drive practice in connexion with venereal disease into the hands of unqualified persons, or at least to those who would not notify.

He believed there would be a much more efficacious means by taking advantage of the working of the new Insurance Act, assuming that the matters in dispute between the profession and the Government were settled; especially as actuaries said that one-third of the population would be insured. It did not seem possible to suggest a method of securing treatment in hospitals for any large proportion of the population, because knowledge of the disease which was afflicting them deterred many from going to hospital for treatment. He therefore thought the great hope for ascertaining the amount of venereal disease extant and taking means for getting them treated was under the working of the Insurance Act. This should be tried for the next four or five years before attempting notification under the Public Health Acts.

The Royal Society of Medicine.

July 8, 1912.

Sir HENRY MORRIS, Bt., President, in the Chair.

A Discussion on Syphilis, with special reference to (*a*) its Prevalence and Intensity in the Past and at the Present Day; (*b*) its Relation to Public Health, including Congenital Syphilis; (*c*) the Treatment of the Disease.[1]

Dr. H. R. DEAN: The question of the importance of syphilis in the parents as a cause of mental deficiency in the offspring has led to the expression of very divergent opinions. The majority of authorities appear to have held that syphilis was a relatively unimportant factor. Others, from a study of cases which have come under their notice, have inclined to the view that syphilis was a very potent cause in the production of idiocy.

E. Mendel[2] (1868) quoted an interesting case described by Carl Friedrich Haase in 1828. A young woman who had been infected by her husband had three miscarriages. The fourth pregnancy resulted in the birth of a living child which died with marked hydrocephalus at the age of 7 months. After quoting the opinions of several writers, Mendel remarks that "clinical observations in this connexion are limited to a few cases, the number of which stands in marked contrast to the frequency with which pathological changes are found in the brains of children who succumb to hereditary syphilis. In any case the majority of hereditary syphilitics die long before there can be any question of mental deficiency. In those cases, however, in which hereditary syphilis is latent or cured, but which at a later period develop symptoms of

[1] Fourth meeting (adjourned from June 24).

[2] E. Mendel, *Arch. f. Psychiatr.*, Berl., 1868-9, i, p. 308.

mental disturbance, it is generally impossible to refer such disturbance to hereditary syphilis, for the objective symptoms of this disease are then variable, indefinite, and in no sense specific." Mendel, as may be seen from this extract, saw very clearly the difficulties which beset the proof of the view which he adopted.

It was not, however, generally considered that syphilis was a very important factor in the causation of idiocy. Critchett[1] in 1860 stated that "there does not appear to be any reason for supposing that actual idiocy is at all a common consequence of inherited syphilis. About three years ago we looked through the patients at the Redhill Asylum without finding a single one whose teeth were characteristic, and Dr. Down informs us that he has recently made another inspection with a like result."

Sir Jonathan Hutchinson (1863)[2] found an idiotic condition in only 3 out of 170 cases of congenital syphilis. The same observer (1879)[3] examined the teeth of the idiots at the Earlswood Asylum, but did not find the specific characteristics in any considerable number of cases.

Kerlin (1880)[4] made a careful inquiry into the histories of the parents and grandparents of 100 cases of idiocy. In only two cases was a history of syphilis elicited, while 4 only of the 100 patients showed some evidence of a syphilitic infection.

Judson Bury (1883)[5] described 6 cases of mental deficiency in children who were the subjects of congenital syphilis. He came to the conclusion "that hereditary syphilis is a more frequent factor in the production of mental deficiency than has hitherto been recognized."

Shuttleworth (1888)[6] examined the records of 1,000 cases at the Royal Albert Asylum at Lancaster. In 10 cases only was there reason for suspecting inherited syphilis, and in 4 cases only could the evidence be called satisfactory. Shuttleworth, however, expresses the following opinion: "In spite of these figures, I am inclined to believe that inherited syphilis plays a larger part in the production of mental enfeeblement in childhood than institution statistics would lead us to suppose."

[1] Critchett, *Med. Times*, 1860, i, p. 575.

[2] Hutchinson, "Diseases of the Ear consequent on Inherited Syphilis," 1863.

[3] Hutchinson and Judson Bury (Bucknell and Tuke), "Manual of Psychological Medicine," 1879, p. 151.

[4] Kerlin, "Enumeration, Classification, and Causation of Idiocy," Philad., 1880.

[5] Bury, *Brain*, 1883, vi, p. 44.

[6] Shuttleworth, *Amer. Journ. Insanity*, Utica, N.Y., 1888, xliv, p. 381.

A few cases were recorded by Fletcher Beach (1888),[1] who, however, concluded that "syphilis is not a common exciting cause of imbecility."

Down (Lettsomian Lectures 1887),[2] in discussing the causes of idiocy, states that in not more than 2 per cent. of idiots were there signs of inherited syphilis.

Piper (1893)[3] examined 316 idiots and found evidence of syphilis in 5 per cent.

Hahn (1898)[4] examined 540 idiots at the Alsterdorfer Asylum; of these 40 (7·4 per cent.) were found to be syphilitics. Of 100 deaf-mutes at the Hamburg Asylum 4, and of 60 blind children, 10 showed evidence of congenital syphilis.

Wachsmuth (1901)[5] was unable to find even one congenital syphilitic among 185 idiots whom he examined.

Ziehen (1908) records the result of a careful statistical inquiry as to the frequency of congenital syphilis in cases showing a slight degree of mental deficiency. Ziehen[6] found that syphilis was probably present in 17 per cent. and certainly present in 10 per cent. of the cases examined.

A very different result was recorded by Shuttleworth and Fletcher Beach (1910),[7] who could only trace inherited syphilis in 1·17 per cent. of their cases.

From this review of the subject it will be seen that a very wide divergence of opinion exists. It appears, however, to be quite certain that only a small proportion of idiots show the classical signs and symptoms of congenital syphilis, and if we are restricted to the usual methods of examination we must come to the conclusion that congenital syphilis has little connexion with idiocy. On the other hand, the view that congenital syphilis is an important cause of idiocy has been put forward by several writers. This opinion has been based for the most part on cases of idiocy in which, while the usual signs of syphilis have been wanting in the patient, it has been possible to establish a definite history of syphilis in the parent. The number of such recorded cases is, however, small.

[1] Fletcher Beach, *Amer. Journ. Insan.*, Utica, N.Y., 1888, xliv, p. 387.

[2] Down, *Brit. Med. Journ.*, 1887, i, p. 150.

[3] Piper, "Zur Aetiologie der Idiotie," Berl. 1893.

[4] Hahn, *Deutsch. med. Wochenschr.*, Leipz. u. Berl., 1898, xxiv, p. 262.

[5] Wachsmuth, *Arch. f. Psychiatr.*, Berl., 1901, xxxiv, p. 787.

[6] Ziehen, "Psychiatrie," Leipz., 1908, p. 613.

[7] Shuttleworth and Fletcher Beach, article "Idiocy and Imbecility," Allbutt and Rolleston's "System of Medicine," 1910, viii, p. 875.

The opportunity of collecting further evidence was afforded by the introduction of the Wassermann test. In the five years which have elapsed since the discovery of the Wassermann test it has been established beyond all reasonable doubt that a positive reaction may be regarded as conclusive evidence of a syphilitic infection. It is stated that positive reactions have been obtained with the sera of patients suffering from frambœsia, trypanosomiasis, leprosy, and in certain cases of malaria. These diseases are, however, of rare occurrence in this country, and we may safely conclude that a patient whose serum gives a positive Wassermann reaction has certainly been infected with syphilis. The Wassermann reaction is, moreover, a remarkably constant sign of syphilis, for it is well known that syphilitic patients during a latent stage of the disease, at a time when no sign or symptom of the disease is apparent, may give a definite reaction to the serum test. By the aid of the serum reaction we are able to detect syphilis in an individual who not only may show no sign of syphilis but may appear to be in perfect health. Still more remarkable are those cases in which a positive serum reaction is from first to last the only evidence of infection. It has been established that mothers of syphilitic children who appear to be in perfect health and who have never shown any sign of syphilis give in a large majority of cases a positive reaction to the serum test. The work of the last five years has established two very important facts :—

(1) A positive Wassermann reaction (if certain diseases, rare in this country, can be excluded) may be accepted as specific evidence of syphilis.

(2) A positive reaction may be obtained with the serum of an individual who shows no other evidence of syphilis.

If we are prepared to accept the truth of these two statements it is obvious that we have in the serum test an invaluable aid in determining the ætiology of a variety of morbid conditions.

The examination of the blood serum of idiots by the Wassermann reaction has been the subject of several papers :—

Raviart, Breton, Petit, Gayet and Cannae (1908)[1] examined 246 cases, of which 76 (a little more than 30 per cent.) were found to give a positive reaction.

Kellner, Clemenz, Brückner and Rautenberg (1909)[2] examined 216 cases, of which 13 gave a positive reaction by Stern's method, while

[1] Raviart, Breton, Petit, Gayet and Cannae, *Rev. de Méd.*, Par., 1909, xxviii, p. 840.

[2] Kellner, Clemenz, Brückner and Rautenberg, *Deutsch. med. Wochenschr.*, Leipz. u. Berl., 1909, xxxv, p. 1827; and Brückner, *Münch. med. Wochenschr.*, 1910, p. 1944.

9 only gave a positive reaction by the original Wassermann method. To the 13 cases must be added 3 cases which were deficient in complement and were found to be positive by the original Wassermann method, that is to say, a positive result was obtained in 7·4 per cent. It should be noted that of these 216 cases half were over 30, and 40 cases only were under 14 years of age. Of the patients under 14 years of age 20 per cent. gave a positive result, of those over 14 only 5 per cent.

Lippmann (1909)[1] examined 78 cases at the Uchtspring Asylum and obtained a positive reaction in 7 cases—that is to say, in 9 per cent. An examination of the cases at the Dalldorf Asylum gave a positive result in 13·2 per cent. Lippmann also examined 77 cases by clinical methods and decided that 40·2 per cent. showed signs of congenital syphilis.

In 1909[2] I had the opportunity of examining 330 of the inmates of the Wilhelmstift, an asylum for idiots at Potsdam. A positive result was obtained in 51 cases (15·4 per cent.). Among the 51 cases which gave a positive reaction, 7 were found which had definite signs of syphilis, and 3 or 4 in which syphilis might have been suspected but not with certainty diagnosed. There were 2 cases with definite signs which gave a negative reaction. That is to say, among the 330 patients were 9, or including doubtful cases 13, which from physical signs and symptoms would have justified the diagnosis of syphilis. The results in detail were as follows :—

	Cases examined	Positive
Simple idiocy of all grades	287 ...	44
Congenital spastic diplegia (Little's disease)	15 ...	1
Marked hydrocephalus	14 ...	4
Epilepsy	1 ...	1
Microcephalic cases	4 ...	0
Mongols	1 ...	0
Deaf and dumb	7 ...	1
Progressive muscular dystrophy, with mental symptoms	1 ...	0

All the cases which gave a positive serum reaction were subsequently very carefully examined with the object (1) of discovering any sign of syphilis which had been previously overlooked, and (2) of detecting any symptom or group of symptoms common to all the positive cases. Of the positive cases, one was subject to epileptiform convulsions and showed slight choreic movements, one had strabismus and nystagmus,

[1] Lippmann, *Münch. med. Wochenschr.*, 1909, lvi, p. 2416; *Deutsch. Zeitschr. f. Nervenheilk.*, Leipz., 1910, xxxix, p. 81.

[2] Dean, *Proc. Roy. Soc. Med.* 1910, iii (Neur. Sect.), p. 117.

one had a right-sided hemiplegia, one had spastic diplegia and conformed to the type of Little's disease, one was a deaf-mute, two were aphasic. Among the remaining cases I was unable to detect any evidence of a local lesion.

An examination was made of the cerebrospinal fluid from 12 cases which had given a positive serum reaction. In only one case was a positive reaction obtained. I also obtained for examination specimens of serum from the parents of 10 of the positive cases. The results were as follows:—

Patient	Age of patient	Result of examination of Father's serum	Result of examination of Mother's serum
B.	9	Positive	Positive
Har.	15	Not examined	Positive
Kr.	11	Positive	Positive
M.	11	Positive	Not examined
V.	13	Not examined	Positive
O.	11	Negative	Not examined
Sc.	16	Positive	Not examined
R.	12	Negative	Not examined
N.	9	Negative	Negative
He.	14	Not examined	Positive

Thus among 13 parents of children giving a positive reaction 9 were found to give a positive reaction. Six mothers were examined and 5 gave a positive reaction. Seven fathers were examined and in 4 cases a positive reaction was obtained. It will be noticed that in the above table in the case of the patient Har. a positive reaction was obtained at an interval of fifteen, and in the case of Sc. at an interval of sixteen years after the birth of a syphilitic child. The period during which a positive reaction may be obtained is known to be extremely variable in the case of the acquired form of the disease. In the congenital form it might be expected that the percentage of positive results would bear a close relation to the age of the patients examined. A grouping of the 330 cases according to age gives the following results:—

	Examined	Positive	Percentage of positive results
(1) Patients aged 10 and under (of these two only were less than five years old)	94	20	21·27
(2) Patients from 11 up to 15 years of age inclusive	142	24	16·9
(3) Patients from 16 to 20 years of age	66	4	6·06

Of patients aged from 21 to 30 years, 24 were examined, with 3 positive results. The remaining 8 patients ranged in age from 31 to 44 years, and all eight gave a negative reaction.

The above table appears to show that the percentage of positive results diminishes rapidly after the sixteenth year, and that a larger percentage of positive results might be expected from the examination

of a series of very young patients. In any case, the average age of the patients investigated must be regarded as an important factor in any estimation of the prevalence of congenital syphilis, and it seems to me possible that the very contradictory results already published may be reconciled by taking the age factor into consideration. Of the 51 cases in which a positive serum reaction was obtained, 7 only showed conclusive evidence of congenital syphilis from a clinical standpoint. In the remaining 44 cases a diagnosis of syphilis rested on the evidence of the serum test.

I had hoped that by a careful examination of those cases which had given a positive result to the serum test, it might be possible to detect some symptom or group of symptoms which was common to all. This I failed to do. Very few of the positive cases showed any evidence of a gross lesion in the central nervous system; and this, I think, is quite in accordance with what one might expect, for the gross changes in the brain which are known to be due to congenital syphilis are not compatible, as a rule, with a continuance of life. If a causal relationship exists between congenital syphilis and idiocy, the condition which arises may perhaps be classed as parasyphilitic. The absence of the ordinary signs of congenital syphilis in idiocy is closely paralleled in the already authenticated parasyphilitic diseases. It is, of course, well known that tabes and general paralysis commonly occur in patients where the early symptoms of syphilis have been mild, or even unnoticed. Among the cases of the juvenile form of general paralysis, collected by Mott (1909) [1] quite half were found to show no sign of congenital syphilis, but nevertheless to have been born of syphilitic parents and to have brothers and sisters who exhibited the ordinary signs of the disease.

It seems to me reasonable to think that many cases of idiocy should be classed with that form of syphilis which manifests itself alone by a selective toxic action on the elements of the central nervous system. I do not wish to attach an exaggerated importance to the results of the examination of the serum in one series of cases, but when it can be shown that a considerable percentage of idiots afford evidence of a syphilitic infection, and since it is well known that the virus of syphilis is capable of exercising a selective action on the central nervous system in cases in which there is no other evidence of the disease, I think it is not unreasonable to infer a causal relation between the two conditions.

Further evidence has been published by Atwood (1911)[2] who

[1] Mott, *Arch. of Neur. and Psychiatr.*, 1909, iv, pp. 13-23.

[2] Atwood, *Journ. Amer. Med. Assoc.*, Chicago, 1910, lv, p. 464.

examined 204 cases in America. A positive result was obtained by Noguchi's method in 30 cases (14·7 per cent.). The results of an examination of three cases which gave positive reactions were published by Bellingham Smith and Woodforde (1911),[1] and Chislett (1911)[2] examined 14 idiots, 8 of whom gave a positive and 6 a negative reaction. Chislett also records the results of an examination of an entire family. The father had general paralysis of the insane, and the Wassermann reaction was positive. The mother had no knowledge of the occurrence of any primary or secondary symptoms. She had had a tertiary ulcer on the left leg eight years after marriage. The eldest son, aged 16, was said to have been "very nervous and stupid at school." He showed no sign of syphilis, but gave a positive reaction. A girl, aged 12, was deaf in one ear, but otherwise normal; her serum gave a positive reaction. A girl, aged 10, gave a negative reaction, and appeared to be quite normal. A boy, aged 8, had rhinitis and conjunctivitis, and gave a positive reaction. The two youngest children, aged 6 and 4 respectively, were healthy and gave a negative reaction.

Knöpfelmacher and Schwalbe (1912)[3] examined 29 cases of hydrocephalus, 8 of which gave a positive reaction.

The results obtained in Denmark by Thomsen, Boas, Hjort and Leschly (1911)[4] were very different. These workers examined 2,061 feeble-minded persons, of whom only 31 (1·5 per cent.) gave a positive reaction. On the other hand, E. Krober (1911)[5] examined 262 idiots in the Hephata Asylum at Gladbach, and obtained a positive reaction in 21·4 per cent.

Although the results obtained by different workers in different countries are not in entire agreement, sufficient evidence has accumulated to make it appear extremely probable that congenital syphilis is an extremely important cause of mental deficiency. It must be remembered that the majority of idiots who give a positive Wassermann reaction show no other sign of syphilis. They are, in fact, cases of latent syphilis, and we are aware that, during the latent stages of the acquired disease only some 40 to 50 per cent. of the patients give a positive reaction. I am therefore inclined to think that the actual percentage of positive results obtained by examining a series of idiots

[1] Bellingham Smith and Woodforde, *Proc. Roy. Soc. Med.*, 1911, iv (Child. Sect.), p. 166.
[2] Chislett, *Journ. Ment. Sci.*, 1911, lvii, p. 499.
[3] Knöpfelmacher u. Schwalbe, *Zeitschr. f. Kinderheilk.*, 1912, iii, p. 428.
[4] Thomsen, Boas, Hjort u. Leschly, *Berl. klin. Wochenschr.*, 1911, xlviii, p. 891.
[5] E. Krober, *Med. Klin.*, Wien, 1911, vii, p. 1239.

by the serum test comes very far short of the number which are actually infected. If this assumption is correct we shall have to recognize syphilis as the causative factor in a very considerable percentage of cases of idiocy. The problem appears to me to be worth further effort, and an examination of another large series of cases could hardly fail to afford interest. Particularly valuable information would doubtless be forthcoming if it were found possible to make an examination of all the members of a family in which a case of idiocy had occurred. In any case, the results which I obtained in Berlin show that it is desirable to examine the blood serum at the earliest possible age. The low percentage of positive cases obtained by some workers must in all probability be attributed to the fact that a large number of adult patients have been included in their results. I am inclined to think that an examination of a series of very young patients will give a very high percentage of positive results. Additional and valuable evidence will no doubt be obtained if it is found possible to include an examination of the blood of the parents.

We may classify the cases in which congenital syphilis is the cause of disease of the nervous system into three groups:—

(1) The first group includes infants who are born with marked evidence of syphilis. In these cases the brain, together with other organs of the body, is the seat of marked change. These children, as a rule, die within a short time of their birth.

(2) This group includes children who appear to be healthy at birth but who develop mental defects at the time of the second dentition or at puberty. To this group belong the cases of juvenile general paralysis.

(3) In this group may be placed those cases of mental deficiency, imbecility and idiocy, in many of which the Wassermann reaction constitutes the sole evidence of a syphilitic origin.

It is well known that in many cases the syphilitic virus appears to exert a selective toxic action on the central nervous system, and we might expect this toxic action to exert its influence most injuriously during that period when the brain, the most highly specialized organ of the body, is undergoing development.

Syphilis is a widespread disease, and as former speakers in this debate have pointed out, there is no reason for supposing that its prevalence is on the wane. On the other hand, there is a very general belief that the type of the disease is changing. While the more obvious and gross manifestations of the disease are less often seen there is

reason to think that parasyphilitic disease is increasing. The connexion between syphilis and general paralysis of the insane has been established. Is it not reasonable to think that further research may establish a connexion between syphilis and other diseases, which at the present time are not suspected of a syphilitic origin? The remote effects of syphilis may be even more numerous than we suppose. Organs other than the brain and spinal cord may be the seat of parasyphilitic disease. The Wassermann reaction affords a method by which this problem can be attacked, but it is obvious that results obtained by the serum test must be subjected to rigorous criticism. The occurrence of a positive Wassermann reaction in any one patient is in itself no proof that the condition which it is desired to investigate has been caused by syphilis. The presence of a syphilitic infection in such a case may be a mere coincidence. It is only after a very large number of cases have been investigated that the coincidence of a certain group of symptoms with a positive Wassermann reaction can justify us in forming an opinion. Such an investigation must be carried out on a large scale, for the examination of a very large number of patients is a necessary step to obtaining the required statistics. A commencement has been made, and the results obtained by the examination of patients with aortic disease may be here quoted.

Donath (1909)[1] examined 27 cases of aortic insufficiency, aortitis and aneurysm of the aorta; a positive serum reaction was obtained in 85 per cent. of the cases.

Brückner and Galasesco (1910)[2] obtained a positive reaction in 17 of 22 cases of aortic regurgitation.

Longcope (1910)[3] obtained a positive reaction in 18 of 22 cases of aortic regurgitation.

These results are perhaps sufficient to show the possibilities of a systematic use of the serum test. The cases examined have, however, been too few for the results to be of real value, and in the majority of instances only those patients have been examined in whom a suspicion of syphilis was entertained.

An inquiry on broader lines is needed, and such an investigation should include the examination of cases in which syphilis is not suspected. By such means it might prove possible to enlarge our knowledge of the

[1] Donath, *Berl. klin. Wochenschr.*, 1909, xlvi, p. 2015.

[2] Brückner and Galesesco, *Compt. rend. Soc. de Biol.*, Par., 1910, lxviii, p. 74.

[3] Longcope, *Bull. Ayer Clin. Lab.*, Philad., No. 6, p. 60.

pathological changes produced by syphilis. An investigation on these lines has been undertaken by Churchill (1912)[1] who has examined 102 infants and children, patients at a children's hospital in America. A positive Wassermann reaction was obtained in no fewer than 39 cases.

As regards the treatment of congenital syphilis, it is obviously desirable that it should be commenced at the earliest possible moment. The serum test affords a means of early diagnosis and leads to the treatment of children in whom the disease is latent during the early years of life. Valuable information can without doubt be obtained by examining the blood of the mother during pregnancy.

Knöpfelmacher and Lehndorff (1910),[2] who have devoted particular attention to this subject, have examined 135 mothers of syphilitic children and have obtained a positive result in 65·2 per cent. Of these 135 mothers 31 only showed definite signs of syphilis, while in the remaining 104 a diagnosis was only possible as a result of the serum test.

Stroscher (1910)[3] obtained a positive result in 100 per cent. of the mothers of syphilitic children.

Mulzer and Michaelis (1910)[4] found that 96 per cent. of infants with manifest congenital syphilis gave a positive Wassermann reaction, while children with latent syphilis react like adults in the early latent period. They also found that 83 per cent. of mothers of syphilitic children gave a positive reaction.

In an earlier paper Knöpfelmacher and Lehndorff (1909)[5] reported two cases in which women after a series of syphilitic children gave birth to healthy children who in both cases were found to give a positive Wassermann reaction. Such cases must be regarded as cases of latent congenital syphilis, and it is in such cases that the use of the serum test should have a special value in leading to early treatment.

Bimfel (1909)[6] examined the blood of 230 mothers. Each case was examined at least twice during pregnancy and once during the puerperium. Among 21 cases in which there was a suspicion of syphilis a positive result was obtained in 13 and a negative result in 8 cases. Eleven of the women who reacted positively gave birth to three children

[1] Churchill, *Amer. Journ. Dis. Child.*, Chicago, 1912, iii, p. 363.

[2] Knöpfelmacher u. Lehndorff, *Jahrb. f. Kinderheilk.*, Berl., 1910, lxxi, p. 156.

[3] Stroscher, *Derm. Zeitschr.*, Wiesb., 1910, xvii, p. 485.

[4] Mulzer u. Michaelis, *Berl. klin. Wochenschr.*, 1910, p. 1402.

[5] Knöpfelmacher u. Lehndorff, *Med. Klin.*, Wien, 1909, v, p. 1506.

[6] Bimfel, *Wien. klin. Wochenschr.*, 1909, No. 36, p. 1230.

with symptoms of syphilis (serum reaction positive) and eight children without symptoms (serum reaction negative). Eight cases of secondary syphilis were examined, and a positive result both in mother and child obtained in 7. Of 9 cases of latent syphilis all gave a positive reaction; in six of the children a positive reaction was obtained, in two a partial reaction.

If it can be established that congenital syphilis is a frequent cause of idiocy it is reasonable to hope that very great success will follow the application of therapeutic and prophylactic measures.

We now possess in the Wassermann reaction a means of diagnosis which enables us to detect syphilis in cases in which it cannot be recognized by any other method. Wassermann has suggested that the serum test should be applied to every woman who is admitted to a lying-in hospital. When we consider the numerous cases in which syphilis is quite unsuspected we must admit the value of his suggestion. If a positive reaction was obtained, treatment of the mother would be commenced at once, and treatment of the child might begin from the earliest possible time after its birth. It can hardly be doubted that benefit would follow from the wholesale adoption of such measures.

Mr. JOHN H. DAUBER: I should like first to refer shortly to Mr. Jonathan Hutchinson's suggestion that Shakespeare was himself the subject of syphilis, because no previous speaker has touched on this subject, although few of us, I hope, will accept Mr. Hutchinson's view. The evidence he adduces is so slender as not to warrant, in my opinion, such a suggestion. That Shakespeare paints with a master hand a picture of syphilis signifies nothing, because in other plays he has depicted with equal force and accuracy certain mental diseases—e.g., Melancholia in "Hamlet," Senile Dementia in "King Lear," Monomania in "Macbeth," as Dr. Foster Palmer has so ably pointed out in his interesting monographs on these plays. Shakespeare did not himself suffer from all the vices or diseases he portrays. As to Sir William Davenant being the illegitimate son of Shakespeare, rumour is not proof. Many a man lays claim to a parentage he has no right to. As a general rule men do not stand as godfathers to their illegitimate children; they keep in the background. "King Lear" was a late play certainly, but the character of Cordelia was penned by no misogynist. The women of the late plays come out strong for good or evil. It has been said that specialists see their specialism in everything and everybody. It has been left to Mr. Hutchinson to see even in Shakespeare himself a case of his own speciality.

This afternoon has been set aside for the discussion of "Syphilis in its Relation to Public Health." I am venturing to speak in the hope of inducing the Society to share my view, or rather my strong conviction, that owing to the recent discoveries in the field of syphilis the time has arrived when we, as a profession, should once again take action, and ask the Legislature to assist us in our campaign against syphilis, by placing infected persons under some sort of direction, restraint and tutelage, in order to check the spread of the disease. Several of the previous speakers have expressed a similar opinion that, if we are to deal successfully with syphilis in the future, our hope lies more in the efficacy of preventive than curative measures. You will remember the following lines in Southey's poem on the "Battle of Blenheim":—

"'But what good came of it at last?'
Quoth little Peterkin.
'Why, that I cannot tell,' said he;
'But 'twas a famous victory.'"

It would be a great pity, in my opinion, if this discussion, now drawing to a close, should be, like the battle of Blenheim, barren of practical results instead of being converted into useful action. I venture to suggest that if our profession is content to employ its energies solely in the cure of individual cases, instead of adopting a strong line having for its objective nothing less than the prevention of the spread of syphilis and the eventual stamping out of the disease, it will make the greatest mistake and will not be doing justice either to itself or to the public.

In malarious countries medical men are no longer satisfied with treating individual cases of malaria; their main object is prevention, not cure, they try to get to the source of the disease, by rendering it as difficult as possible for the Anopheles to multiply by destroying its breeding places. In England syphilitic prostitutes wander free and unfettered. If we allow the seed of syphilis to be sown broadcast we must expect a plentiful crop of syphilitic disease.

The opinion is often expressed amongst the laity that the medical profession keeps its knowledge too much to itself and that it should instruct the public more than it does at present. It seems to me it is our bounden duty now to speak out boldly and insist that the public know the truth about this question. Then the responsibility will rest with them; at present it rests with us if we do not enlighten them. We can try to get public opinion on our side by saying to them: Here is a disease conveyed from one to another through any contact infection

by a definite micro-organism, highly contagious when introduced by any means into the blood, lymphatics, or tissues. With the exception of cancer it is the most dreaded disease under the sun, and in one respect it is worse than cancer, being transmitted direct from parent to offspring. It is a disease often most difficult to eradicate from the system when once acquired, sometimes needing years of prolonged and careful treatment. Elaborate laboratory tests are necessary at frequent intervals to determine whether the patient is cured. It is productive of the most widespread mischief when well established, sometimes never leaving the patient till it kills him after years of misery. In its later phases, when the nervous system is attacked, it is practically incurable. It often predisposes to cancer, but on the other hand it is, unlike cancer, a preventable disease.

In this country the Legislature generally moves in response to public opinion. Men will deliver themselves from any tyranny when they have learnt to hate it sufficiently. We must teach them so to hate the tyranny of syphilis that they will rid the country of it. Our forefathers in the Middle Ages stamped out leprosy. Future generations of men would for ever bless us if in our time we could succeed in stamping out syphilis. When men are in earnest most things are possible. Within the last few years we have ourselves seen that wise legislation has completely exterminated hydrophobia. The medical profession, unaided by the Legislature, could never have accomplished even this result. The State lends its assistance to us in the treatment of many diseases. The acutely infectious diseases come under its control. The treatment of insanity is regulated by the State, and it is now the earnest conviction of many that the State should assume some control over patients suffering from syphilis. Quite recently the Legislature has seen the importance of dealing with tuberculosis, and has made it compulsorily notifiable. It is a punishable offence now to expectorate promiscuously in public places—an action only, under certain circumstances, of danger to the public—and it is most noticeable how this objectionable habit has diminished everywhere as the result more of direction than compulsion. Yet syphilitic men or women may disseminate syphilis without the slightest check being placed by law upon their action. Germany has already taken a strong line in this direction of prevention. Persons may not marry in that country within ten years of acquiring syphilis, and within the next twelve months a law will come into force making it a criminal offence under heavy penalties for a syphilitic man or woman to knowingly infect a healthy person,

while in the German army and navy, as is well known to all of us, complete and ample instruction as to the prevention of venereal disease is systematically given, and every means taken by the authorities to arrest or limit its spread. In this country, at the present moment, there is an entire absence of all legislation on the subject, and our powers as medical men are therefore exceedingly limited. We can only deal with those individual cases which come before our notice. We can do nothing unless aided by the Legislature to check the spread of the disease.

At my hospital (the Hospital for Women, Soho Square) most of the cases of venereal disease that come before me are those of innocent women who have been infected by their husbands. These women, in my opinion, need some protection. When a woman marries she is aware that she runs certain risks at childbirth and during and after pregnancy, but it is not part of her bargain to run the risk of being infected by venereal disease. It may wreck her life or produce life-long disability. This should not be asked of her.

If syphilis were to become a notifiable disease some restraint could be placed upon the actions of syphilitic patients under legal penalties. It ought to be a felony for a person knowing him or herself to be syphilitic and capable of infecting another, to communicate the disease to an innocent person. It is a common saying that health is superior to wealth, but how different are the laws for the protection of health to those for the protection of property! "Salus populi suprema lex" was the old Roman maxim. May it in time become ours; it is not now. Syphilis might be banished altogether if the country were once convinced that it was an unnecessary scourge. If it determined to exterminate it, it could do so—

> "With caution judge of possibility:
> Things thought unlikely, e'en impossible
> Experience often shows us to be true."

Are we as a profession to be satisfied with our present limited powers? This is the question, to my mind, before us. At present we are somewhat like firemen called in to extinguish a conflagration, which as fast as we put it out in one direction is being continually rekindled by others in another, so that but little progress is made and the flames continue to spread. In my opinion we should apply to the Legislature to stop these incendiaries over whom at present we have no control, otherwise our work is almost useless. It seems to me that the medical profession will resemble Nero, who fiddled while Rome was burning,

if it contents itself with merely discussing the dosage of salvarsan, or the merits of a special hypodermic mercurial cream, while refusing to touch the great question of the prevention of the spread of the disease.

If the public dislike the words "venereal," "syphilitic," and so on, let us rechristen the disease altogether and start afresh. Are our hands to be tied in dealing with this disease because it usually first attacks the generative organs? The continuity of the race depends upon the integrity of these organs and they exercise an all-powerful influence over our lives. Certain sections of the community regard illicit sexual intercourse with more bitter hatred than they do many actual crimes and felonies. The puritanical party have such a horror of any sort of "Contagious Diseases Act," which they persist in considering as equivalent to the "State regulation of vice," that they would apparently rather see the innocent punished with the guilty than that the guilty should go unpunished. They consider that venereal disease is the appropriate punishment for sexual vice, that it is a great check upon sexual immorality, and that it would be the greatest mistake to remove it. The idea that illicit sexual coitus should be indulged in with impunity is repugnant to these people as contravening their own self-made ideas of justice. It is not uncommon for frail human beings to attempt to father upon Providence their own crude notions of morality, justice, and punishment, but if we are to arrogate to ourselves an intuitive knowledge of Divine ordinances, all scientific and social progress is impossible. We may just as well fold our hands and cease to endeavour to ameliorate the conditions of life. It is not by these methods that malaria, the plague, yellow fever, and other fell diseases have been combated. If we can induce these people to abandon all preconceived ideas and preformed judgments and look at this matter in an unbiased, practical and scientific manner, as if this world and our happiness in it were the "be-all and end-all" of all human activities, then we may perhaps convince them that their attitude is anti-social and detrimental to the common health.

The old Contagious Diseases Act failed because it dealt with one sex only. Too much power was given to the police, there was perhaps some petty tyranny, the Act fell into bad odour, and in London at least the results were felt to be disappointing; but that the Act was of great public utility in many of our seaport towns and abroad few will deny, and its repeal was strongly opposed by all who had seen its beneficial effects amongst our soldiers and sailors.

Most of us know the conditions that prevail abroad. Take Brussels, for example. Habitual prostitutes are compelled to become écartées;

they have a police licence which they must produce on demand to a police officer. This card shows when they were last medically examined, if they are healthy, and the district to which they must confine themselves. But as only the regular prostitutes are certified, and the disease is not notifiable, and the male sex is exempt from compulsory inspection, it is doubtful if Brussels is freer from syphilis than London.

I would urge that infected men must be under compulsory medical treatment, as well as infected women. Neither sex should have preference or privilege. The Suffragettes might see to this. Both sexes whilst infective must submit to preventive treatment.

The first step we can take is to demand that syphilis shall no longer be exempt from the action of the Infectious Diseases Notification Act. I fail to see why the disease should receive preferential treatment and especial favour. But we must, as a profession, be unanimous in our demand. It will avail nothing if we approach the Legislature with divided counsels. A resolution that syphilis should become a notifiable disease was passed unanimously at the beginning of last year by one of the local medical societies in the south-west of London. A short while ago I was invited to attend a meeting of the Society of Medical Officers of Health for the Metropolis, and the opinion prevailed there that to deal effectively with the prevention of syphilis some form of notification must come into force. In the Westminster Division of the British Medical Association this subject has been under discussion by the Council, but owing to pressure of work in connexion with the National Insurance Act it has been temporarily postponed, but Dr. Allan, ex-President of the Division and Medical Officer of Health, induced the Westminster City Council to apply last year to the Local Government Board for permission to supply the Wassermann reaction gratuitously to all medical men in the district applying for it. To this request the Local Government has not yet replied. I instance these facts to show that there is considerable activity already in this direction. Major French, R.A.M.C., spoke to the Royal Society of Medicine last month of the incalculable benefits conferred upon our Forces in India by the Cantonment Act of 1897, and of the success in the control of syphilis in Egypt and Malta by disciplinary enactments. Parenthetically let me say that I should like to see the convincing paper he then read to the Society in the hands of every adult man and woman in the country. There is already a large body of medical opinion in favour of notification.

"Many strokes, tho' with a little axe,
Hew down and fell the hardest timbered oak."

We may overcome prejudice and self-complacency if we are persevering enough. I would suggest, if notification were made compulsory, some such procedure as the following: After notification to the medical officer of health the latter would send notice to the patient as to the steps he (or she) must take to avoid infecting others and to get rid of the disease himself. The patient would be told he must carry out the prescribed medical directions, otherwise he would be liable to penalties; and in the case of a prostitute she would be removed and detained. If a husband or wife had acquired syphilis, it would be the duty of the medical officer of health to warn the infected party that if he or she communicated the disease to the other such action would be punishable; it might even be advisable to warn the uninfected party so that coitus with an infected person might be avoided. It is obviously most important that child-bearing women should not be infected. Cards showing attendance at the hands of a medical man or hospital would have to be produced, and only in the case of obstinacy and blank refusal to carry out medical directions would a police summons be issued, and even then there need be no publicity. Few people resent genuine attempts to benefit them, and medical men could do more by explanation and sympathy than officialism by coercion. It is to be remembered that even a prostitute does not like syphilis, any more than a sailor likes shipwreck. Both are risks incidental to their respective callings.

Major Harrison, R.A.M.C., who has had much experience in the working of the Cantonment Code in India, is insistent upon the fact that this matter of the control and direction of prostitutes must be in the hands of tactful medical officers, rather than of the police. Coercion would be kept in the background as a last resort. Persuasion and argument would be our chief weapons. If sexual immorality there must be, let it at least be a clean immorality, untainted by foul disease which will fall as often as not upon the innocent. Syphilis is already notifiable in Sweden, a country ever in the van of human endeavour and enlightenment, and at the present time notification is on its trial in some parts of Australia. In New York some tentative steps in the same direction are in contemplation, if not already in effect. Is it not right that we in this country should bestir ourselves also? Self-complacency is our besetting sin. Above all things, we hate to have our comfort disturbed. Inaction is always easier than action. But for some of us it is not enough to be told that the difficulties in the way of notification of this disease are insuperable. There are some men, and

alas, too many, who can always see "lions in the path," who almost go out of their way to look for them. The same objections were raised before the passing of the present Infectious Diseases Notification Act. It was declared that "people would never stand it," and much more was said to the same effect. But if the country is convinced that the existence of syphilis is detrimental in the highest degree to its health and happiness it will submit to some discipline and control in the treatment of this disease as of others.

The exact method of procedure following upon the notification of a case of syphilis to the medical officer of health or other approved medical authority would be a subject for future consideration, and much could be learnt from countries where notification is already in force and from our own military medical men in India, Malta and Egypt.

Let us call to mind the fact that little more that 400 years ago syphilis was unknown in England. It is relatively a recent scourge. At present it is rather the fashion to celebrate centenaries. We must see to it that syphilis does not celebrate another centenary in this country. If we can but induce the Legislature to make the disease notifiable we shall, I think, have driven the first nail into the coffin of syphilis.

Dr. J. P. CANDLER: In connexion with the present discussion on syphilis, I wish to put before this meeting the results of our experience of the Wassermann test on the blood and cerebrospinal fluid of cases of general paralysis of the insane and to offer some remarks on the value of the test with regard to general paralysis and syphilis. The work has been carried out in the Pathological Laboratory of the London County Asylums in collaboration with Mr. Mann, Dr. Mott's chemical assistant, and as Dr. Mott mentioned on the first day of the discussion (p. 83), we have had the opportunity of examining the cerebrospinal fluid of a large number of cases by this test. Already over 100 of these cases have died, and the result of the Wassermann reaction has been confirmed by autopsy, and microscopical investigation when necessary, in 97 per cent. Our percentage of positive reactions in general paralysis agrees very closely with those obtained by others whose findings have been generally accepted as correct.

EXAMINATION OF THE BLOOD SERUM IN GENERAL PARALYSIS.

We have lately directed our attention to the examination of the blood serum, as well as the cerebrospinal fluid of cases of general paralysis. We have done so because the serum reaction is of great

importance from the point of view of diagnosis, and because there appears to be considerable difference of opinion as to the percentage of positive results obtained with these cases. Plaut has obtained a positive reaction on the blood serum of general paralysis in 99 per cent. Boas examined the blood serum of 139 cases and obtained a positive result in *every* one. The experience of Carl Browning and Mackenzie is that the blood gives a positive result in 96 per cent., while other observers have only obtained positive reactions in 60 to 70 per cent. of the cases. We have examined the serum as well as the cerebrospinal fluid in 109 cases of general paralysis. The cerebrospinal fluids were sent up from the various asylums for diagnosis, and in all cases which gave the slightest positive result the serum was also examined. The results are in accordance with subsequent clinical diagnosis and in some cases have been confirmed by autopsy.

The results obtained may be summarized as follows: Number of cases examined, 109; number of positive results with cerebrospinal fluid, 109; number with serum, 107. The number of cases giving a positive result on the cerebrospinal fluid includes two cases, B. and E., which gave a slightly positive result on the cerebrospinal fluid when first tested; this positive result, however, was not obtained on subsequent examination. One of these patients, E., has since died, and the case has been shown microscopically to be one of general paralysis. The number of cases giving a positive result on the serum include five cases, G., S., H., T., and C., which on first examination gave a negative result, but which on subsequent examinations gave positive reactions. *Altogether, a positive reaction on the serum in general paralysis was obtained in* 107 *cases out of* 109—*i.e.*, 98·1 *per cent.*; but if we deduct the five cases in which the serum at one time gave a negative result the percentage incidence of positive cases is 93·6 per cent. There are certain points in the reactions obtained in these cases to which we wish to draw attention. The cases which call for mention are seven in number:—

(I) G., male. Clinical diagnosis: General paralysis of the insane.

Date	Cells	Cerebrospinal fluid result	Date	Serum result
May 17, 1911 ...	+ ...	+ marked	December 29, 1911 ...	−
,, 2, 1912 ...	+ ...	+ ,,	June 3, 1912 ...	+ moderate
June 3, ,, ...	+ ...	+ ,,		

(II) S., male. Clinical diagnosis: General paralysis of the insane.

Date	Cells	Cerebrospinal fluid result	Date	Serum result
November 10, 1911 ...	+ ...	+ marked	February 2, 1912 ...	−
			May 22, ,, ...	+ marked

(III) H., male. Clinical diagnosis: General paralysis of the insane.

Date	Cells	Cerebrospinal fluid result	Date	Serum result
June 15, 1911 ...	+ ...	+ marked	February 10, 1912 ...	−
February 24, 1912 ...	+ ...	+ ,,	,, 24, ,, ...	−
			(Prevention starting with slightly increased dose)	

(IV) I., male. Clinical diagnosis: General paralysis of the insane.

Date	Cells	Cerebrospinal fluid result	Date	Serum result
June 26, 1912 ...	+ ...	+ marked	February 24, 1912 ...	−
			June 26, ,, ...	+ marked

(V) C., male. Clinical diagnosis: General paralysis of the insane.

Date	Cells	Cerebrospinal fluid result	Date	Serum result
June 20, 1912 ...	+ ...	+ marked	February 24, 1912 ...	−
			June 20, ,, ...	+ marked

(VI) B., female. Clinical diagnosis: General paralysis of the insane.

Date	Cells	Cerebrospinal fluid result	Date	Serum result
December 14, 1911 ...	+ ? ...	+ slight	December 29, 1911 ...	−
May 10, 1912 ...	+ ? ...	−		

(VII) E., male. General paralysis of the insane confirmed by autopsy and microscope.

Date	Cells	Cerebrospinal fluid result	Date	Serum result
August 21, 1911 ...	+ ? ...	+ slight	February 10, 1912 ...	−
February 24, 1912 ...	+ ? ...	−	,, 24, ,, ...	−

All the cases in which the serum gave a negative result were given special attention and the tests were repeated after further inactivation and the use of increasing doses of serum (in some cases up to twice the usual amount) but with a similar negative result, except in the case of H., in which some degree of inhibition was observed when a slightly larger dose of serum than usually employed was used in the test. The cases recorded appear to be divisible into two classes: (1) Cases I to V, in which with a strongly reacting cerebrospinal fluid a variable result was obtained with the serum; (2) Cases VI and VII, where with a feeble and disappearing reaction with the cerebrospinal fluid there was associated a negative result with the serum. We do not propose to attempt to explain the reason for the reaction changes which we have described, but we wish to point out that, according to our findings, not only may both the serum and the cerebrospinal fluid in a small

percentage of cases of undoubted general paralysis of the insane fail to give the reaction, but that the reaction may be negative at one period and positive at another in the same individual, and that this phenomenon is rather more likely in our opinion to occur with the serum than with the cerebrospinal fluid. We do not agree with the statement that the blood serum of *every* case of general paralysis will give a positive result, and further we are of the opinion that in cases of suspected general paralysis in which a negative reaction is met with in either fluid it is advisable to repeat the test subsequently on one or more occasions. Plaut has stated that he has met with all grades of intensity in positive reactions with the fluids in general paralysis. We particularly wish to emphasize this statement. We have found that as a general rule both fluids give a well-marked and intense reaction, which can be easily detected by a simple qualitative Wassermann test; but in some cases of general paralysis, which were confirmed by post-mortem examination, the reaction was quite slight. It is this type of case which it is most important to recognize, and which will be missed if the technique is not accurate and the reagents accurately standardized. We wish, therefore, to make a few remarks upon the test with regard to the various causes which may lead to inaccuracy.

All our own observations have been made by a method which follows very closely the original plan adopted by Wassermann. Every precaution has been taken to standardize repeatedly all the reagents used in the test, and to adopt a system of controls to guard against every possible source of error. The same liver extract has been used throughout the whole of the work and the same technique adopted in every examination. We have estimated our results by a quantitative determination of the amount of complement-deviating properties of the serum and cerebrospinal fluid in every case examined; we have never relied on a simple qualitative test. The blood to be tested was always withdrawn from a vein, the serum was removed from the clot as soon as possible and inactivated at once. The cerebrospinal fluid was centrifuged before the test was performed, in order to remove all solid particles. Sheep or ox corpuscles were used with their appropriate hæmolysin and guinea-pig serum for complement. The antigen used was an alcoholic extract of the liver of a syphilitic fœtus which had been prepared by special methods. This extract is exceedingly good and reliable and has been supplied to other workers, who have also reported very favourably on its efficiency.

We are therefore convinced that the negative results we obtained in

cases of general paralysis were correct interpretations of the reaction. The two errors to be avoided are: (1) the return of a negative serum as positive, and (2) the return of a positive serum as negative. The first source of error can easily be avoided provided a proper test is used and adequate controls arranged; the amount of complement absorbed by the serum in the presence of extract accurately arranged; and an extract used whose antigenic and anticomplementary values are satisfactory. The other source of error may be far more difficult to avoid, and the cause rests mainly with the extract. Now as we have previously stated, the majority of sera and cerebrospinal fluids in general paralysis and the sera in well-marked cases of syphilis will give a satisfactory reaction by a simple qualitative test and with a moderately indifferent extract. But the cases which give the trouble are the feebly reacting sera and cerebrospinal fluids in cases of general paralysis and latent syphilis; further, in cases of active syphilis under treatment with salvarsan it is most essential to be able to determine precisely whether the patient's serum is showing the slightest amount of retardation, which is an indication for further treatment. Our experience with syphilitic sera is not so great as with general paralysis, nevertheless, we have met with instances of returning feeble reactions which are of great significance to the physician. One of the most important observations which Plaut has made deals with the inability of certain aqueous extracts to bring out a positive result in every case where it is possible of detection. Fortune has favoured us with a valuable extract, but our own experience leads us to believe that the low percentage of positive results obtained by some observers may be almost solely due to the use of unsuitable extracts. We have been fortunate in obtaining a fairly abundant supply of livers from presumably syphilitic fœtuses. Special care has been given to the preparation of alcoholic extracts, and although the majority of them act well as antigens in showing marked reactions, comparatively few are efficient in showing the moderate or slight reactions which are often met with in the serum and cerebrospinal fluids.

To make sure that an extract is an efficient antigen it is necessary to test it side by side with a standard extract, using several fluids which are known to give weak reactions with the standard extract. Every extract should be rejected which fails to react with weakly reacting fluids. It is therefore necessary to preserve and carbolize as many weakly reacting fluids as it is possible to obtain for the purpose of testing these extracts. Further, it is essential to note that the value of an extract cannot be definitely settled by testing it against a good

reacting serum or cerebrospinal fluid which has been diluted down with saline, for with this it may give a positive result, whereas it may still fail to show up a naturally weakly reacting fluid.

In our opinion a suitable alcoholic extract prepared from the liver of a syphilitic fœtus will give as good results as any other form of extract, but livers of syphilitic fœtuses are not abundant, and further, extracts made from many of these are quite useless, probably from changes due to decomposition products. We believe that good extracts can be obtained from freshly prepared animal tissues, and Browning and MacKenzie speak most favourably of a mixture of lecithin and cholesterol, but of these we have not had sufficient practical experience. We consider the preparation of suitable extracts most important for the success of the reaction.

It is becoming more and more evident that the Wassermann test is going to play a most important part in the identification and treatment of syphilis, and it is absolutely necessary to have the test put upon a proper basis whereby an accurate report on slightly reacting fluids can be safely given. Many modifications of the test are employed in this country, some of which are undoubtedly useless, and there is a great variability in the relative amounts of the various materials used by different workers and also in the methods of estimating the reaction. It would be of great value if this Society were to investigate the whole subject, and by a consensus of expert opinion determine on the most suitable method with dosage for the performance of the test. Attempts should also be made to determine which is the best form of extract to use and how it should be prepared. The publication and universal adoption of *one* method would lead to more satisfactory results and to a greater respect of the value of the test than it has even won at present.

We have to acknowledge our indebtedness to the Medical Superintendents of the London County Asylums whose cases form the basis of this paper, and to the various medical officers who have kindly obtained the specimens for us; especially are we indebted in this direction to Dr. George Evans and Dr. Paine.

Dr. J. W. Barrett, C.M.G. (Melbourne): I thank you very much for the opportunity given me of addressing this meeting. Dr. Mott suggested my attendance because I am for the moment in the unique position of being one of the advisers of the Government of Victoria, and of having been with others responsible for the carrying out of some of the proposals which have been made this afternoon. We conducted,

with the aid of the Government of Victoria, and under the most able direction of Dr. Burnett Ham, Chairman of the Board of Health, a most extensive experiment in regard to the distribution of syphilis. At its conclusion the Government inquired in effect: "What do you now advise us to do?" We were thus put in a position of responsibility, and we found that it is much easier to make suggestions than it is to put the proposals into operation, even when you are given a reasonably free hand. Dr. Mott suggested I should come and give you an account of what was a remarkable experiment. But before doing so I should like briefly to refer to the paper of Dr. Mott, with which, as far as my knowledge goes, I am in complete general agreement. But none of the speakers referred to the Noguchi reaction for syphilis, which was devised at the Rockefeller Institute, New York, and may be seen in practice at the Sinai Hospital. It offers reasonable hope of providing a better means of detecting the presence of latent syphilis than the Wassermann reaction. The physician of the future when diagnosing syphilis will probably depend on the clinical evidence, the Wassermann reaction, and the Noguchi reaction. The Noguchi method is quite simple in technique, and I do not propose to take up your time with it. It will be better to indicate briefly the history of the experiment in Australia, which is recorded in the *Australian Medical Gazette* and *Australian Medical Journal*. But papers are apt to give to some extent the dry bones of a problem, and not the reasons which led to action. The position is, shortly, as follows: Professor Allen made post-mortems in the Melbourne Hospital, of two series of 100 patients each, who had died from all kinds of disease. He found that roughly one-third of the cases in each hundred showed evidence of syphilis; there was aortic atheroma, thickening of Glisson's capsule and the pia mater, and so forth. In my work in Victoria, in the Eye and Ear Hospital, the following position had been reached: Syphilis exists without any clinical manifestation whatsoever. Furthermore, a good deal of syphilis exists without clinical evidence other than various choroidal degenerations, and some of my colleagues had, in the course of fairly extensive practices, traced family development where syphilis was known to exist, and found in some members, say, one or two craters of choroiditis. With this evidence, when we found another case of choroiditis in which neither history or evidence of syphilis could be obtained, we still assumed that it was syphilitic. The position taken up by Professor Allen and ourselves was challenged. It was said: "The evidence is not conclusive; you must show that some other toxin will not produce these

results; you are confusing the issue." This argument became chronic, but with the access of information many practitioners came over to our side, particularly those dealing with children's diseases, and at the Australian Medical Congress of 1909 the following resolution was carried, after a prolonged and interesting discussion: "That syphilis is responsible for an enormous amount of damage to mankind, and that preventive and remedial measures directed against it are worthy of the utmost consideration." The resolution was presented to the Government of the day. Its presentation was followed by a deputation of clergy, who urged that something should be done. The Chairman of the Board of Health, Dr. Burnett Ham, was sent for by the Premier, who asked him whether the facts were really as stated, or whether he had to deal with syphilophobes. Dr. Ham wisely replied that he could not say, but if the Government would provide the necessary means he would make investigation by means of the Wassermann reaction, and obtain conclusive information. The proposal was accepted, and a Committee, including Professor Allen and a number of other medical men, including myself, was appointed to supervise the experiment. It was conducted as follows:—

From June 1, 1910, to May 31, 1911, syphilis was made compulsorily notifiable. Names were not furnished, but the age, sex and clinical conditions were supplied. And in each case blood was sent to the University, where Dr. Hiller made a Wassermann determination. The clinical evidence was thus correlated with the results. During that period also a number of children who died in the Children's Hospital were examined post mortem, and their blood sent to the University, and the two sets of records were compared, to ascertain whether gross lesions attributed to syphilis coincided with the positive reaction. The cases which were to be reported were primary, secondary or tertiary syphilitic lesions, all cases of thoracic aneurysm, aortic regurgitation, apoplexy in the young, locomotor ataxy, and general paralysis; also cases of congenital syphilis, cases where the mothers had frequently aborted, or where many deaths of children occurred in any family. During the year 5,500 Wassermann determinations were made, of which 900 were from the Melbourne General Hospital, 1,100 from the Eye and Ear Hospital and our own private work, and 3,500 from the general profession; 1,900 positive Wassermann reactions were obtained, which we interpreted broadly as meaning syphilis in 10 cases for every 6 positive results. We assumed, taking all classes of cases of this kind, that, on the average, 6 positive Wassermann reactions corresponded

to 10 cases of syphilis. At the Children's Hospital the evidence was conclusive, and determined the position taken up by Professor Allen. In my clinique—that of the Eye and Ear Hospital, which on Mondays and Thursdays included eye cases, and on Tuesdays and Fridays ear cases—we for one year gave ourselves over to the investigation of this matter. We had a staff of some eight or ten surgeons, whose energies were directed to the examination of doubtful cases, to the recording of the clinical symptoms, and to the correlation of the results. At the end of the first eight months there were 52 positive and 10 partially positive results amongst the eye cases. But by this time the area of suspicion had widened: for example, we began to regard many cases of neurasthenia as being based on a syphilitic background. So we waited on Dr. Ham and asked permission to conduct a still more extensive experiment. That permission was granted, and for the last four months of the year all patients who came to the Eye and Ear Hospital on Monday and Thursday and Tuesday and Friday had their blood examined, no matter what disease they suffered from. We were led to this position by facts of this kind: A patient came in with chaff in the eye. After removal of the chaff conjunctivitis set in and persistently remained. A determination of the blood of that patient was taken, and was found to be positive. A man came with a slight cut over the eye: the scar became indurated; the test was positive. A cataract was extracted, and after some days an inexplicable iritis set in; the determination proved positive. The accumulation of this class of fact led us to the opinion that probably many of these troubles were due to latent syphilis, syphilis of which there was no clinical evidence, and that the trauma probably excited a condition which otherwise would not have developed. During those last four months we examined 443 eye cases, and obtained 37 positive reactions and 27 partials. On the former showing this result indicates that in the first eight months we had missed a large number of cases of syphilis, for the corresponding figures for the eight months would have been somewhere about 74 positives and not 52, and 54 partials, not 10. So in a clinique of people who were alleged to be syphilophobes many cases were being missed on the clinical findings. Out of a total of 443 cases on Monday and Thursday there were 35 positive reactions and 23 partials, or a total of 13·2 per cent.; and in the clinique on Tuesday and Friday afternoons there were 107 cases with 9 positives and 8 partials, a total of 15 per cent. The bulk of those people showed no clinical evidence of syphilis.

We had made the nearest attempt we could to ascertaining the incidence of syphilis in the whole community. We regarded the positives and partials as the only evidence of syphilis, and adding nothing for those who failed to give the reaction. The one set of figures gave 13 per cent. and the other 15 per cent. We then reported these facts to the Government, with the additional statement from the staff of the Women's Hospital that half of their operative work was necessitated by gonorrhœa. The Government asked us what course we advised. We first enlisted, informally, the sympathy of the women in the community. The National Council of Women in Melbourne was approached by some medical men and asked to put an end to the practice on the part of educated women of pretending not to know what everybody knew they did know. They did not like the position, but they acted in the most dignified manner. The Council asked women practitioners to address the members on the subject, and to give them a balanced view of the position. After hearing the statement, the Council informed the Government that it would co-operate in any reasonable steps which might be taken to deal with the question of syphilis. As women possess the franchise in Australia this information was important. The next matter was to persuade the Press—the lay Press—to call syphilis by its proper name, and to avoid such expressions as "malignant contagious disease" or "a secret disease." The *Argus* newspaper, to its lasting credit, decided that the proper term would be used in future. The next problem, and the most difficult one, was to separate the moral from the medical, which is essential to the effective handling of venereal diseases. More than half the persons who suffer from venereal disease are not responsible for its acquisition; it is communicated to them by somebody else, in marriage or by inheritance; and if you allow the moral to be confused with the medical, the persons who are responsible with those who are not responsible get branded with the reproach of immorality. The effect is to send them away for secret treatment. There thus arises a false modesty which makes the position most difficult. An acute issue was shortly reached, because the Society for the Prevention of Organized Vice wanted some of us to join in their crusade. We informed the supporters that if their campaign was a moral one we would be glad to help in our individual capacities, but if it was understood that the Society wished to interfere with medical management we would rather they went their way and we went ours.

There appears in the discussion a statement that prostitution is due to poverty. Prostitution is not due to poverty, because in Australia there is no poverty, and I think that, approximately, there is the same

amount and kind of prostitution as in other countries. In Australia a woman can earn good wages at domestic service, and poverty does not cause her to follow this kind of life. Prostitution is due to the desire to get a living without giving the proper return for it; it represents a mental and moral aberration, and must be looked at from that point of view. No doubt in the old countries poverty may be a determining factor, but in a country where there is no poverty prostitution still exists.

Having advanced so far, what was to be the next step? We advised the Government that it should, at public expense, set aside a ward in the Women's Hospital and one in the Alfred Hospital, Melbourne, to which anybody could go as an indoor patient for treatment who was liable to communicate syphilis or gonorrhœa; that medical men should be circularized and invited to send in such patients. These patients would get past the stage at which they could communicate the disease as quickly as possible. Sensible advice could be given to them at the same time. After some difficulty the Government adopted that course. In New South Wales there is an Act providing that a criminal suffering from venereal disease need not be released until cured. That does not apply to persons punished by fine, but only to those who undergo imprisonment.

With regard to a Contagious Diseases Act I am not very enthusiastic, and, in any event, the attitude of the ladies in Melbourne would determine the matter, because they have intimated to us informally, but in the clearest possible language, that they will not contemplate any Contagious Diseases Act or any treatment of women which differs from that of men, and that if women are to be segregated because they are communicating disease, then the men who are communicating disease must be segregated as well. And of course that amounts to a *reductio ad absurdum.* If you have been in Yokohama and seen what happens there, you will realize the difficulty of doing anything of the kind. Men from the fleets of the different nations come ashore in varying numbers day after day and have intercourse with women; you could not control those men, and consequently the machinery would break down. I think, however, that the course followed in some countries is the right one—namely, that information be given regarding the means of preventing infection. This can be done by medical men who are employed by the brothels, and in other ways. I do not think the problem of morality is touched, one way or the other, by the question of infection. The world is not likely to be made more or less moral by allowing people to become syphilitic.

In outline, that is the position we have reached in Victoria. I do not know what the next step will be as I have been away for three months. We shall, however, endeavour to eradicate the disease. We hope, by an educational movement—in which women as well as men take part—to deal with it sensibly. Some of us think that syphilis is the principal cause of death before senility, that it underlies many infections and causes much disaster. And we have the specific evidence of the staff of the Women's Hospital that half their operative work is due to gonorrhœa. It is probable that if you eliminated syphilis and gonorrhœa—and such a result is quite possible—you would have, from the medical point of view, almost a new world to deal with. You have spoken of going to the British Government and asking that something shall be done. We are in the fortunate position of being able to speak about what we have tried; and as people who, having tried to take action, know the difficulties. Professor Allen is in Great Britain, and if you think of going to the Government he will, I am sure, be glad to go with you and, if it will be of any service at all, we can say what our experience has been. It is desirable, I think, to let the lay Press understand the problem and secure its co-operation by a clear and judicial statement of the facts.

The foregoing statement would be incomplete without the acknowledgment of the splendid manner in which the Victorian profession supported Dr. Ham's efforts, and of Dr. Hiller's devotion in the accurate conduct of the Wassermann determinations.

Dr. J. M. BERNSTEIN said that his excuse for entering into the discussion on syphilis was that for many years he had been investigating the subject in as far as it came under his ken as a pathologist and a physician, and now that the diagnosis and treatment had come into the realm of science, syphilis had become part and parcel of the work of the scientific physician. Much of what he might have said had been already dealt with, and based upon more extensive statistics than he could have adduced. He had hoped that one small point would be left to him, but the last speaker, in his fascinating account of the investigation of syphilis without any signs, had taken even this crumb away from him; but he was pleased to have so strong a support.

Much light had been thrown on the prevalence of syphilis at the present time by the serological diagnosis. He had made a point of doing, himself, the Wassermann test, by the original method, on thirty to forty hospital cases weekly for some years, and had concluded that

many mysterious trains of symptoms seen in his own out-patient department were due to syphilis, and often on further investigation the patient could be made to remember an infection in early life.

There were cases, previously not diagnosable, of severe and constant headache in middle life, or vague pains in the spine, sometimes in fathers or mothers of large and healthy families. In these it was sometimes found that twenty or thirty years previously they had been infected with syphilis, but thought they were cured. In many cases with neurasthenic states, men who in middle life were losing their nerve, &c., a surprising number gave a positive result, and in many of them the symptoms disappeared under suitable antisyphilitic treatment.

With regard to congenital syphilis, he had had some considerable experience of this in connexion with the West London Hospital and some special hospitals. At the first of these hospitals for over two years he had examined the blood of infants who were clinically diagnosed by Dr. Saunders to have syphilis. To make the matter more complete, he had examined the blood of the parents, or one of them, and in most cases where the parent's blood could not be obtained the blood of some collateral member was examined. Over one hundred had been examined, and an enormous number of them had given a positive or partially positive result, which latter he regarded as establishing syphilis. In nearly all cases the mothers also gave a positive reaction, and where the child was under the influence of mercury the mother alone gave the reaction. In connexion with diseases of the eye, Mr. Bishop Harman, over eighteen months ago, investigated a large series of cases of children of 8 to 10 years of age suffering from interstitial keratitis, cyclitis, and other eye diseases, the serological work being done in his (Dr. Bernstein's) laboratory by Dr. Brunt, under his supervision. The results showed that a large number gave the positive reaction; all that were clinically syphilitic, and also those who were suggestively so, and a few in whom there was no reason to suspect syphilis. Sometimes there would be a history of three or four children being born blind or idiotic, and where their bloods were examined they gave a positive reaction, and that was so also in many where the disease was not even suspected. With Mr. MacLeod Yearsley he had more recently examined fifty children from the London County Council Schools for the Deaf, their blood being sent to him, and beyond which he knew nothing about the cases. Three or four only of those were positive. The figures would be published by Mr. Yearsley. From the Children's Hospital Mr. Addison had sent him specimens of the blood

of patients suffering from diseases of joints or of bones, the patients being between 3 and 11 years of age. Mr. Addison's original views as to the specific nature of those clinical manifestations had been supported. From Dr. Grainger Stewart's out-patient department he had culled a few cases of congenital tabes and congenital general paralysis of the insane, two of them in girls aged 15 and 17 respectively. They had given markedly positive reactions. In every case possible they had examined the blood of one or other of the parents, and found corroboration. Finally, he had had sent to him by Dr. Simson women who had miscarried time after time, and a marked positive reaction was obtained from the blood of these. Basing the treatment on that diagnosis, they had been able to ensure the woman having her next birth at full term.

In view of the points which had been brought out by specialists on the subject he would touch on the results of the treatment, because no statistics could be large enough yet to help in forming opinions as to the line of treatment which had been so much discussed. He and his colleagues had been among the first to use salvarsan, and he had himself used it in patients from 14 weeks to 60 years of age. In addition, they had treated with salvarsan babies *in utero* in the case of three of the women who had been repeatedly having miscarriages, or who had contracted syphilis during the pregnancy. As a result of that, three babies had been produced which did not react positively at birth to the Wassermann test; though he must add the first died at the age of 7 weeks from summer diarrhœa, the child being illegitimate. He had seen no contra-indications for treatment, though they had treated severe cases of aneurysm, aortitis with regurgitation in younger subjects, syphilitic paraplegia, gummatous meningitis, &c. One case which he was asked by Dr. Grainger Stewart to treat was apparently moribund and certainly comatose from gummatous meningitis; there were twitchings and marked pressure symptoms, and though the blood gave a negative reaction the cerebrospinal fluid gave definite cytological and serological proof of syphilis. Two days after the first injection of "606" the man was conscious and rapidly recovered, the effect being almost magical. Several cases of tabes showed marked improvement, and in some of these he also had obtained a positive Wassermann reaction in the cerebrospinal fluid when it could not be obtained from the blood. In a series of three to four hundred injections he had seen practically no bad results save one case of unilateral ophthalmoplegia. In one case in which he was asked to do a post-mortem examination the patient had died while the injection was going on. But there he was sure,

from his examination, that it was due to the patient being a confirmed alcoholic; his heart was so fatty (as were also his other viscera) that it would not stand the extra bulk of fluid injected. Sudden death has occurred from a similar cause during the injection of normal saline, and he himself has produced it in his experiments with rabbits. He would suggest that the dangers were failure of the diseased heart, such as this one; separation of a thrombus formed in a damaged vein, or a ligatured vein which had become the seat of endophlebitis; and finally septicæmia. There was seldom any need to damage the vein, which can be used time after time for the injections. He thought that the operation of venipuncture was often treated too lightly, and he would suggest that the essential factor in successfully accomplishing this was a sharp and properly bevelled needle. He was inclined to think that where there was reason to suspect a condition of endarteritis, treatment with salvarsan should be delayed until a course of iodides had removed the active mischief.

As to the value of the salvarsan treatment, they had been told lately that eight to ten injections were necessary to cure syphilis, but he felt sure that this was an exaggeration of the true state of affairs. The statistics of the Army Medical School—the real fountain-head of unbiased observations on this matter—which have been put forward by some of the speakers, would bear him out in this. His own view was that primary and secondary syphilis in the majority of cases completely disappeared under two injections (and he had even had similar results with one injection), though a small number were refractory as far as the blood test showed. But in some of the tertiary cases he had found that the Wassermann reaction failed to disappear or returned after some months; and several of these were cases which had failed to get well under a complete and thorough course of mercury. Many of his cases had now been under observation for a period of eighteen months, and he was hopefully anticipating having accomplished a complete cure. His course of action in the future would be that while the patient could be kept under observation and have periodical blood tests performed he would content himself with two injections; but where this was impossible, or where the Wassermann reaction failed to become negative, he would further advise a course of mercurial injections.

There was just a point concerning the discrepancy of results of the Wassermann reaction, occasionally with the same specimen, which had cast some doubts on the value of this test. It was true that the test which has been described as the most scientific method of diagnosis in

medicine, being based on scientific premises and logical deductions, is now, owing to the various antigens that can be employed, less understood than at first; but statistics show that, even on an empirical basis, its value is none the less great. But the discrepancies were due to individual variations in technique, and he felt that in the hands of *carefully trained* workers, using the *original Wassermann method* with its true scientific foundation, there would be in all cases concord. He, for his own part, would refuse to relegate the Wassermann reactions of his own patients, and insisted on doing them himself—a tedious procedure, but repaid by the conviction of the accuracy of the results.

Of practical importance was his observation that, unfortunately, sickness and rigors were still found, even with specially distilled water, though only in a few cases. He could not say that there was any difference between a series of cases treated with ordinary distilled water and another series treated with specially distilled water. He was inclined to think that the lessened incidence of these after-effects had some other explanation, and possibly associated with improvements in the drug.

With regard to the public health aspect, although it was recognized that prophylactic medicine was the ideal medicine—that ideal (which had been aimed at since the days of the Patriarchs) would not be reached; and so there would always be curative medicine, and therefore the necessity of laying stress on diagnostic methods.

With regard to notification, which had been advocated in the discussion, what ought to be notified? Should it be open syphilis or should it include concealed syphilis—where the disease showed no obvious manifestations, but where there was still danger of infection; or should it include all the parasyphilitic lesions, the congenital cases, and all the active sequelæ, &c.? It was all very well to compare the eradication of hydrophobia and even of glanders and other conditions with syphilis, but the conditions in the latter were much more difficult. One spirochæte with its long incubation period, coupled with the secret habits of its host, getting into the country when all the syphilis had, hypothetically, been wiped out, would be sufficient to cause the disease to break out afresh. He did not see how it would be possible to examine every returning emigrant and immigrant by the blood test to see if he was syphilitic, and if so to notify. Already the hospital patient was getting wiser, and very knowing. Not infrequently one suggested to a male patient that he should go to the laboratory and have his blood tested, but he never got there, and one learned afterwards that he told the porter he knew too much to have it done. A

hopeful sign was that the public were getting a better appreciation of the dangers of venereal disease, and it was not infrequent for a man who wished to get married to come to be assured that he was in a fit condition to enter that state. The "matrimonial candidate" was better known in Germany and America, in which latter country, judging from the literature, the subject was perhaps too well ventilated in the public Press. He agreed with one of the speakers that the control of syphilis should be more a control of the person, and that it should be on the lines of an improved *morale* for the younger generation. The absolute control of syphilis should not be in the hands of the Local Government Board, but in the hands of what he might call the Home Government Board. Amongst those people in whom home influences and teachings had been entailed for generations, he was of opinion that a better bill of health could be shown as regards infectious diseases. But though he could not see any possibility of being able to notify all cases of syphilis, it would strengthen their hands if it were in their power to notify those cases which one sometimes saw, such as chefs from hotels and cooks from private houses, who were covered with syphilitic lesions, forming a focus of infection and great opportunities for being very dangerous; some of the worst cases he had met with came from several of the largest and well-known restaurants in London, and yet it was impossible for him to move in the matter, though he would point out that it is within the powers of a medical officer of health to enter and inspect the kitchens of a restaurant, and hence the loyal citizen might find a means of eradicating an infectious case from his erstwhile favourite restaurant.

It was hoped that there would be established large central laboratories to which blood could be sent; the private practitioner had not the means to do that in every case. He himself, as bacteriologist to the City, had been approached by the Westminster City Council with regard to this matter, but up to the present the Local Government Board had not given sanction for such an expenditure, which indeed formed rather a difficult problem. And the laboratory could be used also for the diagnosis of glanders, diphtheria, tubercle, and other conditions. Such central and municipal institutions have already been founded in other countries, and there is much to be said for them, provided they are controlled by a director of repute whose reports would be beyond dispute.

He would wish to apologize for the scattered nature of his observations and for the absence of statistics in support of his view, but if time permits it is hoped that many of them will be further elaborated.

Dr. ARTHUR POWELL: From the fact that Hippocrates, Galen, and other writers make no mention of tabes or of general paralysis, Dr. Norman Moore argues that syphilis did not exist in ancient Rome. Apply the same argument to modern India. Neither tabes nor general paralysis are found among the natives of India, we may therefore, according to Dr. Moore, assume there is no syphilis in India! We know, on the contrary, the cases of syphilis in that country must be numbered by millions. If we have such a variation in type in the present day, may not the type of syphilis in Italy have varied in the lapse of time through attenuation of the virus, continual syphilization of the race, or change in the manner of living, as Dr. Mott suggests the apparent increase of parasyphilitic affections may have arisen in Europe? While syphilis seems to spare the nervous system of the natives of India, my experience is that it affects their arteries much more severely than it does those of Europeans. In Bombay City all cases of sudden death are brought to me by the police. I find post mortem the most common cause of sudden death, with the exception of violence, is rupture of aneurysm of the arch of the aorta. Sudden death from this cause is eight times as frequent as death from valvular disease of the heart. I have had three cases of aneurysm at the same time lying on my mortuary tables. The character of the aneurysm points to syphilis as the cause.

As regards the existence of syphilis in ancient times, I am surprised that no one has referred to the disease described in Leviticus as "leprosy." Whatever that disease may have been, it most certainly was not leprosy. It was a contagious disease characterized by a primary sore in the form of a "boil" or an ulcer, followed by a secondary general cutaneous eruption, and in some cases by a falling out of the hair. The symptoms were liable to relapses and in many cases disappeared spontaneously.

From a public health point of view I quite agree with most of the previous speakers that among the infective diseases syphilis should receive no preferential treatment. It is absurd that a maudlin sentimentalism should single out from all the disease-causing microbes the *Treponema*—the "Lustschräubehen" of a recent German author—as a special pet, with whose propagation no State regulations may interfere.

I cannot agree with Mr. McDonagh that medical supervision and segregation of diseased prostitutes has in nowise diminished venereal disease. I can as soon believe that the removal or putting out of action of the enemy's riflemen on a battlefield will have no effect on the

number of bullet wounds in the opposing army. Major French has already thoroughly dealt with this point, and I will only endorse his views as to the beneficial effect of the Cantonments Act in India. The Contagious Diseases Acts may turn a number of open prostitutes into the clandestine class, but the clandestine prostitute can never have as large a practice as her sister who openly pursues her profession. Should her clients become numerous, her existence must become known to the most inefficient police force. Mr. McDonagh has another objection to State interference in that it differentiates between the sexes. To me this seems no argument. If it be a crime to differentiate between the sexes, we have a good fellow-criminal in Nature or the Creator. The law can only do that which is feasible and expedient. The law attempts to restrain the dissemination of other diseases. It places restraints on the sale and dissemination of dangerous explosives, firearms, alcohol and other poisons. In these cases the main supervision and control falls upon the manufacturer or seller. They happen to be almost without exception males, yet no cry of injustice to that sex is raised. It is the prostitute who sells and chiefly disseminates the *Treponema* and *Gonococcus*. Fortunately we have no profession of male prostitutes in this country; all the profession are females, but it is at the profession, not the sex, the laws are aimed. It is easy for the law to get at and restrain the prostitute; it is difficult, if not impossible, to get at the male syphilitic. The object of the laws is to check the dissemination of disease, and it should take those steps which are not only possible but easy.

More than one speaker has recommended as a preventive measure the teaching to boys the dangers of syphilis. In such a measure a great deal of tact would be required lest an undue syphilophobia should be produced. The dangers of syphilophobia are by no means small. I have held autopsies on eleven cases of suicide due to syphilophobia, and I know many cases of lunacy due to the same cause. I am not sure that some of the suicides which came under my official notice might not be more appropriately termed homicide caused by ignorant friends and medical advisers giving an unjustifiably lurid prognosis.

I am sorry to say it is not only the "penny dreadful" novelist, but medical authors who should know better, who have painted such monstrous pictures of the bogey syphilis. Most of us have met the type of syphilitic who, primed with such old wives' fables, carefully shakes his socks each night to see if any of his toes have been left behind. Even in this debate we have heard one speaker refer to syphilis as being transmitted to the fourth generation!

Treatment.—Five years ago I used a good deal of atoxyl and arylarsonates. With soamin I have seen successes as striking as with salvarsan. I have had two severe, though transient, accidents with soamin in sixty-three cases treated. In the case of a patient where mercury and the iodides have failed, if salvarsan is contra-indicated, I would still venture to advise soamin. Can any one conversant with the literature of the subject say what is the minimum quantity of this drug that has caused optic atrophy? I can find no record of any case in which less than 60 gr. had been administered. We frequently find marked improvement after three or four doses of 6 gr. of soamin in cases that had obstinately resisted mercury and iodides. Mr. D'Arcy Power advises that solutions of salvarsan should be injected at a temperature of 105° F. Mr. McDonagh objects, and says the higher the temperature the more toxic the effects. My experience is that it is advisable to use the fluid at a temperature of at least 105° F. In the past twenty-four years I have given many hundred intravenous injections of saline fluid in cases of cholera. Many of these injections were performed in the patients' own huts, where owing to unavoidable delay the fluid sometimes became too low in temperature. I noticed that rigors followed in many such cases. Acting on that experience, in the last eight cases in which I used salvarsan I injected the fluid at a temperature of 108° to 110° F. In seven of these cases the patient's temperature never rose above normal; in the eighth the temperature rose to 99·4° F., but the patient was quite unaware of any fever. These injections were made last year before my attention had been drawn to the necessity of using freshly distilled water. The water I used had been in stock for some months, but had been, of course, boiled twenty-four hours and again two hours before use. The number of cases is so small that the absence of fever may only be a coincidence, but I have seen rigors follow injections at 98° and 100° F. when the water used had been distilled on the same day.

In case of death following the use of salvarsan I would ask the observer carefully to examine the condition of the heart. Through the kindness of Principal Hewlett, of the Bombay Veterinary College, I had the opportunity of examining post mortem seven horses that had died of atoxyl injections. In five of the seven I found striking and characteristic hæmorrhages below the endocardium of the left ventricle. I have elsewhere drawn attention to this as a common sign in acute arsenical poisoning.

SUMMARY OF THE DEBATE BY THE PRESIDENT.

Sir HENRY MORRIS: In bringing to a close the important debate which has now extended over four sittings, I propose first to contribute some remarks on the history of syphilis, next to comment on some of the points raised in connexion with the Wassermann reaction and parasyphilis, and finally to make a few observations on the treatment of the disease.

HISTORY: PREVALENCE AND INTENSITY IN THE PAST.

The greater part of what I have to say refers to the question of the prevalence and intensity of syphilis in the past, because in the first place I find in it much which interests me, but chiefly because less attention has been given to this than to either of the other two aspects of the subject which were set down for discussion.

Whether syphilis did or did not exist in prehistoric and early historic times still remains an unsettled question. Probably it will always so remain. Intelligible as is a belief that the disease prevailed in Europe in pre-Christian and early Christian centuries, it rests upon very meagre evidence, supported by inferences from facts rather than by facts themselves. Nor has this debate, so far, added any evidence of a more convincing kind.

It is said that about 2500 B.C. a Chinese writer, Nusi King, described the phenomena of venereal disease, and among them the symptoms of lues venerea. But what was the lues? Gonorrhœa, chancroid, lepra, or lupus might perhaps equally have been referred to under a term meaning "a spreading calamity," "a pest," or "a contagious disease."

No reference is made to venereal disease in the Babylonian Code known as the Laws of Hammurabi—the oldest known laws in the world—which date from 2100 B.C., or 500 years before the Laws of Moses. Yet, had syphilis been a scourge in those days it would probably have come under some legal control, because the Babylonian legislation took account of several medical and surgical affections, and its medico-legal judicature was very severe on surgeons guilty of malpraxis, or who even were merely unsuccessful in the performance of operations.

Dr. Albert S. Ashmead, of New York, in discussing "The Question of a Relationship between 'Syphilitic' Llamas of the Department of Puno,

Peru, and Pre-Columbian Syphilis in Man,"[1] says, "The Japanese race has been saturated with syphilis for 1,300 years; the Chinese race since 1124 B.C."; and he supposes that these races, "by their long generational experience with the disease, have acquired a great measure of resistance to the germ."

The descriptions given of venereal diseases in the centuries between remote antiquity and mediaeval times, by Greek, Latin, Arabian, and other authors, concern non-specific ulcers, buboes, and the brenning or burning, by which was meant gonorrhœa; and the inferences drawn from them as to the existence of syphilis are attributable to the confusion arising from grouping all affections which spring from commerce between the sexes under the general term "venereal disease."

Dr. Norman Moore, adopting the conclusions of the three learned men whom he quotes, namely, Jean Astruc, Professor of Montpellier, Dr. John Freind, and Van Swieten, is of opinion that further researches into ancient writings, even by scholars possessing the greatest linguistic attainments in Greek, Latin, and Oriental languages, is not likely to lead to the discovery of proof of the prevalence of syphilis in early times. He regards the existence of the disease in those times as not proven; and indeed negative evidence is all that can be expected or which is possible if there really was no such disease in antiquity. It might then be said of syphilis as Mark Twain said of Shakespeare, whom he speaks of as being "Just a tar baby"[2]: "We can go to the records and find out the life-history of every renowned racehorse of modern times—but not of Shakespeare. There are many reasons why, and they have been furnished in cart-loads . . . but there is one which is worth all the rest of the reasons put together—he hadn't any history to tell."

But we can look to other records besides those written in books, and eaten into the bones of men, as I will show presently.

Dr. Norman Moore considers the endeavour of Francisco Lopez de Villalobos in 1498 to identify syphilis with the condition described by Avicenna (980-1037) in the fourth book of his "Canon of Medicine," under the name of saphati, as altogether unconvincing; and though he inclines to the belief that the period of the first appearance of syphilis in Europe was the end of the fifteenth century, yet he thinks much more evidence than is generally set forth is required before we can accept the statement that this disease was imported from America by Columbus.

[1] *Amer. Med.*, Burlington and New York, 1909, xv, p. 35.

[2] Mark Twain's "Is Shakespeare Dead?" 1911.

SYPHILIS IN MAN AND ANIMALS INDIGENOUS TO SOUTH AMERICA.

It seems to me that further evidence has been brought forward within the last fifteen or sixteen years by Dr. Ashmead in support of the views (1) that syphilis prevailed in America as well as in Japan in very early times; and (2) that in America it was a disease of animals as well as of man; Dr. Ashmead[1] quotes David Forbes, a great authority on everything connected with the native Indian population, to show that the very special and fundamental racial characters of the native Indians of Bolivia and Peru are in complete accord with the nature of the countries they inhabit, and have needed *ages* for their evolution. Hence, concludes Forbes, these people—the Aymara Indians—must have been established in these countries for a very long time. Of the three great tribes of South American Indians the Incas were the chief, and were the conquerors, in the eleventh century, of the second greatest tribe, namely, the Aymaras of Bolivia and Peru. It is in the Aymara Indians that the indications of what may be called autochthonous syphilis are strongest, and, as Dr. Ashmead remarks, the conclusion which may be drawn from these facts is "that the probabilities of the existence of syphilis in this part of the world before Columbus are almost overwhelming."[2]

Forbes never met with any instance of an Indian disfigured by the disease, their treatment of it being successful. They employ mercury (metallic form) and calomel from native cinnabar. Chewing coca is said to prevent mercurial salivation.[3] He thinks syphilis must have been known among these Indians from a very early period: (1) because they have in their language a name for this disease; (2) because they are quite familiar with its treatment; and (3) because skulls are occasionally taken out of graves dating from the period antecedent to the Spanish conquest on which may be seen depressions or scars pronounced by several medical men to have resulted from syphilitic caries, and which in two instances which came under his (Forbes's) observation afforded proof that the disease had been arrested in its progress and new bone formed during the lifetime of the individual.

The same writers tell us that the llamas of Peru, and the alpacas (animals peculiar and confined to the highlands of Bolivia and Peru)

[1] *Journ. Cutan. and Gen.-urin. Dis.*, New York, 1895, xiii, pp. 415, 416.

[2] *Ibid.*, p. 417.

[3] *Ibid.*, p. 416.

suffer extensively from a disease which is identical with syphilis in man and which is healed by the Indians by a precisely similar mode of cure, consisting principally of inunctions with mercurial ointment; and Forbes says: "The mortality among the alpacas caused by the disease when not extremely carefully treated, is said to be very great indeed, and the bones of the diseased animals are stated to be very much affected by caries exactly as in man. The question whether this disease may have been communicated from the alpaca to man, or vice versa, is an open one. It is well known that such unnatural intercourse is common, and that under the Incas several laws were enacted against it. Even after the Spanish conquest an old law not permitting the llama drivers to start on their journeys unless accompanied by their wives was retained in force, and this regulation was understood to be intended as a safeguard against such abuses."

Commenting on two unsuccessful inoculations of llamas with human syphilis, Dr. Ashmead[1] asks: "May it not be that if the disease" [syphilis] "in pre-Columbian America was originally an animal disease, that there may be breeds of llamas whose disease we suspect it to have been, who have by this time acquired really a measure of resistance or immunity, just as special breeds (Chinese and Japanese) of human beings?"

The llamas of the department of Puno, Peru, were called by the natives "syphilitic llamas," and Dr. Ashmead proposed that other experiments on the Incan and Aymaran Indians of Peru with the virus taken from one of the syphilitic llamas should be made, with the view of determining whether the disease supposed by the natives to be animal syphilis was in reality human syphilis or not. If these experiments have been made, the results, so far as I can find, have not been published.[2]

Dr. Ashmead says that human syphilis as it occurred in Japan in the seventh century was a veritable epidemic, as it was in Western Europe in the fifteenth century, and he continues: "Writers of these periods, in both geographical situations, claim that the disease was veritably epidemic then, and was quickly followed by a spread of leprosy. In Japan it is true, for I have translated several Japanese works on the subject and know it to be a fact."

[1] Dr. Alfred S. Ashmead, *Amer. Med.*, 1909, xv, p. 36.

[2] See *The Medical Fortnightly*, St. Louis, 1909, xxxvi, p. 348.

INFERENCES FROM THE POTTERY OF THE INCAS.

Proof has been sought in the pottery of the Incas of the existence of syphilis in Bolivia and Peru in pre-Columbian and in even very remote times. It is admitted that the Inca pottery and the types of the representations of human features thereupon are of much earlier date than the Spanish conquest of these countries; but the cause of the deformities and disfigurements of the human faces portrayed on these ceramic vases is a question still in dispute. Were they the results of leprosy, of lupus, of syphilis, or of mummified changes after death? Virchow thought they were a strong argument in favour of a pre-Columbian lepra, and he emphasized the fact that the similarity of the changes which lepra and lues brought about on the surface of the body was often very marked, and that in the Middle and later ages mistakes as to the identity of the two conditions occurred. He then made a special point of stating that he himself had never seen a syphilitic bone dating from the pre-Columbian era.

In 1895 Dr. Ashmead reopened the question by a letter to Virchow, and since then has published several communications on the subject of the Inca pottery and pre-Columbian syphilis in America. He is of opinion that these ceramic representations are of syphilis, not of leprosy. One of his correspondents, Professor A. F. Bandelier, a resident of Lima, Peru, and having a first-hand profound knowledge of the country and its inhabitants, wrote[1]: "Syphilis and related infirmities are common everywhere among the Indians as well as among other portions of the inhabitants. The disfigured faces on the pottery are generally regarded as representing that disease, and I never heard leprosy mentioned in connexion with them. In addition to the kind of vases representing such phenomena, others are found of a still more obscene character, and one of them I sent to the Museum from Chan-Chan showing man in connexion with the llama . . . The idea of copying on ceramics the effects of syphilitic affections upon human faces is, without any doubt, pre-Columbian in Peru . . . That even quite a number of vessels representing syphilitic diseases should be post-Columbian is therefore only a proof of the fact that they are survivals of artistic ideas carried out long previous to the Columbian era, and that hence the disease existed in Peru untold centuries ago as well as it exists to-day."

[1] Quoted by Dr. Ashmead, *Journ. Cutan. and Gen.-urin. Dis.*, New York, 1896, xiv, pp. 56, 57.

Dr. Ashmead[1] quotes also Mr. Teoberto Maler, who has been exploring sepulchres in various parts of Yucatan (a peninsular of Mexico between the Gulf of Mexico and the Caribbean Sea) for many years, and who is the correspondent of the German Societies. This gentleman wrote in 1895 of syphilis: "According to the ancient Spanish historians it seems, without any doubt, that syphilis is an original American disease, and the Spaniards found it for the first time among Indians of Haiti, Cuba, &c. It is also true that the ancient Peruvians imitated frequently in clay figures syphilitical accidents—for instance, human faces with the nose eaten away, &c. I saw at Paris, in the Ethnographical Museum of the Trocadero, many interesting specimens of Peruvian pottery of this kind. This naturally does not exclude that the same disease existed also in China or Eastern Asia in very remote times." Of leprosy Mr. Maler says: "It only exists in the Spanish class and mixed people. I never saw or heard of a true Indian family affected with it. . . . The true Peruvian antiquities refer, as I believe, to syphilis and not to leprosy, but without doubt leprosy existed in very ancient times in most of the Asiatic lands."

Dr. Ashmead is of the same opinion, for he writes[2]: "The representations which I have found on the Huacos pottery in the American Museum are, in my opinion, not leprosy, but syphilis"; but he seems to imply that both syphilis and leprosy were very ancient Asiatic diseases, and that they reached America from no other place but East Asia.

Professor Eugen Hollander, of Berlin, takes a different view of the representations on the Inca pottery. In a paper read at the Budapest Congress in 1909, after remarking that the Peruvian amphoræ are of a pre-Columbian epoch, he says that the deformities of the central portion of the human face represented upon them are held by different scholars and physicians, with great divergence of opinion but with equal confidence, to be the result of destruction by lupus, by lepra, and by lues; so that there is much doubt as to the critical estimation of these objects. He himself attempts to show that the mutilations depicted do not represent disease, but are death portraitures of mummies, and in certain instances symbolical representations.

Of the various specimens of Inca pottery on view in the Ethnographical Department of the British Museum, and others, not exhibited, which were kindly shown to me by Sir Hercules Read and Mr. Athol Joyce in their workrooms at the Museum, one only appears to be in any manner a portraiture of a syphilitic face. In looking at these specimens,

[1] *Ibid.*, p. 95.
[2] *Ibid.*, p. 53.

as well as the much larger collection in the Trocadero Museum, Paris, one has to bear in mind that cutting away the nose and upper lip was a form of punishment among the natives; so that the character of the remaining edges, as well as the loss of these features, should be taken into account in judging whether the mutilations represented are due to disease or injury.

If syphilis was known in America, and was also prevalent in Japan and Eastern Asia, in early times, it is a question whether the disease was transmitted from Asia to America or from America to Asia. But if the natives of South and Central America had no communication with the outside world previous to the arrival of Columbus, as the writings of Henry C. Mercer, Teoberto Maler, and D. G. Brinton,[1] afford good ground for saying was the case, syphilis in America must have had an independent and distinctly separate origin ages before America was visited by Columbus.

A Summary of Dr. Ashmead's Evidence.

If we pass in review the evidence put forward in Dr. Ashmead's communications in favour of the existence of syphilis, in early times, in America and the eastern parts of Asia we find it embraces: (1) The traditional belief that many of the figures on the early specimens of Inca pottery were representations of the destructive processes of syphilis. (2) The statements of scientific investigators who are familiar with the countries and language of the native Indians that syphilis certainly existed and flourished in America in pre-Columbian times. (3) That there is no positive trace of pre-Columbian leprosy in America. (4) The traditional belief that the llamas and alpacas suffered extensively from a disease identical with syphilis in man. (5) That the Indians cured this disease, both in animals and man, by a treatment consisting principally of inunctions of mercury. (6) The accounts in Japanese books of syphilis in Japan in the seventh century, which books Dr. Ashmead has himself translated. (7) The failure of Dr. Ashmead to inoculate with human syphilitic virus either prostitutes in Japan or llamas in Peru. Dr. Ashmead had in his clinic in Tokio, Japan, charge of 2,000 licensed prostitutes; he tried, but invariably failed, to inoculate with syphilitic virus harlots who had had syphilis. This showed, he thinks, that a syphilitic could not have a chancre twice. In this way he was able to determine the exact nature of a suspicious sore. Chancroids would repeat themselves; chancres never did.

[1] Quoted by Dr. Ashmead, *ibid.*, pp. 94, 95, 97, 98.

A letter from Dr. Francisco Grana, of Lima, Peru, to Dr. Ashmead[1] tells of a treatise just completed by Tello,[2] on syphilis during the Inca period, based on bibliography and *numerous anatomical specimens*. The specimens included several skulls which display exostoses, believed to be of a syphilitic character; they were taken from tombs (Huacos), together with pottery, textiles, and other objects. These skulls (as well as the human skulls and the bones of alpacas referred to by Forbes) contradict the conclusion to be drawn from the statement made by the anthropologist to the Smithsonian Institute in Washington, which is quoted by Dr. Norman Moore.

Is it not possible that the extreme rarity of bones of ancient skeletons showing changes indicative of syphilis is due to the free use of mercury in antiquity? The ancient use of mercury in a given country, however, by no means necessarily implies that syphilis was a disease of the inhabitants; because it was largely used internally and externally, for internal complaints, for skin eruptions and diseases, and as a germicide for lice and other vermin. In China, Japan, South and Central America, and in India this mineral was much employed as a remedy. Mercury is found in China. In India its wonderful powers caused it to be considered by the followers of a religious doctrine called Ruseshism as "one of the manifestations of God."[3] The Romans and Arabs used it externally. The Hindus and the Chinese from very early times took it internally in medicine. The Yogis of India drank twice a month a potion of sulphur and quicksilver which they said gave them long life.

Is it not probable that as syphilis is known to be much milder in character in hot countries and in the Tropics than in more northerly and colder climates, tertiary syphilitic changes in bone rarely occurred in such countries. And if with a milder form of disease there are associated the prevalent use of mercury and the great measure of resistance which long generations of infection with the disease begets, have we not an explanation of some importance why these bony witnesses of ancient syphilis are wanting or very scarce, even in hot countries where neither circumcision, depilation, regular use of baths, &c., after coition, or other conditions which prevent contagion were commonly in force?

[1] *American Medicine*, 1909, xv, p. 37.

[2] See *Annales de Dermatologie et de Syphilographie*, 5th sér., 1911, ii, p. 125. See also *La Chronique Médicale*, 1911, xviii, p. 489, and *The Medical Fortnightly*, St. Louis, 1909, xxxvi, pp. 347 *et sqq*.

[3] "History of Aryan Medicine," by Sir B. Singh Jee, K.C.I.E., Thakore Saheb of Gondal, p. 146.

Evidence from Contemporary Writers that Syphilis was Imported in 1493.

The Columbian origin of syphilis in Europe would be disproved by showing either the non-existence of syphilis in the New World in pre-Columbian times, or the existence of the disease in Europe prior to the return of Columbus in 1493. Having dealt with the former, I will now say a few words on the latter question. The principal evidence upon which is based the belief that syphilis was first introduced into Europe from Haiti in 1493 are the writings of three distinguished contemporary physicians, an Italian and two Spaniards. Dr. Norman Moore has quoted at length from the work of the Italian (Girolamo Fracastoro) Hieronymus Fracastorius, so that I will make no further reference to it. The two eminent Spanish physicians are Francisco Lopez de Villalobos and Diaz de Isla. They furnish strong proof of the importation of syphilis from Haiti by the fleet of Columbus. De Villalobos, whose habits of life exposed him to the risk of being himself an example of the disease, published his book, "Sumario della Medicina," in 1498. This work contains a supplement on syphilis (entitled "Un Tractado sobre les Pestiferas Bubas") which he calls "pestiferous bubas," and it is important to notice that it is quite separated from his account of "imposthumes," ulcers of the genitals, and gonorrhœa, which is contained in the body of the work. The book is a metrical composition, for like Avicenna (980-1037) before him, and Fracastorius (1530) later, de Villalobos adopted the plan of writing in verse. Only two copies of it are known to exist, one in the Royal Library of Madrid, the other in private hands. Its contents were made known by Dr. Bonifacio Montejo, of Madrid, in the later half of last century.

De Villalobos describes the primary, intermediate, and secondary symptoms of syphilis; speaks of it as being a "new and contagious" disease; and mentions that the period when it first began to attract attention was in 1495, or a little earlier. The impression that de Villalobos made any endeavour to identify syphilis with saphati must, I think, have arisen from his having spoken of the secondary rash as "the Egyptian itch," and his comparison of the pruritus of some syphilitics with that caused by the Egyptian itch. As a matter of fact, he took great pains to make clear the diagnosis between syphilis and the dartrous affection known as the saphati of Avicenna, and which was the frequent precursor of leprosy. With much emphasis he states that the pimples of saphati do not infect, as do those of syphilis, and are of a brighter red colour.

Dr. Bonifacio Montejo has also directed attention to the authentic copies of Diaz de Isla's book (dated 1539 and 1542). The work had previously been little known in Europe except through the faulty Latin translation of Welschius.[1] Diaz de Isla, whose book was apparently written and completed in 1510, speaks of syphilis as having first shown itself in Barcelona, and as having been brought to Europe from Haiti in the middle of April, 1493. Like Fracastorius and de Villalobos, he regarded it as "hitherto unknown, entirely new, and never before read of in books of medicine." He tells that he had under his own treatment, before they arrived ashore, men belonging to Columbus's fleet and others who were suffering from syphilis, and that he also treated in Barcelona, prior to the King of France going to Naples, many patients affected with the disease. He explains how the French camp in Italy became infected by many Spaniards joining it from Barcelona.

Objectors Refuted.

By way of discrediting this evidence it was pointed out that Christopher Columbus landed at Palos, some miles south-west of Seville, and tarried at Seville a fortnight before proceeding to Barcelona. Why did the disease not break out in Seville, it was asked? There is evidence to show that it did. A Sevillian physician, Monardes by name, writing in 1580, says syphilis was called "Serampion de las Indias"; and Dr. Montejo has shown that as early as 1502 there was a hospital set apart for the treatment of the syphilitically affected in Seville and that the disease was called "serampion de las Indias" because, as the record has it, "when this hospital was built there was no such disease, for it only became known after the discovery of the Indies in 1492."[2] For further details of the writings of these Spanish physicians I refer the reader to Mr. George Gaskoin's articles in the *Medical Times and Gazette* for 1867.

Other evidence to discredit the Columbian importation of syphilis into Europe from Haiti was brought forward in the letters of Petrus Martyr, who made out that the disease was known in Spain in 1488, and in Bodmann's "Antiquities of the Rheingau," containing reference to a case as having occurred in Europe in 1472; but both statements have been proved to be forgeries.[3]

[1] George Gaskoin, *Med. Times and Gaz.*, 1867, ii, p. 90.

[2] "Siglo Medico," December, 1860, quoted by George Gaskoin, *Med. Times and Gaz.*, 1867, ii, p. 63.

[3] *See* George Gaskoin's articles in the *Med. Times and Gaz.* for 1867, ii, p. 201, and Iwan Bloch's "History of Syphilis" in "A System of Syphilis," edited by D'Arcy Power and J. Keogh Murphy, i, chap. 1.

SYPHILIS PROBABLY IN ENGLAND LONG PRIOR TO DISCOVERY OF AMERICA.

But notwithstanding all these facts in support of the Columbian importation of syphilis into Europe, it is impossible to shut one's eyes to other facts which point to the existence of the disease in England, France and other countries a century and a half before Christopher Columbus set out in search of the New World. In England, for example, John of Gaddesden (1340), in his work "Rosa Anglica," when referring to the infection of leprosy from coitus, told of the precautions which should be taken to prevent infection. There is ample proof that there was a contagious venereal disease which went by the name of leprosy; that so-called leprous men and women were capable of communicating an infectious malady to those who had carnal connexion with them; that after coition the symptoms always first displayed themselves in the parts through which the poison was conveyed; that only when the pudenda were diseased did the leprous woman convey the infection in coition; that in true leprosy those parts are not disordered; that the disease conveyed by a (so-called) leprous person in coition was followed by the breaking out of scabs and ulcers all over the body, and bore a greater similitude to leprosy as it is described by the ancients than to any other disease; that some ancient writers, as I stated in my introductory remarks, endeavoured to prove that the pox is only one species of leprosy, and that there may be a transition from one of these diseases into the other; that the rebellious nature of these cases of so-called ulcerous and scabby leprosy led surgeons to try a great number of remedies, but that all of them were useless unless mercury was joined with them; that as the dressing of such ulcers was tedious the patients were ordered to daub the ointments over the sore and cover them with linen till the next dressing; that following this treatment the ulcers were in a little time healed, the patient's mouths became sore, but that otherwise the patients were cured. It was in this accidental way that the method of salivating by unction was discovered. It is thus obvious there was a disease which went by the name of leprosy, which was communicated to man by coition with a (so-called) leprous woman, but which was not in reality leprosy. It is also now well known that many deaths from venereal disease were formerly ascribed to leprosy; that leper hospitals were numerous in England up to the time when syphilis was distinguished from leprosy; and that as more and more patients were recognized as syphilitic, and treated as such, the demand on the leper

hospitals correspondingly diminished. In France, as shown by Raymond, of Paris, many syphilitics were buried as lepers in the leper cemeteries of the Middle Ages.

Another Englishman, Bartholomew Glanville, who flourished about the year 1360, wrote a book ("De Proprietatibus Rerum") describing as leprous, persons with typical tertiary syphilitic symptoms. Among the causes of this sort of leprosy he enumerates the ordinary modes of syphilitic contagion; he describes the infection passing from father and mother to the child, "as it were by Lawe of Herytage"; and speaks of a "childe fedde wyth corrupte Mylke of a Leprouse Nouryce" as being another way of infection. In a word Glanville, in the middle of the fourteenth century, "under the name of one species of the leprosy, gives a summary of the symptoms of the pox, and the several ways whereby it is at this time communicated."[1]

When we think of the lack of facility for travel, of the smaller urban population, of the fewer opportunities of irregular and indiscriminate sexual intercourse, and consequently of the fewer cases of venereal diseases in those days; and when also we remember how comparatively little opportunity there was for interchange of professional opinion and experiences, we can understand how medical practitioners overlooked the association and causal relationship of secondary and tertiary syphilis with a local sore contracted weeks before, which had healed and to all appearance was finally cured, and perhaps had even been forgotten. It is thus also intelligible how medical men came to associate those syphilitic symptoms with the symptoms of leprosy—the disease which seemed to them to have the greatest analogy to the constitutional and local manifestations of what we now recognize as syphilis.

Before surgeons could regard the primary chancre as the cause of the subsequent symptoms of syphilis, and syphilis as a separate and distinct disease, the lessons and the experience of some such group of conditions as those which attended and followed the Columbian importation of the disease were perhaps necessary. Cause and effect would be less likely to be overlooked when all phases of the disease were seen in patients confined on board ship during a long voyage; when the disease spread among troops living near cities and massed together in large numbers; when the course and progress of the disease were intensified and exaggerated by the exposure, deprivations, and hardships of war; and when, perhaps in part owing to the more severe action of

[1] "A Letter concerning the Antiquity of the Venereal Disease," by William Becket, *Phil. Trans.*, 1720-21, xxxi, p. 59.

a virus taken from one race and infecting a different race, the virulence of the disease assumed, as we are told it did in 1493-95, the characters of an epidemic.

Variations in Virulence at different Periods.

The virulence of syphilis has varied not only as between races, but in the same race at different epochs. Dr. Ashmead, speaking from personal experience, says that "a Japanese prostitute whose symptoms may appear very mild would always inoculate the European with a vicious type of the disease, even producing in him the type of epidemic syphilis of the fifteenth century in Europe." This he thinks is because of the shorter generational experience, extending to only 500 to 600 years, and the consequent inferior measure of immunity or resistance of Europeans as compared with other races—like the Japanese and the Chinese, who have been saturated, according to Dr. Ashmead, with syphilis for from between 1,300 to 3,000 years. The disease in the seventh century in Japan was of great virulence, and is said to have been like the epidemic in Europe in the fifteenth century. Fluctuations at other epochs have been largely caused by fluctuations in the nature of the treatment. In modern times, when iodide has supplanted mercury, syphilis has been seen in aggravated forms. During my own professional life the frightful cases of skin, bone and visceral syphilis which one used to see have disappeared under the systematic administration of mercury and the employment of antiseptics.

Shakespeare and Syphilis.

Mr. Hutchinson pointed out that I had omitted from my introductory remarks any reference to one of Shakespeare's best allusions to syphilis. No doubt in the quotation referred to by Mr. Hutchinson in the fifth act of "Henry V," Pistol, when speaking of "the spital of malady of France" was referring to a hospital for venereal diseases, and equally, no doubt, alluded to syphilis as "the malady of France"—"Morbus Gallicus" being one of its many names. My object, however, was not so much to make mention of all the passages in Shakespeare's plays in which some sort of allusion is made to syphilis, as to point out his frequent use of the words "Pox" and "Pox on't" in the manner of an oath and in much the same way as some persons are in the habit of saying "Damn" and "Damn it" in the present day. That Shakespeare, when using the word "pox" nearly always, if not invariably,

meant syphilis, is to my mind quite certain; and this use of the word as an execration, or an ejaculation implying disgust, shows how uppermost in men's thoughts, and how common, the disease was in his day. Elsewhere Shakespeare uses the word "Goujees" meaning "pox," as in Act v, scene 3, "King Lear"; and "Good Year" in the same sense as "Goujees" and "pox" as in "Henry IV," Pt. 2, Act ii, scene 4.

As regards Shakespeare's graphic account of syphilis in "Timon of Athens," and his repulsive allusion to the female sexual organs in "King Lear," why should we associate them with Shakespeare's own personal experiences? The characters in a drama must not be supposed always to represent incidents in the life of the author, any more than does the hero of a novel. The author made out Timon to be not merely a misogynist but a misanthrope, bearing a bitter grudge against not only women but all mankind, because of the ingratitude he had experienced in men; King Lear he described as becoming mad because of the ingratitude of his daughters.

As opposed to the attempt to associate these descriptions with the individuality of the author, or to connect the physiognomy and parentage of Sir William Davenant with Shakespeare, I would point out that if Shakespeare was the author and actor of the plays which go by his name there is nothing in the portraits of the dramatist, nor, so far as we know, was there anything in the voice of the actor, to suggest that he himself was the parent of a congenital syphilitic offspring or the subject of facial and laryngeal syphilis, so realistically described by him. If, on the other hand, William Shakespeare was too illiterate to have written the plays, what will the advocates of the Baconian authorship of them say to the insinuation that the writer of the Novum Organum, and the editor of the authorized version of the Bible of 1611, was describing his own diseased plight in words he made Timon of Athens, in the play of that name, utter?

Does Syphilis heighten the Imagination?

Mr. D'Arcy Power has asked me to ascertain from the psychiatrists the nature of the disease from which the patient was suffering who is described by Horace (Epist. Lib. ii, Epist. ii, ll. 128-30):—

> "Fuit haud ignobilis Argis,
> Qui se credebat miros audire tragoedos
> In vacuo laetus sessor plausorque theatro."

Here was a man whose delight it was to sit in an empty theatre and applaud the wonderful tragedians to whom he imagined he was listening.

But any suspicion as to this delusion having been caused by syphilis is, I think, removed by what Horace tells us a few lines further on :—

"Hic, ubi cognatorum opibus curisque refectus
Expulit elleboro morbum bilemque meraco
Et redit ad sese."

The man was cured by means of pure hellebore, which, so far as I am aware, has never had ascribed to it any curative influence on syphilis. If it were syphilis from which he suffered this patient's recovery lends a degree of support to Dr. Pernet's suggestion that syphilis may have played some part in the development of the musical genius of Beethoven, who was said to have been a congenital syphilitic ; for Horace goes on to say :—

"Pol, me occidisti, amici,
Non servasti, ait ; cui sic extorta voluptas,
Et demtus per vim mentis grastissimus error."

When the patient recovered owing to the treatment he had received at the expense of his relatives, he exclaimed, "By Heavens, friends, you have destroyed, not saved me ; to rob me thus of my pleasure, and take from me by force such a very agreeable delusion of mind."

The Bearing of "606" and Wassermann's Reaction on Tabes and General Paralysis.

Dr. Mott would lead us to the conclusion that the tardy, delayed, or late syphilitic manifestations of disease of the nervous system which are usually termed parasyphilitic and metasyphilitic have increased in these times, and for two reasons : (1) The struggle for existence involving more and more the nervous system makes the cerebrospinal centres the "*locus resistentiæ minoris*" of disease ; and (2) a widespread racial immunity to the severer forms of secondary and tertiary syphilis has been brought about by the general syphilization of the race. This racial immunity is attributed "to the reaction of the cells of the body to the syphilitic virus." The parasyphilitic affections of the nervous system such as tabes and general paralysis of the insane are generally looked upon as degenerative processes and as such are uninfluenced by antisyphilitic treatment. If the same degree of racial syphilization which diminishes the severity of secondary and tertiary forms of syphilis is a condition favourable to degeneration of the cells of certain parts of the spinal cord and brain ; and if, as we are told, no case of syphilis has ever been really cured by mercury, can we look to salvarsan to prevent latent syphilis and the reaction of the

body cells before the degenerative process has been established? If not, the race is in a parlous state, in spite of the advent of "606." If tabes and general paralysis of the insane come on after about the same number of years whether mercury has been given or not, will experience teach us that they will do the same after salvarsan? That they will not occur in persons who have been treated with salvarsan from the outset of syphilitic infection is our only hope of abolishing tabes and general paralysis. We must all join Dr. Mott in wishing "that the early administration of '606' followed by mercury may in a measure do what mercury alone has in a measure failed to do, as regards averting parasyphilitic affections in later life—viz., it may by rapidly and effectually killing off the spirochætes in the blood and lymph prevent the hypersensitizing of the cells of the body and excessive defensive reaction." It may in time, we hope, cause desyphilization of the race. If these parasyphilitic nerve affections were not the result of nerve cell degeneration we might have looked for some improvement from appropriate treatment of them; but how can we expect degenerated nerve cells to recover? A house which has been burnt down may be rebuilt, and the fire may be prevented from spreading to surrounding property by suitable means. But if the degenerative effects of nerve cell reaction to the syphilitic virus have once commenced and the process cannot be influenced by treatment, neither repair of the parts already damaged can occur nor can the cell destruction be prevented from spreading. In other words, tabes and general paralysis, so far as they are due to latent syphilis, will neither be cured nor prevented by any remedy as yet known to us.

Time alone can reveal whether the use of salvarsan at the very outset of infection by the virus of syphilis will ultimately annihilate tabes and general paralysis of the insane; or whether the mere introduction for the briefest period of time of the spirochæte into the tissues and the blood will in a certain proportion of persons in a syphilized race light up the cell reaction which ultimately produces these diseases.

Does Racial Immunity to Active Syphilis favour Tabes and General Paralysis?

If the prevalence of syphilis for many generations, and the systematic treatment of the disease by mercury, create in a race a degree of immunity to severe forms of syphilitic lesions, but at the same time a tendency to cell degeneration, there ought to be evidence forthcoming

in support of these statements from Japan, China, and South America. That there is a degree of racial immunity to severe forms of syphilis in the Japanese seems indicated by the experience of Dr. Ashmead, which I have previously quoted. Is there also a great tendency in that race to cell degeneration either of the nervous system, as shown by tabes and general paralysis, or in the arterial or any other great anatomical section of the body? Is syphilis the only, the exclusive, cause of tabes and general paralysis? Since 1881 there has been an increasing tendency so to regard it. The first recognition of the intimate association of syphilis and general paralysis was made in countries (the Scandinavian) where syphilis had for a long time been a notifiable disease. Dr. Mott is of opinion that syphilis is the commonest cause of nervous diseases in general. Sir George Savage points to certain cases in which head injury in syphilitic cases had been the exciting cause of general paralysis of the insane, and he is uncertain as to whether or not head injury without syphilis can produce it. But he is not logically entitled to draw the conclusion that it is proved that it is not syphilis "alone or always which produced general paralysis of the insane," because among certain tribes and isolated collections of men "syphilis was all but universal, but general paralysis absent." The inverse condition might have justified this conclusion—namely, if among these groups of persons general paralysis had been almost universal and syphilis absent. Sir George Savage has constantly observed in general paralysis a good memory with great mental loss. Such retention of memory is not common in parasyphilitic affections. He has also noticed certain forms of mental degeneration associated with syphilis, starting in some local symptoms, such as ptosis, aphasia, or a local palsy which passed off in some cases completely, and the cases did not end in general paralysis. On the other hand, some such local brain lesions frequently usher in general paralysis. He goes no further than to say that his belief is that syphilis is a great cause of general paralysis.

Wassermann's Reaction as a Specific Test for Syphilis not yet fully Understood.

Dr. Mott tells us (1) that the spirochæte—the special organism producing syphilis—has never been found in the lesions of general paralysis; (2) that general paralysis occurs in persons in whom the antecedent signs of syphilis are usually slight and often entirely absent; (3) that with an experience of 500 post-mortems in bodies diagnosed as being

the subjects of general paralysis, those in which gummatous or other severe syphilitic lesions existed turned out to be pseudo-general paralysis due to gross syphilitic lesions of the membranes and vessels of the brain. Assuming these to be indisputable facts, are they not strong evidence against, not in favour of, general paralysis being a disease of syphilitic origin and due to latent syphilis? On what is based the present reliance in the syphilitic causation of this disease? Is it not to-day the Wassermann reaction? And does not this assume that syphilis, and syphilis only, gives the reaction? And is this assumption warranted by facts and by what is known at the present time? Dr. Mott also tells us (4) that he has never seen a primary sore or a secondary eruption in a general paralytic person; and (5) that Krafft-Ebing has inoculated the virus of a hard chancre into nine general paralytic persons who had never shown any sign or given any history of syphilis, yet not one of them was infected. These two statements (4 and 5) will be held to be proof that these persons were syphilized already and were therefore immune to reinfection with the syphilitic virus. In support of this there is the rarity of a second attack in the same person of ordinary acquired chancre, and the failure of Dr. Ashmead to inoculate Japanese harlots with the virus from a chancre.

And here may be interposed the following question: Are those who are syphilized already the only persons who can resist the infection of syphilis?

Are we justified in asserting that the only state of the body which can resist infection with syphilitic virus is one of already existing syphilization? If I understand Dr. Mott aright, he is of opinion that in the case of a widespread infectious disease, sooner or later the virulence of the disease dies down because there are increasing numbers of the population (1) in whom the local infective process is so mild as to be unobserved; (2) or who have the disease in the primary and secondary stages but in a very mild form; (3) or who possess a complete immunity. If this be so, why suppose that every general paralytic who has been exposed to infection of syphilitic virus, either by experiment or in the ordinary manner in which syphilis is acquired, but who does not take the poison, has previously had syphilis and still has it in a latent form? Why not suppose the third alternative—namely, the possession of complete immunity—as the explanation in many cases? Mr. McDonagh will reply, "There is no such thing as immunity"; either a person has not had syphilis before and will take the infection if exposed to it, or being syphilized already he does not take it afresh. This argument is,

of course, in favour of the syphilitic origin of general paralysis when applied to the statements (4) and (5) mentioned above, but is it not a *petitio principii*? Does it not beg the very question in dispute? To my mind, however, this argument, erroneous as it is, is more consistent with facts than the inference as to the existence of syphilis drawn from the positive Wassermann reaction. This reaction, it would seem, is now very generally accepted as a reliable test of the presence of syphilis. It was confirmed in 107 out of 110 cases of general paralysis. In general paralysis there is a very marked reaction of the blood and cerebrospinal fluid in nearly 100 per cent. of cases. No case diagnosed as one of paralysis which gave a positive result with Wassermann's test was found to be other than general paralysis at autopsy, and this in spite of the fact that in paralytics "the signs of antecedent syphilis are usually slight, and often entirely absent."

Of the value of the Wassermann reaction in the diagnosis of general paralysis Dr. Mott says there cannot be the slightest doubt. But it is a long jump from the almost cent. per cent. positive Wassermann reaction in general paralysis to the conclusion that mild or latent syphilis is the cause of this reaction in cent. per cent. of cases of general paralysis. When Dr. Mott tells us of a large number of cases in his own experience of syphilitic cord and brain disease which fifteen years ago he believed to have been cured, but which subsequently died from syphilitic complications after having been paralysed, we are on safe ground. But what proof is there in many other cases, beyond the Wassermann reaction, that the patients have had syphilis in any form or degree whatsoever? Mr. D'Arcy Power points out that the reaction is not given by every patient even when there are obvious signs of syphilis, and adds that it is positive in 95 per cent. of cases of secondary, in 75 per cent. of tertiary syphilis, and only 50 per cent. in latent syphilis; yet, as stated above, the reaction is positive in almost 100 per cent. of general paralysis. How is it then there is this difference, that in the latent cases in which general paralysis has developed the reaction is positive twice as often as in latent syphilitic cases in general?

Dr. Mott says an important distinction of parasyphilis from syphilis of the nervous system is that in the latter a positive reaction of the cerebrospinal fluid is obtained in less than 20 per cent., whereas general paralysis gives the reaction in 97 per cent., and tabes in 60 per cent. Mr. McDonagh tells us that in his experience not more than 70 per cent. of mothers of syphilitic children give a positive Wassermann reaction; that if a provocative injection of salvarsan is given to such

women a positive reaction afterwards is the rule; he considers that every woman who bears a syphilitic child is the subject of latent syphilis, and tells us that such women not infrequently develop their first syphilitic symptoms as tertiary manifestations at or about the menopause, but that during the child-bearing period the Wassermann reaction may be negative even if the mother has just begotten an undoubted syphilitic infant which gives a positive reaction. Here, then, we have an accepted test for syphilis giving the most intense positive reaction and the most constant positive reaction in what are called parasyphilitic cases; whereas the reaction is not given in the first days following infection with syphilitic virus, nor in 5 per cent. of secondary syphilis, nor in 25 per cent. of tertiary, nor in 50 per cent. of latent syphilis; it occurs in 70 per cent. of mothers, all of whom are said to be syphilitic and liable to develop tertiary symptoms later in life, or it may be absent in the mother and present in her newborn infant, or vice versa, or it may be absent from both the mother and her newborn syphilitic infant; and finally, in contrast with the intensity and frequency of the reaction given by general paralytics, the positive reaction is far less intense and many times less frequent in actual and unmistakeable syphilis of the nervous system.

The parasyphilitic diseases are believed to be more prevalent now than formerly, because of the widespread racial immunity to and latency of syphilis. Thus the position of the Wassermann test for syphilis is this, it gives the most intense reaction *in latent syphilis associated with general paralysis;* when the primary infection and secondary symptoms have been so mild as to have escaped notice; and when the conditions which cause attenuation of the virus and produce racial immunity—viz., (*a*) the continuance of mercury treatment over a period of years, (*b*) the control and suppression of suppurative processes by means of antisepsis and asepsis, and (*c*) teetotalism—have been in force, than when syphilis is present in an active form. In short, and in a sentence, the milder the syphilis—even to the degree of its having escaped the notice of the patient so that he may be quite unaware that he had ever had the disease—the greater is the tendency to tabes and general paralysis, and the stronger and more constant is the Wassermann reaction. An unwelcome inference from all this is that mercurial treatment carried far enough to cure or prevent the symptoms of secondary syphilis may set up a strong predisposition to cell atrophy and to the so-called parasyphilitic diseases at a later period.

Still pursuing the subject of the Wassermann reaction, we find

that certain foreign investigators, contrary to the experience of Mr. McDonagh quoted above, state that all mothers of congenital syphilitic children give a positive reaction, and continue to do so for four years after giving birth to such child or children, and consequently, it is concluded, that in spite of *apparent* health, the mothers of congenital syphilitic children are much more frequently syphilitic than has been supposed. Here, again, reliance is placed on the Wassermann reaction as a proof that the person whose blood or cerebrospinal fluid yields the reaction is syphilitic. Yet some of these women who gave such a marked reaction had no sign of syphilis, "had never suffered in any way, never had a day's illness." I have referred to the uncertainty of the reaction already; a few other instances might be given. Congenital syphilitic children of mothers who have given a strong reaction have not themselves reacted; in other instances the child has given a negative reaction just after birth, but a positive reaction later when symptoms of syphilis appeared upon it. A case of ophthalmoplegia externa and interna gave a negative reaction, but cleared up under specific treatment. A patient with syphilitic arteritis gave a negative reaction until after six weeks of treatment with mercury and potassium iodide; then he gave a well-marked positive reaction. This is the "reaction provocative" of Ehrlich. Mr. McDonagh refers to "cases" which with a primary sore gave a negative Wassermann reaction, but which gave a positive reaction after an injection of salvarsan; he recommends that in the intervals between treatment when the body fluids give a negative reaction a further injection of salvarsan should be given, and the blood tested again and found again to give a negative reaction before the patient is said to be cured; and he further adds that patients who have had syphilis and been so far treated with mercury "as to have been driven into the latent stage with a negative Wassermann reaction" give a strong positive reaction after one or two provocative injections of salvarsan. So constant is this that he says salvarsan can be used as a test of a cure, and that it shows also that mercury never cured a case of syphilis.

With such uncertain, variable, and paradoxical results as I have quoted, are we really in a position at present to say what the precise meaning and value of the Wassermann reaction is? I know nothing of the process of the test beyond having had it explained to me in a clear and skilful manner verbally, and by outline figures; and from having been shown in the laboratory the difference between the negative and positive reaction. It all seemed to me very complicated

and very marvellous, and I wondered how it had ever entered into the thoughts of man to apply rabbits' serum and guinea-pigs' serum to sheeps' corpuscles, then to add an extract of the liver of a congenital syphilitic infant (or of a liver which is not from a syphilitic subject and which I am assured, nothwithstanding Dr. Candler's precautions, does equally well), and mix up with them all some of the serum of a patient whose blood is to be tested, and then to draw a momentous conclusion based on whether the red blood corpuscles of the patient sink to the bottom of a test-tube or break up and yield their colouring matter to the whole volume of the test-tube's contents. It seemed to me—whose line of life is not bacteriological research—as I have said, all very intricate and wonderful, though I am told that in reality it is not so, but on the contrary has all come about by steady, regular, and well-understood steps, thoroughly familiar to laboratory experts. But of this I feel sure, that only very competent and well-practised workers are fit to be entrusted with these investigations.

And what do we learn from them? Why this: That the experts have arrived at no unanimous opinion as to the precise value and significance of the Wassermann reaction. The majority, perhaps, of those who have recourse to it, think, notwithstanding the discrepancies I have detailed, the positive reaction is a more reliable test than the negative; others that the negative but not the positive is the more reliable guide, because as yet we do not know the limits within which "group" reaction occurs with Wassermann's test; others, again, that the negative reaction is not a proof of the cure of syphilis until it has been obtained after, as well as before, another injection of salvarsan or the further inunction of mercury. That the positive reaction is not always given even when syphilis exists in an active form is proved. Nor can we say that only syphilis will yield a positive reaction. The blood of scarlet fever patients has done so, and we are not sure there may not be other conditions also in which the positive reaction occurs.

I learn from conversing with bacteriologists that the Wassermann test is an application of the "complement-fixation" reaction, for the diagnosis of syphilis; that "complement-fixation" reactions are of two kinds—(1) "group" reactions, and (2) "specific" reactions. Moreover, I am told that the Wassermann test represents what is called a "group," and not an absolutely "specific" complement-fixation reaction; that the antibodies in the serum of patients with scarlet fever are capable of reacting with the specific antigen used by Wassermann in his test; that hitherto it has not been possible to define the

limits within which Wassermann's test represents absolutely "specific" reaction; and that, apart from knowledge of the clinical history, it is impossible to judge whether a positive "Wassermann" obtained with a particular serum represents a "specific" or merely a "group" reaction. But note! Only the "specific" reactions are absolutely decisive for prognostic purposes.[1]

What is the Immediate Cause of the Wassermann Reaction?

What is the immediate cause of the reaction? The theory held is, I believe, that the reaction is due to lipoid bodies, the result of the action of toxins, or the spirochæte itself, on the body. The spirochæte certainly need not be present, because the reaction has repeatedly been obtained long after this organism has vanished from the patient's body. Nor is it clear that the toxins generated by the spirochæte are necessary, seeing that the most intense and constant reaction is furnished by the fluids of persons who have had syphilis in the mildest form only, and as much as thirty years before. It would appear to be some state as yet undiscovered or unexplained of the solids or the fluids of the body which is required for the Wassermann positive reaction. And though this state of the solids or fluids or both may be more frequently brought about by the virus of syphilis than by anything else, it would seem that there are other causes also — factors, perhaps, altogether independent of syphilis—which play an important rôle.

[1] Mr. Foulerton writes to me as follows: "The difference between 'group' and 'specific' complement-fixation is well illustrated by the work of Much, of Hamburg, on the complement-fixing properties of the serum of persons, and experimental animals, infected with certain organisms of the "acid-fast" group of bacteria. Thus the serum of a leprous patient will give a complement-fixation reaction when either the parasite of leprosy, or the parasite of human tuberculosis, is used in place of the 'antigen.' In other words, the antibodies (or, perhaps, some of them) which occur in the serum of a leprous patient are capable, up to a certain limit, of reacting with either of the closely related parasites which, respectively, are the cause of leprosy and human tuberculosis. And, conversely, the serum of a tuberculous patient will give a complement-fixation reaction with either the parasite of tuberculosis or the parasite of leprosy. And, so far, the reaction is not a 'specific' reaction, it is a 'group' reaction. But by treating the serum of the leprous patient in a particular way with tubercle bacilli, it is possible to remove from the serum that portion of the antibodies which is capable of reacting with the tubercle bacillus. On submitting the leprous serum, thus prepared, to a fresh test, it will be found that it now gives a 'specific' reaction with the parasite of leprosy only, and none with the tubercle bacillus. The tuberculous serum can be similarly treated with the parasite of leprosy, so that the fallacious 'group' complement-fixation reaction is eliminated, and the diagnostic 'specific' reaction is obtainable."

REMARKS ON THE TREATMENT OF SYPHILIS.

But however perplexed we may feel in regard to the Wassermann reaction, no doubt seems possible about the remarkable effect of salvarsan upon the spirochætes and their toxins, nor as to its power of quickly and completely causing the disappearance of the symptoms of syphilis in each of its stages. A careful perusal of contributions to this debate reveals a unanimous agreement as to the efficacy of salvarsan, but differences of opinion (*a*) as to the best system of administration of the drug; (*b*) as to whether or not it permanently cures syphilis; (*c*) as to whether it should be given with mercury or alone; (*d*) if with mercury, as to when the mercury should be commenced; and (*e*) in what form mercury is best employed. Salvarsan has certain great disadvantages. Every dose has to be prepared *ad hoc*, and requires great care in its preparation; it needs care and precision and some manipulative skill to inject it properly; it is not free of even serious risks, and it is not to be wondered at that several fatal results have attended its use, seeing that whilst it is powerful enough to create immunity reaction products which quickly attack and kill the spirochæte, it must, to be safe, do this without harming the body itself. Thus it is not a remedy to be employed by every busy practitioner, nor to be used upon patients who cannot or who are unwilling to submit themselves to the due and proper precautions which should be taken to make the treatment safe.

The treatment with mercury without salvarsan will not therefore, in all probability, be entirely supplanted. Nor from the point of view of curing the primary and secondary symptoms of syphilis and of preventing the recurrence of these symptoms does it seem to me to be at all necessary it should be supplanted. That the full, well-regulated use of mercury does accomplish such complete cure is, I consider, amply proved. I have lived long enough to watch the lives and health of many persons whom in my, and their, early days I have had to treat as patients, and to see them now the healthy parents of healthy children. I have had experience of two male patients who, having been treated with mercury and been cured, have contracted a second hard chancre followed by secondary symptoms, and who were a second time cured with mercury. I can recall cases of men who have had syphilis who have married before they have been fully or sufficiently treated with mercury, and their wives after one or two miscarriages have been treated during each subsequent period with mercury and have borne in one case three and

in another case five healthy children who have grown to womanhood or man's estate. In neither of these cases did the mother show at any time any signs of syphilis nor did her health suffer in any way.

I find it difficult to avoid the following conclusions, viz., that in these cases the seminal fluid of the father conveyed the disease to the fœtus of the first pregnancy, but that, in subsequent pregnancies, mercury administered to the mother had the effect of preventing harm occurring to the subsequent children, and that the mothers themselves, as they remained free of any evidence of syphilis throughout their lives, ought not to be described as the subjects of syphilis. And I hold this opinion notwithstanding my knowledge of another class of case, namely, where the woman who, never having exhibited any symptoms of syphilis, and having had a child or children by a syphilitic husband, has borne a child with marked signs of congenital syphilis to her second husband who was himself quite free of any syphilitic taint.

As time goes on one fact may possibly be brought to light which will make it advisable that every case of syphilis should be treated with salvarsan. If the theory that tabes and general paralysis are caused by degeneration of nerve cells due to latent syphilis be true; if this latent syphilis is brought about owing to the spirochætes and their toxins having been incompletely or only very tardily destroyed by mercurial treatment; and thirdly, if salvarsan administered at any period between the primary chancre and the roseolar eruption which ushers in the second stage of syphilis will prevent the establishment of syphilis in the system in a latent form, then it will become the duty of the profession to see to it that salvarsan is used in every case in the early stage of syphilitic infection. Then, if these hypotheses are proved to be true and salvarsan is given in good time, there may be expected to follow in course of years complete elimination of tabes and general paralysis from the category of diseases.

Need of Instructing Medical Students and the General Public.

Attention has been directed by some of those who have taken part in this discussion (1) to the need of giving more attention to the teaching, and of making the teaching in venereal diseases more systematic, in the medical schools of this country; and (2) to the need of a system of registration of syphilis in the United Kingdom.

No doubt the recent discoveries and the recent advances in our knowledge of the diagnosis and treatment of syphilis must lead to

further and special courses of instruction with regard to them. But I must dissent from some of the statements which have been made during this discussion to the effect that no regular teaching whatever has been given hitherto to medical students in England in venereal diseases. Nor can I accept the suggestion that because a medical student contracts a chancre on his finger whilst attending a midwifery case that it is because his medical education with regard to syphilis has been neglected. I know of a very distinguished obstetric physician and gynæcological teacher who met with the same misadventure. Both alike, I have no doubt, were guilty of some slight carelessness or apparently trivial inadvertence. I do not doubt that the student had received much instruction from several of his teachers about the various manifestations of syphilis; nor that the obstetrician I refer to was in the habit of cautioning his students and nurses against the dangers to themselves which syphilis in the mother presents, and the dangers to the infants which maternal gonorrhœa creates. Still, I feel sure that the question of improved or more specialized teaching in syphilis, as well as questions relating to the statistics, notification, registration, and segregation of persons affected with syphilis will, in the interests of public health, soon come under the careful consideration of a Committee of the Royal Society of Medicine, which was recently appointed—after representatives of the Society had held a Conference with certain representatives of the Eugenics Education Society.

Some months ago an inquiry was set on foot by our Society with the aid, and through the medium, of the Secretary of State for Foreign Affairs, as to what is being done in other countries in regard to the registration and segregation of syphilitics, and already many reports have been received from abroad. These reports, together with the information and recommendations contained in the practical and suggestive papers which this important debate has brought to notice, will, I believe, be of considerable assistance to our Committee, and ultimately of much importance and benefit to the public.

PROCEEDINGS

OF THE

ROYAL SOCIETY OF MEDICINE

VOLUME THE FIFTH

COMPRISING THE REPORT OF THE PROCEEDINGS FOR THE SESSION 1911-12

SECTION OF ANÆSTHETICS



LONDON
LONGMANS, GREEN & CO., PATERNOSTER ROW
1912

Section of Anæsthetics.

OFFICERS FOR THE SESSION 1911-12.

President—

W. J. McCardie, M.B.

Vice-President—

Richard Gill, F.R.C.S.

Hon. Secretaries—

Mrs. Dickinson Berry, M.D.
H. J. Scharlieb, C.M.G., M.D.

Other Members of Council—

H. E. G. Boyle.
Cecil Hughes, M.B.
Rowland W. Collum.

Representative on the Library Committee—

Dudley W. Buxton, M.D.

Representative on the Editorial Committee—

Mrs. Dickinson Berry, M.D.

SECTION OF ANÆSTHETICS.

CONTENTS.

November 3, 1911.

PAGE

Three Cases of Death under Anæsthesia. By Miss A. M. Browne ; Miss J. H. Turnbull, M.D.; W. A. Mallam 1

Notes of a Case in which the Administration of Ether by the Open Method was followed by Acute Bronchitis and (?) Pleuritic Effusion. By J. F. W. Silk, M.D. 10

Notes on a Troublesome Dental Case. By Rowland W. Collum 14

The Systematic Use of a Mouth-prop and Tongue-clip in General Anæsthesia. By H. Bellamy Gardner 16

December 1, 1911.

Crawford Williamson Long (1815-1879): the Pioneer of Anæsthesia and the first to suggest and employ Ether Inhalation during Surgical Operations. By Dudley W. Buxton, M.D. 19

Proposal to make Methylated Chloroform Official 46

February 2, 1912.

An Experimental Study of the After-effects of Chloroform. By A. L. M. Muskens, M.D. 49

The Significance of Acetonuria in Childhood. By R. S. Frew, M.D. ... 60

Microscopical Specimens of Kidney and Liver from Cases of Post-chloroform Poisoning. By R. E. Apperly 67

Chloroform Toxæmia in a Child to whom Fat was Noxious. By W. J. McCardie, M.B. 68

Notes of a Case of (?) Post-anæsthetic Poisoning. By Mrs. Dickinson Berry, M.D. 69

March 1, 1912.

PAGE

Ether Infusion Anæsthesia. By FELIX ROOD, M.B. 77

Hedonal Infusion Anæsthesia; a Report on Seventy-five Cases. By C. M. PAGE, M.S. 84

The Council think it right to state that the Society does not hold itself in any way responsible for the statements made or the views put forward in the various papers.

JOHN BALE, SONS & DANIELSSON, LTD., 83-91, Great Titchfield Street, London, W.

Section of Anæsthetics.

November 3, 1911.

Dr. W. J. McCardie, President of the Section, in the Chair.

Three Cases of Death under Anæsthesia.

Case I.—By Miss A. M. Browne.

The death occurred during induction for an abdominal operation at the New Hospital for Women, October 12, 1911.

Previous history (extracts from the House Surgeon's Notes, taken on admission): Mrs. E. P., aged 50, was admitted into hospital a few days before the operation for the removal of a pelvic tumour. She stated that her general health was good, but menstrual losses had been irregular and excessive for three years. No previous serious illness.

Condition on admission: Patient looks healthy, but fat and flabby; good colour in cheeks: very nervous about the operation. The patient was too fat to permit of marking out the area of cardiac dullness; the apex-beat was *heard* best about the fifth space internal to the nipple-line. The sounds were regular and clear, but soft; no murmurs present. The respiratory sounds were normal. The abdomen was full-looking, and there was much fat. It moved well on respiration. The uterine tumour was visible and palpable nearly up to the umbilicus. The urine was acid, the specific gravity 1015, the urea 2·2 per cent. There was no albumin nor sugar present. There appears to have been some shortness of breath and palpitation on exertion.

My own notes are as follows: The patient was prepared in the usual manner, a dose of castor-oil being administered over night, and an enema at 5 a.m. on the day of operation, followed by tea and bread and butter at 6 a.m., and beef-tea at 10 a.m. She was clothed in loose-fitting flannel garments, and the many-tailed abdominal bandage was not tight.

I saw her for the first time when on the trolly in the anæsthetizing

room, and noted in addition to the report given me by the house surgeon that she had slightly dilated capillaries of the face, but that the lips were a good colour. Her pulse was regular and good, the rate about 80. She was extremely nervous—markedly so. The anæsthetic was commenced just after 2 p.m. by a post-graduate lady student under my direct and continuous supervision. A Schimmelbusch mask was used, and the chloroform was administered slowly, drop by drop. In addition, about 1 dr. of ether was used during the first few minutes. At this stage the respiration was hesitating and restrained owing to nervousness; this, however, quickly improved, and induction proceeded slowly, but well, for about five minutes. The patient then became excited, and struggled, but not violently. She was easily restrained by the nurse, and no pressure was made on the chest or abdomen. During this period the mask was removed, but she settled down again almost at once, speaking (apparently) rationally, saying that she still saw us. The eyes were then open, the pupils about 2 mm. in size, and corneal reflex present. The impression left on our minds, in discussing the matter immediately afterwards, was that she was conscious at this time. The administration of the chloroform was recommenced in the same manner as before. For another two or three minutes respiration continued regular and quite normal, then quietly ceased at the end of expiration. There had been no specially deep respirations. I was observing the pulse at the wrist at the moment when I saw the respiration cease. The pulse was then good and regular, the rate still being about 80. The face was slightly flushed, and the pupils not dilated.

The administration of the anæsthetic was at once stopped, and artificial respiration commenced. The good colour remained for about a minute, but she gradually became cyanosed. There was no secretion in the respiratory passages, no spasm or rigidity; nor was there at any time any pallor or any dilatation of the pupils.

The usual restorative methods were tried, including the administration of strychnine, caffeine, camphor, adrenalin, oxygen (per laryngeal tube), rectal saline and brandy, intravenous transfusion, and the application of the electric current over the heart. Inversion was not attempted on account of her being on a trolly, but her head was lowered. Her legs were raised for a short time, but the position appeared to interfere with the proper carrying out of artificial respiration, and her great weight and bulk made it unsatisfactory. An effort was made to compress the heart through the abdominal wall and diaphragm; no direct compression was attempted.

Artificial aspiration, &c., were continued for three hours, but at no time was there the slightest response or attempt at spontaneous respiration.

The following is a brief résumé of the post-mortem notes: Brain, on careful examination, was anæmic, otherwise appeared normal. Larynx normal. Thorax: No free pleural fluid; height of diaphragm, fifth rib. Lungs normal. Heart: No excess of pericardial fluid; excess of epicardial fat; weight 10 oz.; muscle wall rather flabby and infiltrated with fat; no blood-clots in heart. Valves: Pulmonary, aortic, and tricuspid, normal; mitral, slight thickening of the auriculo-ventricular band; no vegetations; coronary arteries not thickened. Abdomen: Anterior wall prominent and thick, containing much adipose tissue; there was a little free peritoneal fluid. The omentum was much loaded with fat, and greatly larger than normal, indeed, the largest the pathologist had ever seen. Liver: There were a few adhesions to the transverse colon and to the diaphragm; the surface was smooth and rather pale, with some fatty infiltration; the weight was 5 lb. 6 oz. The stomach was rather large, normal in appearance, and empty. All the other organs, except the pelvic, were healthy. There was a uterine fibroid, an ovarian cyst, and an enlargement of the right Fallopian tube. Sections of the liver are reported as showing extensive fatty changes. In the portions of the lungs examined there is marked fibrosis, with some emphysema and considerable dilatation of the vessels. No pathological changes detected in the heart-muscle. A portion of glandular tissue removed from the region of the thymus has the structure of a lymphatic gland. There is no evidence of a persistent thymus gland.

Remarks.

The chloroform used amounted to about 30 minims. It was supplied by Messrs. Duncan and Flockhart, made from ethylic alcohol, was from a bottle of which about one-half had been already used for other patients, and which, on being subsequently independently analysed, was found to be quite pure. A lighted gas-ring was in use about 5 ft. from the trolly; there was no artificial light. The room was well ventilated, and the temperature was about 60° F., probably less.

The post-mortem appearances do not give any conclusive evidence as to the cause of death, but I would note that the brain was very carefully examined, and no disease found. On the other hand, it was very anæmic. In the second place there was some considerable

infiltration of the heart with fat. In addition, the abdomen was somewhat distended by the very enlarged liver and omentum, and the fibroid, and this might well have interfered with the action of the heart.

I should add that the artificial respiration, carried on by Silvester's method, did not appear to be at all effectual; the air entered the larynx freely, but the movement of the chest wall was poor, though there was no rigidity. I may say that this was so marked that the respiration was carried on by two of us, but without improvement. The fibrotic condition of the lung may explain this.

I would suggest that in this case the cause of death was primarily respiratory. The clinical observations point this way. It may, perhaps, be remarked that this is the first fatal case during induction, or which could be attributed to the anæsthetic pure and simple, which has ever occurred in the hospital.

Case II.—By Miss J. H. Turnbull, M.D.

Woman, aged 49. Pale and thin. Low urea excretion. Signs at left base suggesting old tubercle. Frequent winter cough. Operation to be done for fibroids of uterus causing marked anæmia. Kept in hospital some days before operation to improve condition.

Anæsthetic: Chloroform, 2 dr.; a little ether added towards the end of induction. Induction gradual, by drops on open mask, held all the time about an inch from the face. Some slight irritative coughing at first in spite of very gradual administration. This ceased, and there were slight muscular movements of the limbs and a little muttering; no struggling. Respiration quiet and regular. Lips and ears very good colour. Pupils medium, corneal reflex brisk. Then sudden alteration in breathing (five minutes after beginning of induction). There were seven or eight deep, sighing respirations, and at the same moment wide dilatation of the pupil, pulse disappeared, no pallor for ten or fifteen seconds. Then cessation of respiration (no respiratory embarrassment at all before the sudden collapse). Tongue pulled forward, artificial respiration, oxygen, strychnine, &c. No further sign of life.

Post-mortem: Old pyonephrosis of left kidney, high up under ribs. Collapse of base of left lung. Heart-muscle a little flabby and thin, but nothing definite. No excess of lymphoid tissue.

The symptoms came on a very few seconds after a fairly brisk corneal reflex had been obtained—only a few drops had been added—the mask was always a little off the face.

Case III.—By W. A. Mallam.

Patient was a well-nourished, well-developed girl, aged 15½. Six months ago she had had a severe attack of whooping-cough. On October 1 last I saw her for the first time. She was suffering from an influenzal "cold." Her throat was reddened, her nose running. She had some bronchial catarrh and a small temperature. She recovered without trouble from this attack and I saw her at my house again on October 10. She had both tonsils chronically enlarged, adenoids, and a history of frequent cold-catching, attended, on at least two occasions, by some deafness and earache. I advised an operation for the removal of the adenoids and tonsils. She was given oil on the evening of October 16, and on October 17 had nothing after a light breakfast at 8 a.m. till the operation at 2.30 p.m. next day.

When we arrived the child was quite calm and had not been excited. She walked into the operating room from another room on the same floor. I gave her A.C.E. mixture in a celluloid mesh. She was rather a longer time in going under than usual for a girl of her age; but I was giving it very gradually, not wishing to frighten her. There was no coughing, and the pulse and respiration were perfectly normal; there was no excitement. Her face "worked" a little and she swallowed two or three times. At this time her eyelids resisted when I tried to look at the pupils. Almost immediately afterwards the arms, which had been stiff, relaxed. She had no more anæsthetic from that moment. Her head was then pulled over the edge of the table, the pillow remaining under the shoulders. The gag was introduced and the tonsils and adenoids removed quite easily and quickly. Her colour was good and she was breathing quite well. There was a good gush of blood from the mouth and nose, and we swabbed out the throat, removed the gag, and had got her head back on the table before we noticed any trouble. She had previously coughed strongly. At this moment we noticed that the breathing had stopped, she began to get a bad colour, and we could not feel the pulse at the wrist. I at once began artificial respiration. Once or twice she began to take voluntary breaths, but this soon ceased. Her pupils were dilated, no heart sounds could be heard, and notwithstanding artificial respiration for two and a half hours, injections of strychnine, of pituitary body, atropine, and at last cardiac massage through the abdomen, she never recovered consciousness. I had put about 4 dr. of the A.C.E. mixture on the sponge to start with and had not repeated it. She was barely in the second stage of anæsthesia when the operation was begun. Some two or three

minutes elapsed after the mesh was removed and whilst the operation was done before she showed any signs of alarm. She coughed strongly at least once. I had examined her heart at the time of the influenzal attack and again just before I gave the anæsthetic. There were no bruits and the first sound at the apex was quite good, and heard best in the usual position.

Dr. Spilsbury made the post-mortem examination, at which I was present. There were numerous large lymphoid masses at the base of the tongue reaching on to the epiglottis; enlarged glands in the neck; thyroid enlarged. Thymus considerably enlarged and reaching down into the deeper structures of the neck on the left side. Lungs: Considerable amount of emphysema (probably due to the recent attack of whooping-cough). Heart: Dilated, the tricuspid admitting four fingers and the mitral three; no valvular disease. The muscle was pale and (microscopically) showed signs of fatty degeneration. Spleen: Enlarged, and soft lymphoid tissue increased; enlarged calcareous glands in the lower part of the mesentery. Stomach empty. Bowels: Lymphoid masses standing up like sago grains from the rest of the surface, especially at the lower end of the ileum near the valve. Aorta: Both thoracic and abdominal, smaller lumen than usual. All the other organs were normal.

Family history: The mother had a twin sister who died from heart disease. One of the other children has heart trouble (rheumatic), whilst the remaining two have had this same operation performed quite successfully, and are (I believe) quite healthy.

DISCUSSION.

Mrs. BERRY said that she thought Miss Ada Browne had been very fortunate, as she had given anæsthetics, she believed, in about 18,000 cases, and this was her first fatality.

Mr. H. P. SYMONDS said the only question which occurred to him in regard to the communications was, why chloroform was given in all the cases.[1] In one hospital to which he belonged there occurred not very long ago three deaths in a fortnight. In consequence of this the surgical staff decided that no chloroform should be given without the consent of the surgeon. Since this there had been no deaths from anæsthetics. He believed so many deaths followed the giving of chloroform because it was found to be the easiest kind of anæsthetic to give. A.C.E. he considered the most dangerous of all. He

[1] In order to shorten induction, chloroform was given in the first case as a preliminary to ether, which was to be administered by the open method. In the second case chloroform was selected on account of pre-existing lung trouble.

had taken note of these deaths from chloroform for many years, and had a vivid recollection of the first one, because up to that time chloroform was thought to be absolutely safe, so that this fatality caused great alarm. His view was that chloroform should not be given without other forms of anæsthetic having been considered. In nearly all deaths from anæsthetics, chloroform seemed to have been the form chosen.

Dr. LLEWELYN POWELL desired to refer to two points in Miss Browne's case. In the first place, she said she used about 30 minims of chloroform, but at the commencement she stated that she was giving it by the drop method, one drop per second. The administration seemed to have gone on from seven to ten minutes. Therefore he thought there must be some mistake about the 30 minims. The second point was as to the employment of the Silvester method of artificial respiration not being satisfactory. It might be that that was so because the patient was lying on her back. It was often easier to get breathing by the Schäfer method ; either the complete Schäfer, or turning the patient on the side and working with one arm in compressing the chest. The patient's jaw was apt to fall forward, and breathing was better if turned on the side than if lying on the back.

Mrs. BERRY said that Miss Browne had stated that the quantity of chloroform used was correctly given, but the rate at which it was dropped on the face-piece and the time consumed were from recollection only.

Mr. MALLAM said he would be glad to know if anyone had found a means of diagnosing cases of the status lymphaticus.

Mr. CARTER BRAINE said he would like to comment on the method of artificial respiration carried out in Case I. He had once or twice been in very serious difficulties when giving chloroform, owing to the cessation of respiration, and had in the first instance tried the Silvester method of resuscitation. He was convinced, however, that this was not always the best method. In more than one case he had failed to re-establish respiration by its use, more especially if the respiration had ceased during expiration, but on attempting the Howard method he had been successful. There was, in his opinion, very little chance of re-establishing respiration by the Silvester method in such desperate cases, but by using the Howard method and working more on the lower ribs and abdomen, one was able to drive up the diaphragm a little, and a respiration would probably ensue. Dr. Powell suggested turning the patient on the side, which he took to mean that the Silvester method should be discarded. He (Mr. Braine) had also found this plan successful, the reason being that the weight of the intestines and omentum, instead of being on the diaphragm, was supported on the operating table, and in that way tension was relieved.

Mr. BELLAMY GARDNER, referring to Mr. Mallam's case, reminded members that two meetings were devoted to the subject of status lymphaticus two years ago. The result was to make clear that so far as the

diagnosis and prognosis of the condition of lymphatism was concerned, knowledge had only gone so far as finding out that if there had been a history in a young child of eczema, lymphatism might reasonably be suspected. Also, if there had been very severe symptoms during very slight illness, the condition might be thought likely, as also if a series of small soft glands could be felt either in the axillæ or groins. There seemed to be no other signs to justify the diagnosis, though suggestions had emanated from America that a radiogram should be made of the thorax in the hope of showing whether the thymus was enlarged.

Dr. SCHARLIEB said he would like to discuss Mr. Carter Braine's suggestion that the Howard method of artificial respiration should be employed. He had carried out a series of experiments on killing dogs by means of chloroform, with Professor Schäfer, in Edinburgh. In every case in which the Howard method was used the liver was ruptured. In one case of death under anæsthesia in a man he was present at the autopsy, and the liver was found to have been ruptured. In the case of both human beings and dogs post-mortem examinations showed that when they died under chloroform, the liver, spleen and lungs were dripping with blood, so it could be easily understood that any moderately rough method of artificial respiration, such as the Howard method, would easily rupture the liver. By the Schäfer method one could not rupture the liver of dog or man.

Dr. DUDLEY BUXTON, alluding to the subject of attempts at resuscitation by artificial respiration, said it was an important point to notice, following on what Dr. Scharlieb had said, that even if the abuse of the Howard method of restoration might rupture the liver, it did not follow that the use of it would lead to the same result. The abuse of any compression method might lead, and had led, to fracture of the ribs. He remembered that when he was a student there was a one-armed man in hospital who suffered from chloroform overdosing. The Silvester method of resuscitation was employed, modified, however, by the fact that the man had only one arm. Death ensued, and fracture of several of the ribs was found at the necropsy, the damage having been done in the attempts to produce artificial respiration. He did not doubt that the ideal method of carrying out artificial respiration was the Schäfer method. Of course, there were difficulties in the way of its adoption when an operation had been commenced, as in cases of abdominal sections, since turning the patient into the prone position was impossible. It was interesting to recall what had been said by, he believed, Dr. Wood, of Philadelphia, who, referring to the best method of dealing with these cases, stated that since he had adopted forced respiration—i.e., the use of an intubation tube—so that air or oxygen under pressure was forced into the lungs and then allowed to be expired, he had not met with a death. Dr. Wood was convinced that when this was done, there was the greatest amount of perflation of the lungs, and the best pulmonary ventilation ensued. However, even when that method was used incautiously there was danger of rupturing the air-cells, therefore it was necessary to regulate the pressure of the

oxygen or air used. With that possible exception, perflation seemed to present the best chance of getting oxygen or air into the blood. Whether in the cases which had been described the amount of anæsthetic which entered the body had not already destroyed the neural tissue it was impossible to say. Of course, no amount of lung ventilation would recover nerve-tissue which had been greatly over-dosed with chloroform, and he was rather tempted to suggest that in some of the cases mentioned, at all events if the chloroform had been given by a method which allowed the administrator to know the exact percentage of chloroform given, death need not have occurred. He took it that the danger of chloroform was not so much from the chloroform itself but from the method of giving it, and when, for example, the supply was irregular, sometimes in high percentages, sometimes in low, it was this oscillation of the strength of vapour given which was the great danger—perhaps the chief one, in the administration of chloroform.

The PRESIDENT (Dr. W. J. McCardie) said he noticed that in the account of Miss Browne's case the patient died when the pupils were small. This he regarded as a curious fact, and he did not remember that anything was said about the state of the pupils afterwards. But in Dr. Turnbull's case, when the patient died the pupils suddenly dilated. In both those cases the patients were not completely anæsthetized, at any rate they had had so little chloroform that one inferred that the deaths were not due to a toxic but rather to some reflex cause. In the case of the girl aged 15 death seemed to have taken place during fairly deep anæsthesia in a patient of the lymphatic type. It was sometimes difficult in adenoid cases to find reason for suspecting the presence of a lymphatic diathesis, but there were many general indications which, when taken together, though slight in themselves, would give an idea that that diathesis was present. He referred to the fat, flabby child, of good colour, thick neck, with perhaps some enlargement of the thyroid, tongue, and mesenteric glands, which glands were sometimes palpable and more frequently enlarged than those in the axilla or groin. There was also the presence of large pupils indicating low blood-pressure, and soft heart sounds; and one could sometimes make out by percussion some enlargement of the thymus. All those points, carefully considered, would give some idea of the existence of the lymphatic diathesis. Several valuable hints had been brought out as to the best method of inducing artificial respiration in fat and flabby patients. If one could always quickly carry out perflation of the lungs with oxygen in all critical cases, doubtless one would be doing the best possible for the patient. But he took it that one could not very speedily perform intubation in a patient apparently dead in all the hurry and excitement of the catastrophe. Such cases as those now related brought out the seriousness of the work of anæsthetists. He himself had had fatalities, and each fatality increasingly emphasized the importance of at any rate inducing anæsthesia with ether. The giving of ether was possible in most cases, at least for the first few breaths. In some of his own cases he believed that if he had done that life would have been saved.

Notes of a Case in which the Administration of Ether by the Open Method was followed by Acute Bronchitis and (?) Pleuritic Effusion.

By J. F. W. Silk, M.D.

The patient was a man, aged 34, a Turkish bath attendant. Height, 5 ft. 8 in.; weight, 13 st. 9 oz.; flabby and plethoric; ? alcoholic. He was inclined to be somewhat emphysematous, and his chest expansion was not in proportion to his weight and build. His heart sounds, though somewhat distant, were regular and fairly good. There were no moist sounds in his lungs. No albumin in his urine. The operation proposed was for the radical cure of double inguinal hernia; that on the right side had been operated upon some few years previously, but had recurred.

The operation took place on October 11, 1911, at about 2.30 p.m. at King's College Hospital. Temperature of theatre about 65° F.; outside, 57° F., with an east wind. I elected to administer ether by the open method, but I must admit at the outset, that in making this selection I felt that I was doing some violence to my own judgment and teaching. In the ordinary course of events I should have preferred "Mixture." The nose and chin having been packed round with cotton-wool, the ether was administered from two or three layers of domett stretched over a Schimmelbusch's frame. No preliminary injections of atropine or other drug were given. The period of induction lasted for nearly fifteen minutes and was associated with a good deal of struggling. The anæsthesia was maintained with some difficulty, there being a constant tendency to muscular spasm, and slight movements, which were only partly controlled by a liberal use of the anæsthetic, nearly 15 oz. being used in the course of the hour and three-quarters that the operation lasted. The operation was unduly prolonged, partly on account of the imperfect anæsthesia, but mainly because of the adhesions which had formed as the result of the former surgical procedures. During the operation the breathing became rapid, the face pale, and there was a good deal of sweating, but there was no lividity. There was a little exudation of frothy, churned-up mucus from the mouth, and there were some obvious moist sounds in the trachea and bronchi, but they did not appear to be excessive. The pulse became quick and soft.

After the operation I gave directions for the patient to be turned on his right side, but this could not be done as it gave rise to much distress of breathing and lividity; he was accordingly sat up in bed.

When next I saw the patient, on October 13, he looked very ill. The report I received about him was that there had been little or no sickness; his temperature had gradually risen to close upon 102° F.; his pulse to 132; his breathing to 32; his abdominal wounds gave him great pain when he coughed, but nevertheless he was bringing up large quantities of clear, tenacious mucus from the lungs. He had been seen by a physician, who reported that he had a severe attack of bronchitis, with feeble entrance of air into both bases, and signs suggestive of the onset of a pneumonia. His heart sounds were feeble, rapid and irregular, but no displacement of his apex-beat was noted.

By October 18—i.e., a week after the operation—the general condition had much improved. The temperature had fallen to normal. There was still some rapidity of pulse and breathing, and a good deal of mucus; the physical signs of his chest suggested that there was a little fluid in both pleuræ, but this was not verified by exploratory puncture as none was made.

The patient gradually improved, and the chest cleared up. The temperature fell to subnormal. He got up for the first time on November 1.

Remarks.

Although I do not propose to attempt to justify the choice of the anæsthetic, yet I think that this case is worthy of record. Although, too, it is but an isolated instance of mishap, I think that it supports the view that, to some extent at any rate, the open method of administration may give rise to troubles which are practically identical with those which arise in the closed methods.

The details of the administration will, I hope, be criticized; I will only say that they are those adopted with success in many other of my cases. I have described the case as one of "acute bronchitis" as that was the diagnosis made by the physician. I am not quite sure in my mind, however, that it ought not to have been described as one of pulmonary collapse, such as was described by Dr. Pasteur in the Annual Oration of the Medical Society, on May 15 of this year,[1] but the

[1] *Lancet*, 1911, i, pp. 1329-34.

prominent symptom of displacement of the apex-beat to the affected side was not noted; but then, in this case both bases appear to have been affected.

DISCUSSION.

Dr. BARTON did not believe that in this case the bronchitis was due to the choice of the anæsthetic. His view of the case was that as the man was a Turkish bath attendant, probably his heat-regulating centre had been rather spoon-fed, so that he could not stand the loss of temperature following the administration. The temperature along the corridor of the hospital might have given him a chill. He would attribute the bronchitis to the man's peculiar occupation.

Dr. BUXTON asked whether Dr. Silk had adopted the douche method or the drop method.

Dr. R. H. HODGSON, desired to know Dr. Silk's definition of the "open method," and whether he admitted that with the open method there was re-breathing of the patient's exhalations. If so, Dr. Hodgson could quite understand bronchitis and pleurisy following. But if by the open method he meant there was no re-breathing of the ether or of the patient's own breath, he thought it was impossible to say that the bronchitis and the pleurisy were due to the ether, and Dr. Silk had unjustly saddled himself with the responsibility for their occurrence. Ether given by itself, in his experience, not only did not cause bronchitis and pleurisy, but had cured both.

Mr. H. P. SYMONDS said he felt he ought to mention that he had seen a death from ether, and this was his own fault. The man came with a fractured jaw, which three surgeons tried to wire. His trachea was choked with mucus, and as it was impossible for him to cough with a fractured jaw, he died.

Mr. RICHARD GILL said he found Dr. Silk's case extremely interesting, because the patient was an attendant in a Turkish bath, and necessarily lived in a very dry atmosphere. In India the atmosphere was also dry, and six or seven years ago he was asked to give an anæsthetic to a surgeon-major in the Army Medical Service of India. This officer himself insisted that the anæsthetic must be ether. Mr. Gill asked him it he should give a little gas before the ether. The reply was that he did not care what was given before, but he would not have chloroform, and preferred ether. He (Mr. Gill) therefore gave him gas and ether, as he felt bound to keep his word. The operation was an awkward one, a right inguinal hernia, with several pouches, and adherent intestines, so that it occupied about two and a quarter hours. During the whole of this time the patient's state was that of normal anæsthesia, and he believed he was conscious before he got back to his room upstairs. Before Mr. Gill left they conversed together. But in the course of the next few days the patient developed what he himself described as "a beastly state"; he could not sleep, and coughed a good deal; presumably he had some degree of

bronchitis. He was strongly of opinion that those people who had what were called dry mucous membranes in the airway should not have any anæsthetic which had an irritating quality, such as ether vapour. In consequence of the coughing, part of the operation was spoilt, though much of it was successful. Some bulging appeared at the end of three weeks, which needed attention, so that he had to interview Surgeon-Major B. again, with regard to a second anæsthetic. The reply was, "This time I will leave it to you." After much consideration he gave chloroform. It was not often that one had the advantage of giving the two chief agents in anæsthesia to the same individual in such a short period, and at the same time obtain such equality of the conditions. On his recovery the patient informed him that after all he preferred chloroform.

Mr. BOYLE said he thought all present should be very grateful to Dr. Silk for reporting this case. He felt certain there were cases of bronchitis which occurred after the open administration of ether of which no one heard anything. If members of the Section would collect these cases and from time to time report them, he thought that men who were so keen upon giving ether would have their eyes opened to the fact that not only did bronchitis and pneumonia follow, but frequently death also. He thought those who were keen advocates of open ether were prone to support their idea by pointing out its safety at the time of operation. He believed that open ether was safer during the operation in the hands of unskilled men, but it should not be forgotten that there was danger of bronchitis and pneumonia following.

The PRESIDENT agreed with Mr. Boyle that but few cases of lung affections following ether administration were reported. He had never seen a report of a case of bronchitis or pneumonia after "open" ether, even in America or Germany, in which countries the open method had been in use for some years, though he was quite sure such after-effects did occur. He asked Dr. Silk whether there was a secretion of mucus in any quantity during anæsthesia in his case. This seemed to be the determining factor in the production of the bronchitis—namely, an infective condition caused by the inspiration of infected mucus, and probably from a not too clean mouth. In cases of the type referred to he always gave atropine beforehand, as he believed it dried up the secretions and helped respiration. It had been of great help to him in such cases. Or he might give scopolamine and morphia, viz., $\frac{1}{200}$ gr. of scopolamine and $\frac{1}{8}$ gr. of morphia—which would stop secretion. He would also like to know whether the patient was taken along any cold corridors. The temperature of the operating room seemed to have been rather low—namely, 65° F.

Dr. BLUMFELD remarked that in two of the cases narrated the patients were operated upon for hernia. He asked if the experience of his colleagues was the same as his own—viz., that chest troubles after hernia operations were more common than after other operations, often not due directly to the anæsthetic, but rather to small infarcts in the lung, probably clots coming

from the numerous veins which were tied in the operation. There was acute pain in the chest with rise of temperature, and the symptoms in such cases usually lasted about three days.

Dr. SILK, in reply, expressed his indebtedness to the Section for the way his communication had been received. He had no idea of casting any stone at the open method of administering ether, and he did not say that bronchitis was more frequent after this method than after the ordinary methods of administration. The point he wanted to bring forward was, that in selecting ether as an anæsthetic one needed to observe the same precautions in the choice of the open method as one did in the selection of the form of anæsthesia; in other words, that the open method was not as absolutely free from danger as had been suggested by some of its advocates. With regard to the other observations, it had been said that as the patient was a bath attendant he was more liable to trouble following changes of temperature. He thought that as he was a bath attendant on an ocean-going steamer, he was probably quite accustomed to extreme and sudden alternations of temperature. He felt in a difficulty in answering the question as to what he meant by "open ether." He used the term in what he believed to be the usually accepted sense—i.e., the administration over three or four layers stretched across a Schimmelbusch's inhaler. He expressed entire agreement with what Mr. Boyle said. In answer to the President he would say that at no time did there seem to be the amount of mucus which was seen when the closed inhaler was used. If there had been he would have immediately changed the anæsthetic. The most prominent symptoms with which he was not satisfied were the increase in the rapidity of the breathing, the pallor, and the sweating. No doubt the change of eight or nine degrees in temperature from the operating room to the corridors was conducive to the production of bronchial affections in those who were predisposed to such, but in large hospitals it was very difficult to obviate this. But other patients who had taken ether by the open or other methods were subject to the same disadvantages, whatever method was used. He agreed with Dr. Blumfeld as to cases of hernia being very liable to chest troubles, and would go further and say that chest troubles were distinctly more liable to follow any abdominal operation than operations on other regions.

Notes on a Troublesome Dental Case.

By ROWLAND W. COLLUM.

ON April 28, 1906, I went to a dentist's room to give gas to a patient for the extraction of several teeth. She was a girl, aged about 20, and appeared to be a good subject. I gave her gas and oxygen in the ordinary way with a Hewitt's apparatus, but before she was fully under

she vomited, so that no operation was possible. We then gave her a short rest, and let her wash out her mouth; after which I decided to try the effects of plain gas without any oxygen. Again, however, she vomited before she was ready for operation. This was at 11.45 a.m., so we decided to leave her for that day, and try again two days later in the early morning before she had had any food.

Consequently, on April 30 she came once more, this time at 9.30 a.m., having had nothing to eat since the previous night. On this occasion I chose plain gas, but the inhalation had to be stopped on account of retching before a satisfactory anæsthesia had been obtained. It was possible, however, to do a little of the operation. I then administered ethyl chloride in an apparatus consisting of a Clover's ether bag fitted to a face-piece. This was taken only fairly well, and the resulting anæsthesia was short and accompanied by a good deal of movement, but some more teeth were able to be removed. A second application of ethyl chloride caused vomiting before anæsthesia supervened; so we had to leave her once more.

The next day (May 1) she came again at 9.30, before breakfast, and on this occasion I *commenced* with ethyl chloride, and we obtained just sufficient time to complete the extractions before she again vomited.

To sum up: On the first occasion I gave her gas and oxygen, with the result that she vomited before she was under. I then gave her plain gas, but the result was the same; so that no extractions could be done that day. Two days later she had plain gas before breakfast, and a little of the operation was performed. Then a dose of ethyl chloride sufficed for a few more teeth to be removed. A further dose of ethyl chloride only caused vomiting. The next day the operation was completed under ethyl chloride (also before breakfast), and she then, once more, emptied her stomach.

I never expect to see this patient again, but if I did I think I should advise her to have the extraction done at home under ether; at any rate if she had more than one or two easy teeth to be removed. I rather fancy, however, that we cleared her mouth.

The points that I want to be enlightened upon are these: (1) Whether I ought to have recognized her as an unsuitable case from the start? (2) Whether I probably made some error in the administration? And (3), whether, after failing on the first day, it was advisable to attempt gas again two days later, instead of making absolutely certain of success by employing ether.

DISCUSSION.

Dr. SILK asked if Mr. Collum had tried the plan of getting up a *plus* pressure in the bag directly the retching commenced; he had often found this efficacious. It was the same sort of thing as, when giving gas through the nose, one got up a *plus* pressure by cutting off the valves and over-distending the bag. He had done this also with satisfactory results in the case of hysterical movements and retching.

Mr. BOYLE wished to repeat Dr. Silk's question as to increasing the pressure of gas in the bag. He would also like to ask why, on the third occasion that the anæsthetic was given, Mr. Collum did not try to get the patient under gas and ether, or ether, and complete the extraction of all the teeth.

The PRESIDENT asked if Mr. Collum had tried the effect of rinsing out the mouth with weak carbolic lotion. In several cases he had stopped retching by making the patient gargle this. Once he stopped continuous retching during the administration of gas by a small dose of cocaine given by the mouth.

Mr. COLLUM, in reply, said he did not try *plus* pressure with the gas and oxygen; but he had no doubt he did so when using plain gas afterwards; it was his usual practice. In answer to Mr. Boyle as to why he did not give gas and ether instead of ethyl chloride on the third occasion, he did not think there was going to be vomiting each time, or gas and ether would have been given from the first. He had not tried carbolic lotion or cocaine for stopping vomiting, as he had not previously heard of their use for this purpose.

The Systematic Use of a Mouth-prop and Tongue-clip in General Anæsthesia.

By H. BELLAMY GARDNER.

THE principle which I wish to bring to your notice this evening is that of establishing an oral airway in general anæsthesia as a routine practice in every case, in order to avoid some of those contingencies which tend to cause obstructed respiration during unconsciousness. The method I would advocate is that of inserting a small aluminium wedge or mouth-prop between the side teeth before the administration of any general anæsthetic and maintaining it in place by supporting the chin. Directly unconsciousness has been produced the base of the tongue is drawn away from the pharyngeal wall by inserting a tongue-clip near the tip of the tongue and keeping it in place with the hand which holds the mask.

I need not say that the masseteric spasm which occurs during the induction of anæsthesia usually closes the oral airway. I need not labour the point that nasal respiration is in most cases insufficient to provide adequate aëration during unconsciousness. I need not point out that, as in apoplexy, obstruction of the airway and cyanosis occur as the immediate result of paralysis of the muscles of the tongue in anæsthesia; because these facts are beyond dispute and known to all of us.

What is so vitally needful for the medical student is for him to be able to prevent these difficulties from arising, so that he may devote his attention to the effect of the anæsthetic upon the patient's vitality and of the operation upon the blood-pressure and nervous system.

S. J. Meltzer,[1] of New York, speaks of the upper air-passages as the "Death Space" in relation to general anæsthesia. The use of this tragic term is exactly what is needed in teaching anæsthetics, to enforce a more thorough recognition of the mechanical conditions and intrinsic forces which tend to cause occlusion of this part of the airway during unconsciousness. I have not so far been able to trace the origin of the words "Death Space" beyond Meltzer, who uses them as if their import were well known in America; but I have never heard them here, and in the knowledge that great causes in politics have frequently been won by a catch phrase, I venture to start them on their flight. Because I am still convinced that unrecognized obstruction to breathing is the cause of a number of deaths which occur in anæsthesia, I venture to go further and suggest that the words "Asphyxial Death Space" be employed in teaching anæsthetics, in order to impress upon the student the primary necessity of maintaining a free airway in an unconscious patient.

I have used this method of establishing an oral airway increasingly for several years, and believe that if it were adopted as a routine practice it would be the means of saving many lives.

DISCUSSION.

Dr. BARTON said he once got into trouble for using a needle and thread for the tongue, before the introduction of tongue-clips, although if he had not secured it in that way it would have been badly bruised.

The PRESIDENT said the Section was much indebted to Mr. Bellamy Gardner for the idea of the tongue-clip and mouth-props. He supposed the props were inserted far back, over the molar teeth, and so did not exert that

[1] *Journ. Amer. Med. Assoc.*, Chicago, August 12, 1911, lvii, No. 7.

powerful lever action which a central dental prop did in depressing the lower jaw. He thought the mouth-prop and tongue-clip would be particularly useful for patients in the Trendelenburg posture, because it was often difficult to keep a free airway in that position.

Dr. BLUMFELD said that members would agree with Mr. Gardner as to the necessity of preserving a free airway. The routine use of a prop such as Mr. Gardner had seemed to be an admirable method, but he questioned if the routine use of a tongue-clip was good, from the point of view of teaching the student. If one encouraged men habitually to use the tongue-clip there would in the future be more torn tongues. Would not Mr. Gardner agree that in 75 per cent of cases, by the use of his little prop and the proper management of the lower jaw, a perfectly free airway could be maintained?

Mr. BOYLE agreed with all that Dr. Blumfeld had said. He agreed with Mr. Gardner that the use of a prop was very good, but he challenged the need for the routine use of tongue-clips. If students were taught to draw the lower jaw forward properly, and to apply pressure in the proper place to keep it so, they would have the airway patent. But his experience was that if students were left alone, they seized the patient by the chin, and pulled the chin up, so that the lower jaw was stuck against the upper and thus the airway was occluded. There must be many cases in which the tongue-clip was not necessary. If it were to be a routine, he thought there would be many split tongues.

Dr. CECIL HUGHES suggested there were very few occasions when a tongue-clip was necessary. It was so when the tongue became swollen, as under the administration of ether, when the administration was unnecessarily deep. He was very glad to hear about the prop; as a student he was taught always to use the mouth-prop.

Mr. BELLAMY GARDNER, in reply, said he did not see why students should be rougher in handling a patient than was necessary if a tongue-clip was put on. He had not yet hurt a tongue with the clip, and he did not see why anybody else should. Students must be told to use the utmost gentleness in every manipulation. If a mouth-prop were used as a routine when giving chloroform it would be very beneficial. When Mr. Boyle said that if the student would keep the lower jaw forward the airway would be free, he was not clear whether he meant the oral airway with a prop in the mouth or the nasal airway. In his opinion the nasal airway was not sufficiently free as a rule for the blood of the lungs to be provided with sufficient air in anæsthesia, at any rate with ether. It was not a tongue-forceps which he brought forward, but a tongue-clip armed with a pin, and the tongue was not crushed by the instrument. In no case had his attention been drawn by the patient to his tongue; indeed, the patient did not know his tongue had been touched. The passage of a tongue-clip or a needle and thread through the tongue to keep it in position in anæsthesia was painless on recovery. The whole object of the communication was the avoidance of the difficulties which arose from a swollen and paralysed tongue. It was to avert "alarum and excursions" during an operation; to prevent their occurrence by foreseeing their possibility.

Section of Anæsthetics.

December 1, 1911.

Dr. W. J. McCardie, President of the Section, in the Chair.

Crawford Williamson Long (1815-1879): the Pioneer of Anæsthesia and the first to suggest and employ Ether Inhalation during Surgical Operations.

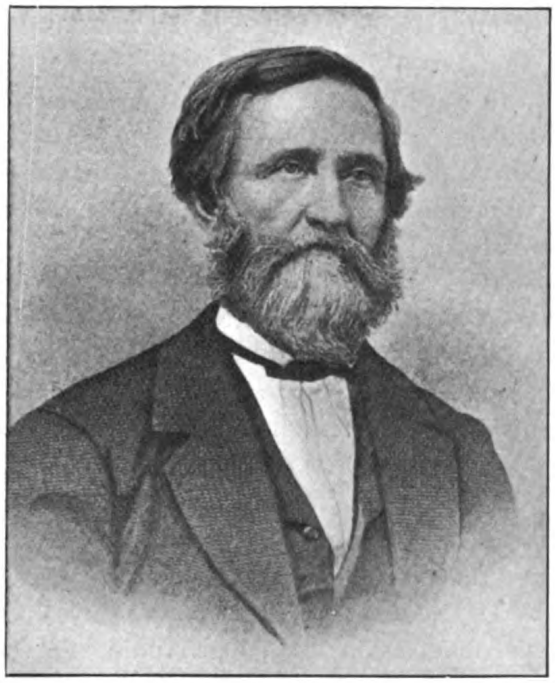
By Dudley W. Buxton, M.D.

Although the discovery of anæsthetics has bestowed the greatest blessing upon suffering humanity, its birth has been marked by much polemic and in some cases with scant recognition of those who have done so much for mankind. While disputants claiming for themselves, or their friends, the plaudits due to great discoverers, have rent the welkin with their angry cries and clamoured for a material solatium, one man, Crawford Williamson Long, reputed to be actually the first to produce artificial anæsthesia by inhalation of ether—was content to stand aside, calm and dignified, as one who strove to benefit his race while seeking for himself neither guerdon nor worldly honour. I propose to submit to your consideration the facts which have been accumulated and which to my mind prove Long's right to the world's acknowledgment that he was the pioneer of anæsthesia. It is impossible to do justice to the man without making at least an attempt to understand the environment in which he worked, and to analyse the professional and public opinion at the time he lived concerning the possibility of anæsthesia. Even so late as 1846 Sir Benjamin Brodie wrote: "Physicians and surgeons have been looking in vain, from the days of Hippocrates down to the present time, for the means of allaying or preventing bodily pain." In his mind were, no doubt, the spongia

somnifera of Theodoricus of Lucca, opium, cannabis indica, mandragora, and the whole gamut of nostrums vaunted through the centuries; the claims of Denis Papin, and of Cardan, whose magnet he averred could abrogate pain; the compression of the carotid arteries by Valverdi; nerve compression, suggested by Ambroise Paré and practised by James Moore and subsequently by John Hunter in what is now St. George's Hospital; Wardrop's (1832) bleeding to syncope, adopted by Richerand; and Mesner's claims to dissociate the astral from the corporeal body. Many methods had been called, but, as the sequel will show, few had been chosen.

But what of the man himself? Crawford Williamson Long was born on November 3, 1815, at Danielsberg, Georgia. His family were of high intellectual and social standing; his grandfather, Captain Samuel Long, who had emigrated from Ireland, was a philanthropist and man of affairs, prominent in the war of his troublous times. Captain Long's wife was a Miss Williamson, of Ulster, and her maiden name was perpetuated in that of her grandson. Of Long's father we learn that he was high in the esteem of his fellow-countrymen and the intimate friend of W. H. Crawford, after whom he named the subject of this history. Crawford held important official posts, twice represented his country in France, and was a Secretary of State. Long's mother, although an invalid, by her literary gifts added culture to her home. Thus was Crawford Long brought up amidst refined and public-spirited persons, an environment which could but make for those traits of character which in later life developed him into the best type of the man and of the physician. A few words will suffice to tell the subsequent story. "Studious and wise beyond his years," Long, whose extreme youth when he went to college earned him the sobriquet of "The Baby," graduated brilliantly at Franklin College — now the University of Georgia—when but 19 years old. His chief friend and class-mate was Alexander H. Stephens, later to be elected Vice-President of the Southern Confederacy, and to him and Long were erected monuments in Statuary Hall, Washington, the twain chosen by Georgia as the greatest men of her State. From Franklin College he proceeded to the University of Pennsylvania, and at the age of 23 graduated in Medicine. As was customary for those who could afford it, Long spent some time after this walking the hospitals of New York. The suggestion made to him by his teachers, who recognized his peculiar merit as a surgeon, that he should attach himself to the medical service of the United States Navy, was discountenanced by Long's father, at whose instance he

settled as a general practitioner in Jefferson, Georgia, and commenced civil practice in 1841 at the age of 26. It should be remembered that it was in this same year that Braid, of Manchester, made his trial of the " neurhypnotic trance," and Esdaile in India successfully operated upon hypnotized patients. Elliotson, who had migrated from St. Thomas's to University College Hospital, was then giving the full weight of his great mental power to mesmerism, although his book on " Surgical Operations performed in the Mesmeric State without Pain " was not



Crawford Williamson Long.

published until 1843. There is no doubt that, at the time of which we write, mesmerism or the hypnotic trance was regarded as the accomplished fact of anæsthesia, and that in the United States many of the leading men in medicine and surgery accepted it as the long hoped for panacea whereby suffering humanity could pass unflinchingly through the ordeals of the surgeon's knife. In France, Richerand had tried it and pronounced for its value, and other surgeons scarcely less eminent were willing to swallow the doubtful reputation of Anton Mesmer so long as they could benefit their patients by employing methods which

had been exploited by his fertile brain. Inhalational anæsthesia thus started with a heavy handicap. In 1846, to anticipate, the editors of the *New Orleans Medical and Surgical Journal* wrote apropos of ether inhalations: "That the leading surgeons of Boston could be captivated by such an invention as this under such auspices and upon such evidences of utility and safety as are presented by Dr. Bigelow excites our amazement. Why, mesmerism, which is repudiated by the savants of Boston, has done a thousand times greater wonders and without any of the dangers here threatened. What shall we see next?" *En passant*, let it be remarked that the sapient editors were condemned to see many things apparently unpalatable. But putting this aside, the quotation appears to show that the utility of mesmerism was widely accepted and constantly practised during surgical operations, even if the Bostonian savants condemned it; and further, that any fresh departure in the direction of promoting painless surgery was open to the most embittered criticism. The importance of this fact will be made manifest in the sequel of our narrative.

We must, however, retrace our steps for a brief space and try to obtain a clear idea of what was the current knowledge about ether before Long's first trials of its powers as an anæsthetic. In the books dealing with drugs and poisons ether found a place. It had been known since the thirteenth century and although some of its merits were recognized it was regarded as so dangerous as to be taboo. However, when through the enthusiasm of Dr. Beddoes, the work of Priestley and the discovery of oxygen and nitrous oxide, pneumatic medicine became a vogue ether again assumed prominence. The foundation of the Hotwells Hospital at Clifton, Bristol, by Dr. Beddoes gave that astute physician a wide experience in inhalational medication. He had studied Mayou's experiments and was familiar with Priestley's work, so that when young Humphry Davy, freed from his indentures to a doctor in Penzance, became his assistant, Beddoes was able to study the gases, the effects of which in the treatment of disease he has given in his delightful collected writings in four volumes entitled, "Considerations on the Medicinal use and on the Production of Factitious Airs." The first part was Beddoes's work, the second being due to James Watt, the engineer. In one of these volumes we find a letter from Dr. Pearson, of Birmingham, the accepted authority in his day upon therapeusis both in this country and in the States. In it he says that Beddoes's researches on "Factitious Airs" had led him to try "the vapour of ether" to relieve the suffering of phthisical patients and with benefit to them. Further, when Mitchell's

book on chemistry appeared Beddoes found to his chagrin that the chemist declared nitrous oxide to be a "virulent poison," so he re-commenced experiments with oxygen and nitrogen as well as with nitrous oxide gas. Davy, who undertook these researches, came to the conclusion that Mitchell was wrong, and that "the gaseous oxide of azote is perfectly expirable." He also announced that its inhalation cured the pain of an aching tooth and added, "It may probably be used with advantage during surgical operations in which no great effusion of blood takes place." Alas, no one took the hint and nitrous oxide remained for nearly fifty years the chief stock-in-trade of travelling lecturers who gave this gas to members of their audience to provoke exhilaration and semi-unconscious gyrations. These "frolics" were well known, and M. Filvée, in his "Lettres sur l'Angleterre" (1802),[1] mentions these revels as one of the many follies to which English people were prone.

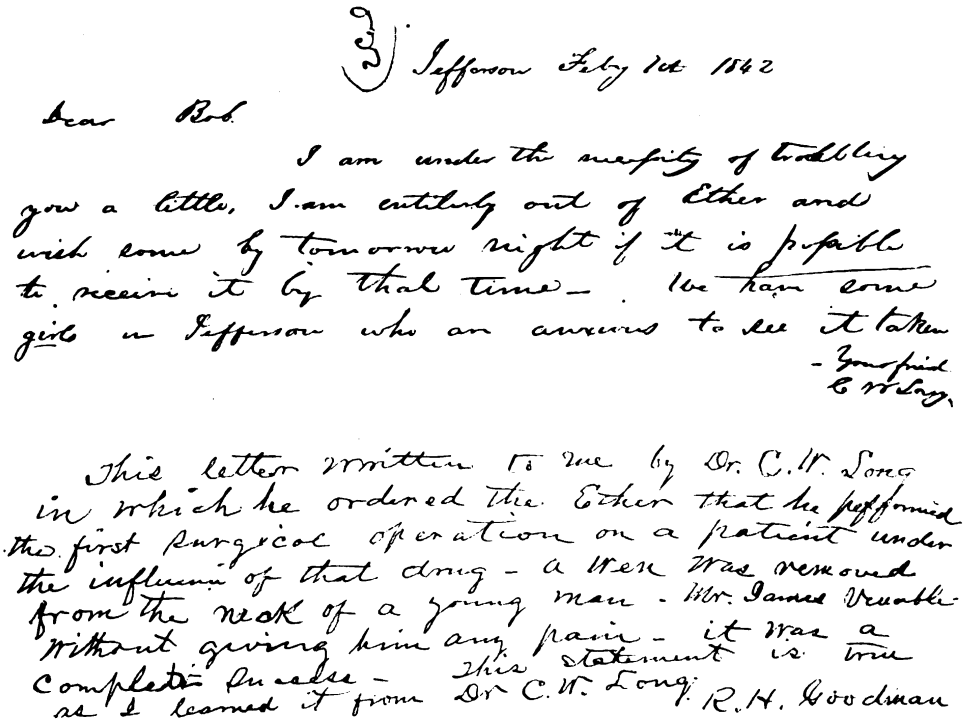
These practices became also common in the States, and it will be remembered it was at one of them that Horace Wells was present when he conceived the idea of using the gas as an anæsthetic. This of course happened much later, in 1844. But nitrous oxide gas needs a plant for its accurate manufacture, so that Pearson's suggestion of employing ether vapour as an exhilarant found ready acceptance. Cullen, whose works were read widely both here and in America, advocated ether, and Warren, of Boston, extolled its use in place of nitrous oxide. We see, then, that in 1841, when Long commenced practice, it was common knowledge that nitrous oxide produced exhilaration, but the suggestion of its employment as an anæsthetic by Davy was forgotten; ether was recognized as producing similar effects, but if we except the doubtful hint in the *Journal of Sciences and Arts* associated with the name of Faraday (1818), no one had grasped its greater merits, while the books of Pereira and others cautioned against its employment, since stupor and death, they averred, might readily be brought about. Long, it appears, had when a student actually inhaled ether during an "ether frolic," and was so far familiar with its effects.

Jefferson in those days was an isolated country village—one might almost say, "the world forgetting, by the world forgot"—so that Long had to pursue his own way relying upon himself and practically out of touch with the centres of surgical thought. His personal charm and high scientific attainments made his house a place of social resort for

[1] Quoted by Mr. Geo. Foy ("Janus") in his life of C. W. Long, 1900, p. 2.

the neighbourhood. Let Long himself tell us of the fateful happenings of December, 1841. He says (*see* Protocol I) :—

"In the month of December, 1841, or January, 1842, the subject of the inhalation of nitrous oxide gas was introduced in a company of young men in this village (Jefferson), and several persons present desired me to produce some for their use. I informed them that I had no apparatus for preparing or preserving the gas, but that I



Jefferson Feby 1st 1842

Dear Bob

I am under the necessity of troubling you a little. I am entirely out of Ether and wish some by tomorrow night if it is possible to receive it by that time— We have some girls in Jefferson who are anxious to see it taken

Yours truly
C W Long

This letter written to me by Dr. C. W. Long in which he ordered the Ether that he performed the first surgical operation on a patient under the influence of that drug - a tumor was removed from the neck of a young man - Mr. James Venable without giving him any pain - it was a complete success - This statement is true as I learned it from Dr C. W. Long R. H. Goodman

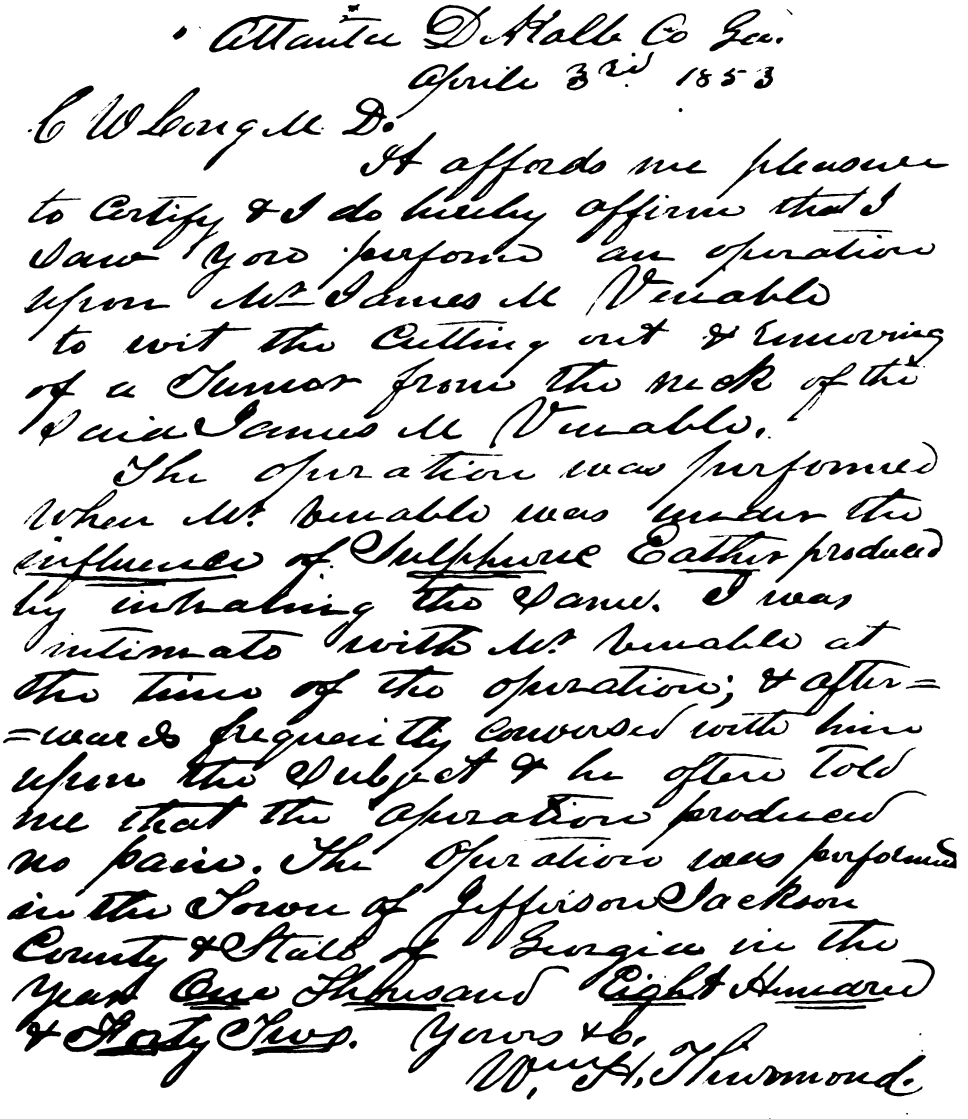
FACSIMILE 1.

Letter from Dr. C. W. Long to R. H. Goodman, ordering the ether used for the first operation done under ether, and a covering letter from R. H. Goodman.

had a medicine (sulphuric ether) which would produce equally exhilarating effects; that I had inhaled it myself, and considered it as safe as the nitrous oxide gas. One of the company stated that he had inhaled ether while at school, and was then willing to inhale it. The company were all anxious to witness its effects. The ether was introduced. I gave it first to the gentleman who had previously inhaled it, then inhaled it myself, and afterwards gave it to all persons present. They were so much pleased with the exhilarating effects of ether, that they

afterwards inhaled it frequently, and induced others to do so, and its inhalation soon became fashionable in this country, and in fact extended from this place through several counties in this part of Georgia.

"On numerous occasions I have inhaled ether for its exhilarating properties, and would frequently, at some short time subsequent to its inhalation, discover bruises or painful spots on my person, which I had



Atlanta DeKalb Co Geo.
April 3rd 1853

C W Long M D.

It affords me pleasure to Certify & I do hereby affirm that I saw You perform an operation upon Mr James M Venable to wit the Cutting out & removing of a Tumor from the neck of the said James M Venable.

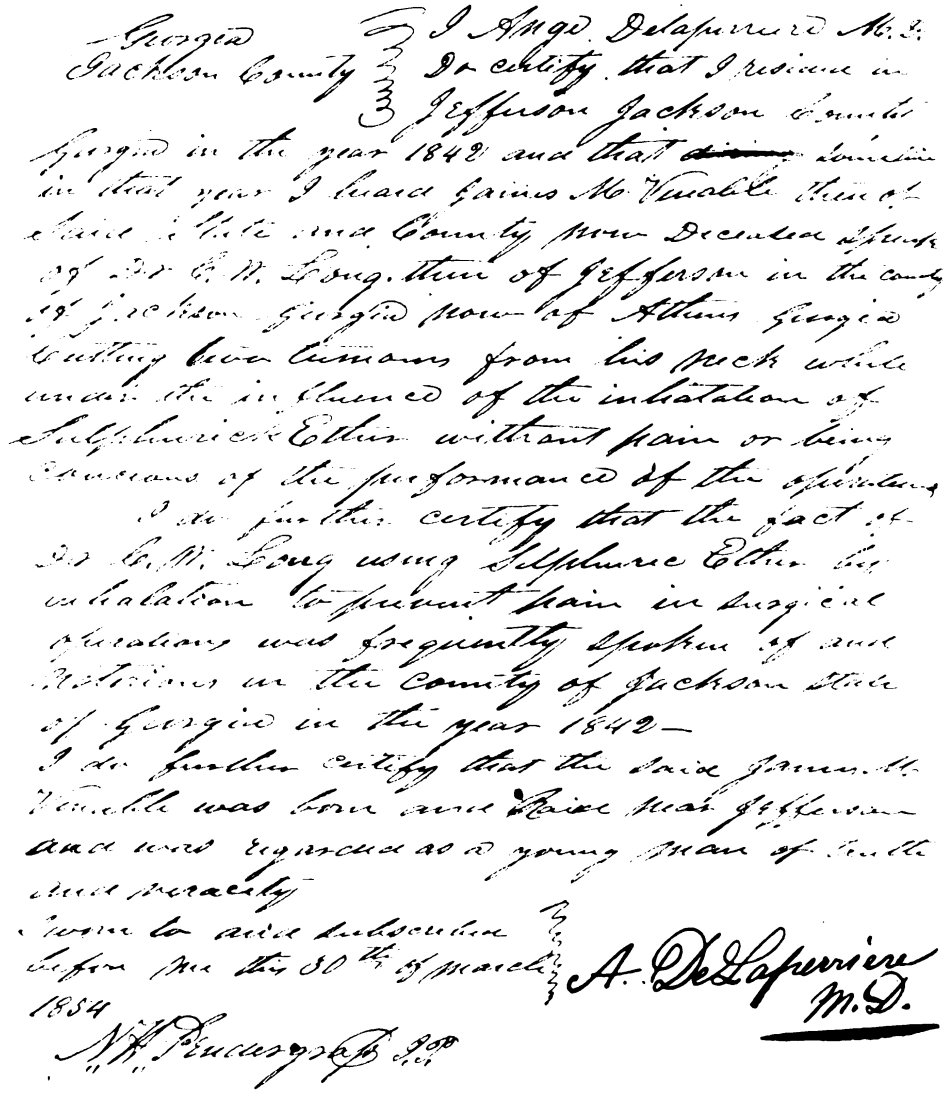
The Operation was performed when Mr Venable was under the influence of Sulphuric Æther produced by inhaling the same. I was intimate with Mr Venable at the time of the operation; & afterwards frequently conversed with him upon the Subject & he often told me that the operation produced no pain. The Operation was performed in the Town of Jefferson Jackson County & State of Georgia in the year One Thousand Eight Hundred & Forty Two. Yours &c.

Wm H. Thurmond.

FACSIMILE 2.

Statement of a witness at the first operation done under ether.

no recollection of causing, and which I felt satisfied were received while under the influences of ether. I noticed my friends, while etherized, received falls and bangs, which I believed were sufficient to produce pain on a person not in a state of anæsthesia, and on questioning them they uniformly assured me that they did not feel the least pain from

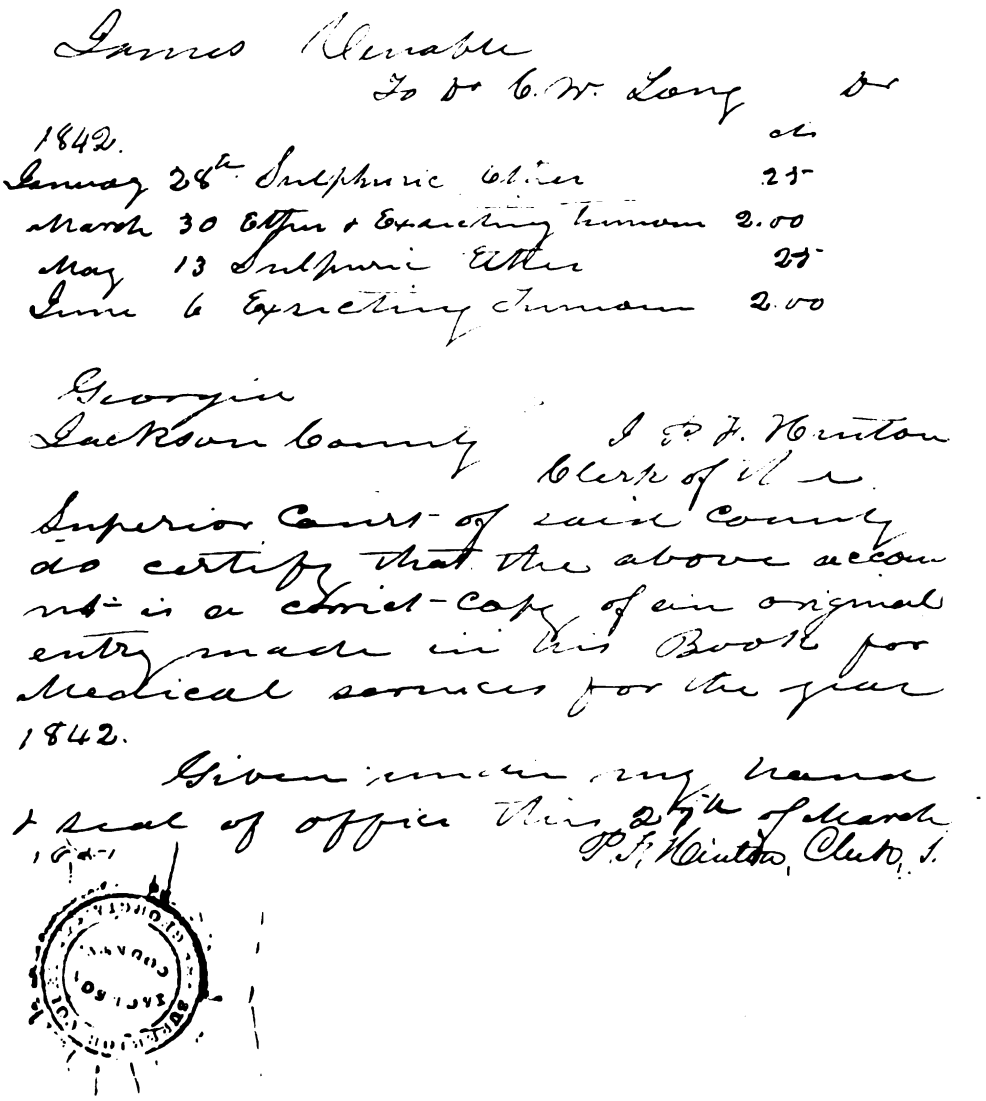


FACSIMILE 3.

Letter referring to the first operations done under ether, and stating that Long's use of ether as an anæsthetic was "notorious in the County of Jackson, Georgia."

these accidents. These facts are mentioned that the reasons may be apparent why I was induced to make an experiment in etherization."

The first trial of his theory was made on March 30, 1842, and the narrative is given in Protocols (*see* Protocols I, VI, VII, and X) appended to this paper. James M. Venable was etherized by Long, who poured ether upon a towel, and when the patient was profoundly



James Venable
To Dr C. W. Long Dr

1842. Dr
January 28th Sulphuric Ether 25
March 30 Ether & Exciting Inhalation 2.00
May 13 Sulphuric Ether 25
June 6 Exciting Inhalation 2.00

Georgia
Jackson County I C. F. Hinton
Clerk of the
Superior Court of said County
do certify that the above account is a true copy of an original
entry made in this Book for
Medical services for the year
1842.
Given under my hand
& seal of office this 24th of March
1849
C. F. Hinton, Clerk S.

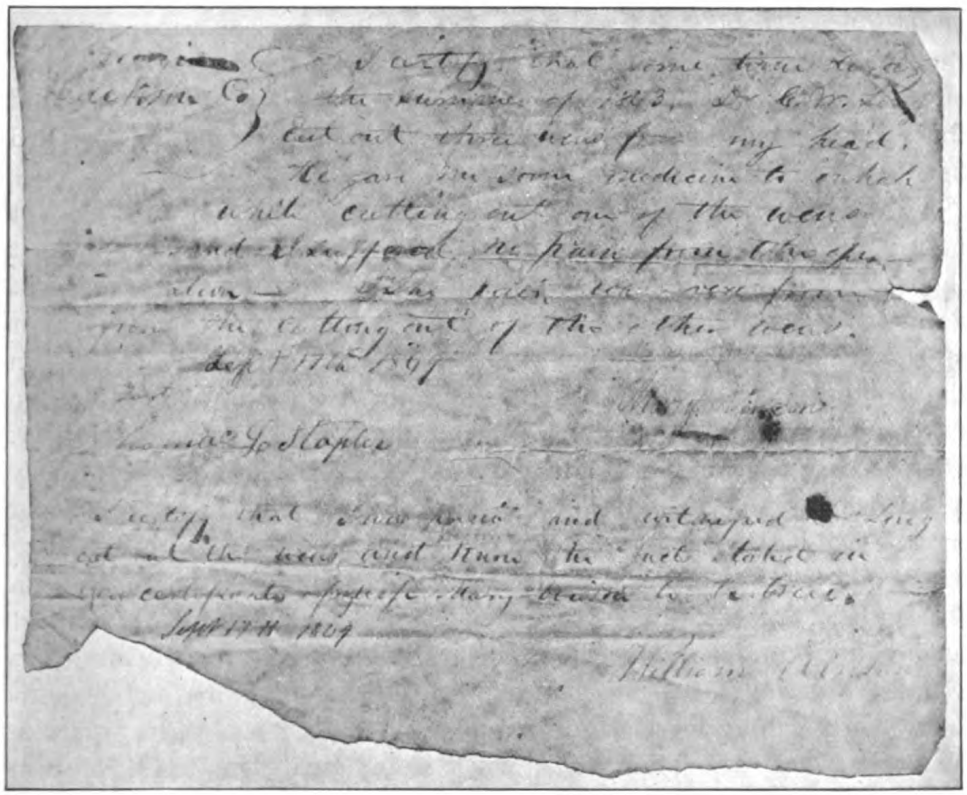
FACSIMILE 4.

Dr. Long's bill for the first operation done under ether, with the County Clerk's certificate.

unconscious he removed a cyst from the back of the patient's neck. No pain was felt. This was four and a half years before Morton's first case. Subsequently Long used ether on several occasions, eight it is said, and every case was successful. That he did not employ it more largely was due firstly to the fact that his practice was one in which operations were seldom demanded, and secondly because the older practitioners around him urged upon him the "danger" of the method and the disastrous results which would follow a fatal accident, such an accident as they were good enough to say must soon occur. Indeed, Long was more than once threatened with pains and penalties by the community in which he lived if he persisted in his ether practices! Now, although Long did not conceal his work, for it was known widely in the neighbourhood (*see* Protocol III), he took no occasion until later to read a paper before a medical society dealing with his experiments. This silence has been misconstrued. The simplest justice, however, must explain Long's reticence in a sense which redounds wholly to his credit. Long believed in his method but recognized it needed careful working out; he patiently experimented when patients seemed suitable; when they were not available he tried it on himself and on his pupils. He knew that the public were in favour of mesmerism, a system which he regarded with disfavour, and he had no surgeon of eminence in his neighbourhood to whom he could apply for extended experience. The doctors who knew of his efforts were adverse to them, and so he preferred to wait and gain experience before attempting to exploit a discovery which might have less in it than appeared at first sight. He was alive to the fact that some might believe they found in his practice a development of hypnotism, an anæsthesia by suggestion (*see* p. 38). But while he waited events were happening in the bigger world of which Long recked nothing. Horace Wells had "discovered" nitrous oxide gas, had tried it and had been successful in Hartford, Connecticut, 1844. His essay before the surgical world in the General Hospital, Massachusetts, had been a fiasco; he (Wells) had been shrieked out of the theatre with cries of "humbug." With or without the aid of Dr. Jackson, a person of scientific attainments and State geologist and chemist, Morton had administered ether vapour to himself, subsequently to one or two patients, and ultimately had been permitted by Warren, the surgeon, to administer his nostrum to a patient in the "General Hospital." "This, gentlemen," had said Warren, "is no humbug": anæsthesia by inhalation was an accepted fact.

These events occurred in 1846, but sadly enough the "ether con-

troversy," which embroiled Morton, Jackson and Wells, involves much that we would rather not dwell upon, since the details are far from pleasant reading. We must, however, refer to some points since they connect the story with that of Crawford Long. We spoke of Morton using a "nostrum"; it was sulphuric ether with flavouring additions, and was termed "Letheon." Its identity with ether was challenged, and eventually the medical profession refused to allow further public



FACSIMILE 5.

Certificate of Mary Vinson and her husband, referring to an operation on the former in 1843, done under ether.

trials without a disclosure of its nature and composition. The attempt was made to protect Letheon by a patent, and to restrict its employment unless a royalty was paid. An English patent was actually granted. W. T. G. Morton was not a medical man, and probably saw no objection to reaping a golden harvest from what he no doubt honestly believed to be his discovery. This restriction on the use of Letheon has been denied, but the protocol of the letter of Dr. Charles A. Davis appears

to substantiate the fact (*see* Protocol VIII). We know the sequel: the world awoke to the anæsthetic value of ether. There can be no question that, whatever motives actuated him, Morton's public exhibitions of ether in an important surgical centre were the direct means of publishing the discovery to all civilized communities. Failing to support the rights which he fondly hoped would protect his patent of Letheon, Morton sought to secure a grant from Congress, and to be proclaimed the discoverer of ether, and the first to apply it for the purposes of anæsthesia. Dr. Jackson advanced that he had a prior right, asserting that he had suggested the use of ether to Morton, and that the latter's employment of it was at his (Jackson's) instance. Into the merits of this controversy we need not enter, since both Morton and Jackson had been anticipated by Long.

It has been pointed out already the reasons why Long did not publish his first cases; however, when Morton's success was bruited about the duty of making a statement concerning his own cases was brought home to Long, and he put himself into communication with the editor of the *Southern Medical and Surgical Journal.* In 1849 we find in the columns of that journal the following: "A few months ago Dr. Long informed us of his early attempts at etherization in surgery. He was then informed that any claims set up at this late day to priority of discovery would be severely criticized, if not violently resisted; and that he had best, therefore, do all he could to fortify his position."

Long, in his communication to the Medical Society of Georgia (December, 1849), explains that so soon as he saw from the current medical literature (1846) that Letheon had been successfully used, he actually commenced a paper for publication detailing his own experiences with sulphuric ether; unhappily the exigencies of his practice intervened and the paper was never completed. In 1849, Morton petitioned Congress for a monetary reward for his discovery, and his claim was opposed by Jackson and Wells. In 1854, after much persuasion, Long was induced to write to Senator Dawson detailing his own work. The Senator sent Dr. Jackson, a curious choice when it is remembered that that gentleman was one of those who claimed the laurels of having discovered ether. The interview took place at Athens, Georgia, to which town Dr. Long had removed in 1851, and Long told his story simply but so effectively as to convince Jackson of its truth (*see* p. 42). The matter was ventilated in Congress by Senator Dawson, and although Morton's petition was never granted yet to Crawford Long no recognition was vouchsafed. It is significant that Dr. Jackson urged

upon Long that they, Jackson and Long, should unite interests, and as partners claim that they had respectively discovered the use of ether as an anæsthetic, and actually essayed experiments and successfully proved the safety and value of the method. Long was not tempted, he declined in words of striking dignity. He writes: "Our claims are rival, and permit me, sir, to say that although our claims are conflicting, I would not knowingly say anything in the article (he was preparing a full statement of his work in the form of an article) which would be displeasing to you. . . . Still, it becomes each one of us to use all honorable means to advance his own claims, and I know you will not blame me for attending to this matter, which so much concerns my reputation." So the matter dropped and men forgot Long or minimized his work, and on Ether Day celebrations to Morton is accorded the palm. In 1877, Dr. Marion Sims championed Long's cause, but, unfortunately, his communication so well conceived was hurriedly executed and introduced the inaccurate statement that S. C. Wilhite had suggested to Long the idea of using ether. Wilhite himself contradicted this (*see* Protocol IX); as a matter of fact he did not come to live with Long as a student until 1844.

So we come to the close of the story of Long and his great discovery, the story of a simple-minded man who, amidst the arduous labours of a scattered country practice, conceived a great idea and, in the teeth of opposition and though haunted by the fear lest disaster might arise and ruin his professional name and reputation, yet had the courage to test his theory by experiment, and so obtained proof of its accuracy. Surely such a man was worthy to be the author of so momentous a discovery, and of being called one of the greatest men brought forth by a mighty nation.

A few words will suffice to trace the closing years of Long's useful life. He took no further steps to enforce his claims, but patiently laboured for his country and his people through the troublous years of the war, and at length, when brighter days returned, he again built up his practice and ultimately died in harness, his last words being an anxious inquiry as to the welfare of a patient at whose bedside he was when his mortal sickness seized him.

There have been memorials erected to the memory of Long, the discoverer of ether anæsthesia, one in Paris and one at Jefferson, Georgia, and in Boston men see a monument in white marble bearing the inscription: "To the Discoverer of Anæsthesia." It bears no name.

Shall we not in England accord the first place to Long, and in no

unfriendly spirit claim for Morton and Wells less exalted niches in the Temple of Fame? For we may not forget that to them, and indeed to Dr. Jackson, the world owes very much, since if not the first to employ ether as an anæsthetic, they made themselves the heralds of an epoch-making discovery and they were in ignorance when they did so that Crawford Williamson Long had anticipated them.

Ether, 1842-1911.

The narrative given by Long of his first administration of ether to a patient, in 1842, that by Morton, which is more detailed, referring to his demonstration of the effects of Letheon on a patient in the General Hospital, Massachusetts (1846) and that given in great detail of the first ether operation performed in a London Hospital—viz., University College Hospital—when Liston operated, reveal the fact that very little was known about anæsthetics and less about methods. In the one case a towel was employed, in the other two a primitive inhaler consisting of an ether chamber and a series of tubes connecting it with a face-piece.

When Simpson introduced chloroform at the end of 1847, ether, at all events in this country, was neglected in favour of the newer claimant, chloroform. The perils of the latter incident to the methods adopted in its exhibition soon led to fresh attempts being made to employ ether or some mixture of it and chloroform. The Committee of the Royal Medical and Chirurgical Society in their Report published in 1864 extolled ether's safety, but pointed out as its inherent drawback that the induction of anæsthesia by it was too slow for convenience. Then came the rational attempts of Clover, Ormsby, and many more, to remove this disability by the use of closed inhalers. No practical attempt was, however, made to study a percentage method for etherization. The very safety of the drug became its chiefest danger, since etherists were so obsessed by the fact that ether does not lower blood-pressure or cause cardiac collapse through depression, that they failed to recognize the perils incident to over-stimulation, especially in asthenic persons. The dangers of post-operative chest troubles were not existent in the pioneer days of ether because the operations performed were comparatively brief and the surgeons taught in pre-anæsthetic days prided themselves on their celerity in operating, and indeed were appraised by the public for this quality. To-day there is no haste, the advance of surgery has invaded the regions once immune from the knife; if ether is adopted for this wider range of operations it is necessary that methods of using it must follow on other than the traditional lines. To

safeguard against excessive dosage we rely on mixed methods, such as the preliminary hypodermic injection of scopolamine, morphine and atropine; we adopt an open mask, evaporating from an enormously expanded area provided by many folds of gauze, and so obtain a more complete nebulation of our vapour; we introduce ether directly into the blood-stream in an artificial circulating fluid of physiological saline by intravenous infusion, with the hope of maintaining an equable and low-grade partial saturation of the neural tissue. In every case, we must remark, the supreme difference consists in the fact that we have replaced a method of excessive dosing by one of moderation and in most instances capable of rapid variation in the strength of ether employed. We have been too overborne by a priori reasoning, too obedient to traditional authority. Whether our newer methods may not introduce fresh dangers we cannot as yet say; if they do, it will be probably because our technique is at fault, and this must be amended. It is startling, when we think of the early workers, to find modern etherists safely and easily encompassing anæsthesia with ether for tongue or jaw operations. Yet such is the case. For example, through the kindness of Mr. Page I saw him accomplish this, following his adaptation of Dr. Crile's method of nasal intubation and pharyngeal blocking. By the intratracheal insufflation method now so efficiently carried out in America we find ether conveyed into the lungs without the inconveniences formerly incident to this method of introducing ether by oral inspiration. The experience gained gives promise of even more efficient plans of using ether, of saving life and enabling the modern surgeon to perform his tasks, often almost daunting in their complexity and difficulty, without the added anxiety of an anæsthesia either imperilling the patient's life or necessarily imperfect owing to the patient's reaction towards the drug employed. If Long's work was the first step towards what we have achieved to-day, and it was so, to him we owe this much that we do his memory great and abiding honour. But we must realize also that anæsthesia to-day is on its trial, it must advance and trample on tradition and rely upon experiment unless we are content to forsake the hope of founding a science, and are willing to content ourselves with a mere handicraftsman's place in the ranks of the medical profession.

I desire to express my thanks to Mrs. Long Taylor, through whose kindness I have been furnished with documentary evidence of the accuracy of the facts I have advanced about her father, Crawford W. Long, also to Dr. George Foy, of Dublin, to whose unique knowledge of this matter and collection of memorials of Crawford Long I have been most generously made welcome.

PROTOCOLS.

(I) Statement made by Dr. C. W. Long, 1849, to the Medical Society of Georgia.

For nearly three years the various medical journals have contained numerous articles on the employment of sulphuric ether by inhalation, for the purpose of rendering patients insensible to pain during surgical operations.

The first notice I saw of the use of ether, or rather of Dr. Morton's "Letheon," as an anæsthetic, was in the editorial of the *Medical Examiner* for December, 1846, in which the Editor gives the following extract from a paper by Dr. H. J. Bigelow, contained in the *Boston Journal:* "The preparation (Letheon) is inhaled from a small two-necked glass globe, and smells of ether, and is, we have little doubt, an ethereal solution of some narcotic substance."

Having on several occasions used ether, since March, 1842, to prevent pain in surgical operations, immediately after reading this notice of "Letheon" I commenced a communication to the Editor of the *Medical Examiner* for publication in that journal, to notify the medical profession that sulphuric ether, when inhaled, would of itself render surgical operations painless, and that it had been used by me for that purpose for more than four years.

I was interrupted when I had written but a few lines, and was prevented, by a very laborious country practice, from resuming my communication, until the *Medical Examiner* for January, 1847, was received, which reached me in a few days after reading the December number. It contained several articles, giving accounts of different experiments in etherization, in which surgical operations were performed without pain. On reading these articles, I determined to wait for a few months before publishing an account of my discovery, and see whether any surgeon would present a claim to having used ether by inhalation in surgical operations prior to the time it was used by me.

A controversy soon ensued between Messrs. Jackson, Morton and Wells, in regard to who was entitled to the honour of being the discoverer of the anæsthetic powers of ether, and a considerable time elapsed before I was able to ascertain the exact period when the first operations were performed. Ascertaining this fact, through negligence I have now permitted a much longer time to elapse than I designed, or than my professional friends with whom I had consulted advised: but as no account has been published (so far as I have been able to ascertain) of the inhalation of ether being used to prevent pain in surgical operations as early as March, 1842, my friends think I would be doing myself injustice not to notify my brethren of the medical profession of my priority of the use of ether by inhalation in surgical practice.

I know that my interests have suffered from not making an earlier publication, and I would not be persuaded at this late stage of the ether controversy to present my claim to being the first to use ether as an anæsthetic in surgical operations, if I were not fully satisfied of my ability to establish its justness.

In the month of December, 1841, or January, 1842, the subject of the inhalation of nitrous oxide gas was introduced in a company of young men in this village (Jefferson), and several persons present desired me to produce some for their use. I informed them that I had no apparatus for preparing or preserving the gas, but that I had a medicine (sulphuric ether) which would produce equally exhilarating effects: that I had inhaled it myself, and considered it as safe as the nitrous oxide gas. One of the company stated that he had inhaled ether while at school, and was then willing to inhale it. The company were all anxious to witness its effects. The ether was introduced. I gave it first to the gentleman who had previously inhaled it, then inhaled it myself, and afterwards gave it to all persons present. They were so much pleased with the exhilarating effects of ether, that they afterwards inhaled it frequently, and induced others to do so, and its inhalation soon became fashionable in this country, and in fact extended from this place through several counties in this part of Georgia.

On numerous occasions I have inhaled ether for its exhilarating properties, and would frequently, at some short time subsequent to its inhalation, discover bruised or painful spots on my person, which I had no recollection of causing, and which I felt satisfied were received while under the influence of ether. I noticed my friends while etherized received falls and bangs, which I believed were sufficient to produce pain on a person not in a state of anæsthesia, and on questioning them they uniformly assured me that they did not feel the least pain from these accidents. These facts are mentioned that the reasons may be apparent why I was induced to make an experiment in etherization.

The first patient to whom I administered ether in a surgical operation was Mr. James M. Venable, who then resided within two miles of Jefferson, and at present lives in Cobb County, Georgia. Mr. Venable consulted me on several occasions in regard to the propriety of removing two small tumours situated on the back of his neck, but would postpone from time to time having the operations performed, from dread of pain. At length I mentioned to him the fact of my receiving bruises while under the influence of the vapour of ether, without suffering, and as I knew him to be fond of and accustomed to inhale ether, I suggested to him the probability that the operations might be performed without pain, and proposed operating on him while under its influence. He consented to have one tumour removed, and the operation was performed the same evening. The ether was given to Mr. Venable on a towel, and when fully under its influence I extirpated the tumour. It was encysted and about ½ in. in diameter. The patient continued to inhale ether during the time of the operation, and when informed it was over seemed incredulous, until the tumour was shown him. He gave no evidence of suffering during the operation, and assured me after it was over that he did not experience the slightest degree of pain from its performance. This operation was performed on March 30, 1842.

The second operation I performed upon a patient etherized was on June 6, 1842, and was on the same person for the removal of another small tumour.

This operation required more time than the first, from the case of the tumour having formed adhesions to the surrounding parts. The patient was insensible to pain during the operation, until the last attachment of the cyst was separated, when he exhibited signs of slight suffering, but asserted after the operation was over that the sensation of pain was so slight as scarcely to be perceived. In this operation the inhalation of ether ceased before the first incision was made. Since that time I have invariably directed patients, when practicable, to continue its inhalation during the time of the operation.

Having so long neglected presenting my claim to the discovery of the anæsthetic power of ether, for the purpose of satisfying the minds of all of its justness, I have procured, I conceive, a sufficient number of certificates to establish the claim indisputably. I present, first, the certificate of James M. Venable, the patient on whom the first experiments in etherization were made, and no comments on it, I conceive, are necessary.

Mr. James M. Venable's Certificates.

I, James M. Venable, of the County of Cobb and State of Georgia, on oath, depose and say, that in the year 1842, I resided at my mother's in Jackson County, about two miles from the village of Jefferson, and attended the village academy that year. In the early part of the year the young men of Jefferson and the country adjoining were in the habit of inhaling ether for its exhilarating powers, and I inhaled it myself frequently for that purpose, and was very fond of its use.

While attending the academy, I was frequently in the office of Dr. C. W. Long, and having two tumours on the back and either side of my neck, I several times spoke to him about the propriety of cutting them out, but he postponed the operation from time to time. On one occasion, we had some conversations about the probability that the tumours might be cut out while I was under the influence of sulphuric ether, without my experiencing pain, and he proposed operating on me while under its influence. I agreed to have one tumour cut out, and had the operation performed that evening after school was dismissed. This was in the early part of the spring of 1842.

I commenced taking the ether before the operation was commenced, and continued it until the operation was over. I did not feel the slightest pain from the operation, and could not believe the tumour was removed until it was shown to me.

A month or two after this Dr. C. W. Long cut out the other tumour, situated on the same side of my neck. In this operation I did not feel the least pain until the last cut was made, when I felt a little pain. In this operation, I stopped inhaling the ether before the operation was finished.

I inhaled the ether, in both cases from a towel, which was the common method of using it.

JAMES M. VENABLE.

(Sworn to before me) ALFRED HANES, J.P.

Georgia, Cobb County, *July* 23, 1849.

Certificate of Andrew J. Thurmond.

I certify that I was a pupil in the academy in Jefferson, Jackson County, Georgia, in the year 1842. Some time during the spring of that year I was present and witnessed Dr. C. W. Long cut a small tumour from the neck of James M. Venable.

I am well acquainted with the smell of sulphuric ether, and I know that Mr. Venable inhaled it, before, and during the time of the operation. He made no sign of suffering pain during the operation; and after the tumour was cut out he asserted that he did not feel any pain from the cutting out of the tumour.

A few months after this operation, Mr. Venable informed me that Dr. Long had cut out another tumour from his neck, while he was under the effects of ether, and that he did not feel any pain from the operation. Mr. Venable was a pupil in the Academy during the year 1842, and I was intimate with him and heard him speak of the operations frequently, and he always asserted that they were performed without pain.

I know the operation was performed in the year 1842; my brother, William H. Thurmond, had charge of the Academy that year, and it was the only time I was a pupil in the Academy.

August 21, 1849. ANDREW J. THURMOND.

In addition to Mr. Venable's, I present the certificates of E. S. Rawls and Wm. H. Thurmond, who were present and witnessed one or both operations (*see* Facsimile 2).

My third experiment in etherization was made on July 3, 1842, and was on a negro boy, the property of Mrs. S. Hemphill, who resides nine miles from Jefferson. The boy had a disease of a toe, which rendered its amputation necessary, and the operation was performed without the boy evincing the least sign of pain. I present Mrs. Hemphill's statement of the report the boy gave her of the operation on his return home, which I conceive is sufficient on this point.

These were all the surgical operations performed by me during the year 1842, upon patients etherized, no other case occurring in which I believed the inhalation of ether applicable. Since 1842 I have performed one or more surgical operations annually on patients in a state of etherization.

The question will no doubt occur, why did I not publish the results of my experiments in etherization soon after they were made? I was anxious, before making my publication, to try etherization in a sufficient number of cases to satisfy my mind that anæsthesia was produced by the ether, and was not the effect of the imagination, or owing to any insusceptibility to pain in the persons experimented upon.

At the time I was experimenting with ether, there were physicians high in authority and of justly distinguished character, who were the advocates of

mesmerism, and recommended the induction of the mesmeric state as adequate to prevent pain in surgical operations. Notwithstanding thus sanctioned, I was an unbeliever in the science, and of the opinion that if the mesmeric state could be produced at all it was only on those of "strong imagination and weak minds," and was to be ascribed solely to the workings of the patients' imaginations. Entertaining this opinion, I was the more particular in my experiments in etherization.

Surgical operations are not of frequent occurrence in a country practice, and especially in the practice of a young physician, yet I was fortunate enough to meet with two cases in which I could satisfactorily test the anæsthetic powers of each. From one of these patients I removed three tumours the same day. The inhalation of ether was used only in the second operation, and was effectual in preventing pain, while the patient suffered severely from the extirpation of the other tumours. In the other case I amputated two fingers of a negro boy. The boy was etherized during one amputation, and not during the other; he suffered during one operation and was insensible during the other.

I have procured the certificates of the lady from whom the tumours were removed and of her husband, who was present and witnessed the operations (*see* Facsimile 5). These certificates were produced in preference to those establishing other operations, because they not only show that the experiments were continued from year to year, but also show that they were conducted so as to test the power of etherization.

After fully satisfying myself of the power of ether to produce anæsthesia, I was desirous of administering it in a severer surgical operation than any I had performed. In my practice, prior to the published account of the use of ether as an anæsthetic, I had no opportunity of experimenting with it in a capital operation, my cases being confined, with one exception, to the extirpation of small tumours and the amputation of fingers and toes.

I have stated that ether was frequently inhaled in this and some of the adjoining counties for its exhilarating effects, and although I am conscious that I do not deserve any credit for introducing its use for that purpose, yet as others through their friends have claimed to be the first to show its safety, most of the certificates I have obtained establish the fact of its frequent inhalation for its exhilarating effects. I met with R. H. Goodman, who was present the night ether was first inhaled in Jefferson, and who removed to Athens, and introduced its inhalation in that place, and presented his certificate. All the young gentlemen who were present the night I first administered ether, with one exception, are living, and their certificates can be procured, if necessary.

I have now, in a very concise manner, presented a "plain unvarnished" account of some of my experiments in etherization, and have said nothing of the comparative methods of ether, and other anæsthetics, because that was foreign to my present subject. Had I been engaged in the practice of my profession in a city where surgical operations were performed daily, the discovery would, no

doubt, have been confided to others, who would have assisted in the experiments, but occupying a different position. I acted differently, whether justifiable or not. The result of my second experiment in etherization was such as to lead me to believe that the anæsthetic state was of such short duration that ether could only be applicable in cases in which its effects could be kept up, by constant inhalation, during the time of the performance of the operation. Under this impression, up to January, 1847, I had not used ether, but in one case, in extracting teeth, and thus deprived myself of experimenting in the only class of cases which are of frequent occurrence in a country practice.

While cautiously experimenting with ether, as cases occurred, with a view of fully testing its anæsthetic powers, and its applicability to severe as well as minor surgical operations, others more favourably situated engaged in similar experiments, and consequently the publication of etherization did not "bide my time." This being the case, I leave it with an enlightened medical profession to say whether or not my claim to the discovery of etherization is forfeited, by not being presented earlier, and with the decision which may be made I shall be content.

(II) CERTIFICATE OF ENTRY IN Dr. C. W. LONG'S ACCOUNT BOOK *re* FIRST OPERATION ON JAMES VENABLE. (*See* Facsimile 4.)

JAMES VENABLE,

1842. To Dr. C. W. LONG, *Dr.*

January 20, sulphuric ether	0.25 cents.
March 30, sulphuric ether and exsecting tumour	2.00 dollars.
May 13, sulphuric ether	0.25 cents.
June 6, exsecting tumour	2.00 dollars.

This entry was certified as correct.

Georgia, Jackson County.

I, P. F. Hinton, Clerk of the Superior Court of said County, do certify that the above account is a correct copy of an original entry made in his book for the year 1842. Given under my hand and seal of office this 27th of March, 1854.

(Signed) P. F. HINTON, Clerk.

(III) CERTIFICATES PROVING THAT DR. LONG'S USE OF ETHER AS AN ANÆSTHETIC WAS COMMON KNOWLEDGE IN THE DISTRICT OF JEFFERSON AND ATHENS WHERE HE LIVED. (*See* Facsimile 3.)

Georgia, Jackson County.

I, Ange de Laperriere, M.D., do certify that I resided in Jefferson, Jackson County, Georgia, in the year 1842, and that sometime in that year I heard James M. Venable, then of said County, speak of Dr. C. W. Long's cutting out two tumours from his neck while under the influence of the inhalation of sulphuric ether, without pain, or being conscious of the performance of the operation.

I do further certify that the fact of Dr. C. W. Long using ether by inhalation to prevent pain in surgical operations was frequently spoken of and notorious in the County of Jackson, Georgia, in the year 1842.

A. DE LAPERRIERE, M.D.

Sworn to and subscribed before me this 30th of March, 1854.

N. H. PENDERGRASS, J.P.

Athens, Clarke County, Georgia.

I, the undersigned, do certify that in May, 1843, I assisted Dr. R. D. Moore in amputating the leg of a coloured boy, Augustus, then the property of Mr. William Stroud, who resided in this County; and that I distinctly recollect hearing Dr. R. D. Moore say: "If I had thought of it before leaving home I would have tried Dr. C. W. Long's great discovery, namely, the administration of sulphuric ether as an anæsthetic in performing the operation." Having neglected to bring the ether, Dr. Moore finally concluded to influence the patient with morphia, under which influence the operation was performed.

JOS. B. CARLTON, M.D.

(IV) LETTER SUBMITTED BY DR. H. H. YOUNG OF THE JOHNS HOPKINS HOSPITAL PROVING DR. LONG'S METHOD OF ETHERIZATION PRODUCED FULL ANÆSTHESIA.

Cohutta, Georgia,
January 15, 1897.

Dr. HUGH H. YOUNG.

Dear Sir,—The patient was placed in a recumbent position on a bed, with the hand to be operated on the front for convenience of the surgeon. Dr. Long poured ether on a towel and held it to the patient's nose and mouth too, to get the benefit of inhalation from both sources. Dr. Long determined when

the patient was sufficiently etherized to begin the operation by pinching or pricking him with a pin. Believing that no harm would come of its use for a considerable length of time, he profoundly anæsthetized the patient, then gave me the towel and I kept up the influence by holding it still to the patient's nose. The patient was entirely unconscious—no struggling—patient passive in the hands of the operator. After a lapse of fifty years you would hardly suppose that a man could remember every minute detail, but I have clearly in mind all the facts I have given you.

Your obedient servant,
(Signed) J. F. GROVES, M.D.

(V) CERTIFICATE FROM R. H. GOODMAN SHOWING LONG'S USE OF ETHER.

I certify that on January 1, 1842, I resided in Jefferson, Jackson Co., Georgia, and that about that time, myself, with several other young men, were in the habit of meeting at Doctor C. W. Long's shop, and other rooms in the village, and inhaling ether which he administered to us.

On January 20 of the same year I removed to Athens, where I introduced the inhalation of ether. I and several of my young associates frequently assembled ourselves together and took it for the excitement it produced. After that, I know it became very common to inhale ether in Athens, and that it was frequently taken in the college campus and on the street.

(Signed) R. H. GOODMAN,
August 4, 1849. Athens, Georgia.

(VI) CERTIFICATE CONCERNING FIRST OPERATION FROM E. S. RAWLS.

Georgia, Clarke Co.

I, Edmund S. Rawls, of Rome, Floyd Co., Ga., on oath depose and say that . . . on one occasion during the year (1842) I was present with James M. Venable in the office of Dr. C. W. Long in Jefferson, Jackson Co., Ga., and witnessed Dr. C. W. Long cut out a tumour from the side of neck of J. M. Venable while said Venable was fully under the effects of the vapour of s. ether inhaled from a towel, and without his exhibiting the least symptoms of suffering pain from the operation. J. M. Venables was so unconscious of the operation having been performed, that he would not believe the tumour was removed until it was shown him.

(Signed) E. S. RAWLS.

Sworn to and subscribed before me this 2nd November, 1853.

E. L. NEWTON, J.J.C.

(VII) Letter from Dr. Jackson.

The Boston Medical and Surgical Journal, Boston.

Thursday, April 11, 1861.

First Practical Use of Ether in Surgical Operations.

Messrs. Editors,—At the request of the Hon. Mr. Dawson, U.S. Senator from Georgia, on March 8, 1854, I called upon Dr. C. W. Long, of Athens, Georgia, while on my way to the Dahlonega gold mines, and examined Dr. Long's evidence, on which his claims to the first practical operations with ether in surgery were founded, and wrote, as requested, to Mr. Dawson, who was then in the U.S. Senate, all I learned on the subject. From the documents shown me by Dr. Long, it appears that he employed sulphuric ether as an anæsthetic agent:—

(1) March 30, 1842, when he extirpated a small glandular tumour from the neck of James M. Venable, a boy in Jefferson, Georgia, now dead.

(2) July 3, 1842, in the amputation of the toe of a negro boy belonging to Mrs. Hemphill, of Jackson, Ga.

(3) September 9, 1843, in extirpation of a tumour from the head of Mary Vincent, of Jackson, Ga.

(4) January 8, 1845, in the amputation of a finger of a negro boy belonging to Ralph Bailey, of Jackson, Ga.

Copies of the letters and depositions proving these operations with ether were all shown me by Dr. Long. . . .

I then called on Professors Joseph and John Le Conte, then of the University of Georgia, at Athens, and inquired if they knew Dr. Long, and what his character was for truth and veracity. They both assured me that they knew him well, and that no one who knew him in that town would doubt his word, and that he was an honorable man in all respects.

Subsequently, on revisiting Athens, Dr. Long showed me his folio journal, or account book, in which stand the following entries:—

James Venable.

March 30, 1842, ether and excising tumour	2.00 dollars
May 13, sul. ether	0.25 cents
June 6, excising tumour	2.00 dollars

On the upper half of the same page, several charges for ether sold to the teacher of the Jefferson Academy are recorded, which ether Dr. Long told me was used by the teacher in exhibiting its exhilarating effects, and he said the boys used it for the same purpose in the academy. I observed that all these records bore the appearance of old and original entries in the book.

On asking Dr. Long why he did not write to me or make known what he had done, he said, when he saw my dates he perceived that I made the discovery[1] before him, and he did not suppose that anything done after that

[1] Dr. Jackson's "discovery" merely amounts to his knowledge that ether was an exhilarant and narcotic; he never employed it as an anæsthetic. This knowledge was common knowledge, as has already been shown.

would be considered of much importance, and that he was awakened to the idea of asserting his claims to the first practical use of ether in operations by learning that such claims were set up by others for this merit, and consequently he wrote to the Georgia delegation at Washington, stating the facts which Senator Dawson had requested me to inquire into.

I have waited expecting Dr. Long to publish his statements and evidence in full, and therefore have not before published what I learned from him. He is a very modest, retiring man, and not disposed to bring his claims before any but a medical or scientific tribunal. . . .

Had he written to me in season I would have presented his claims to the Academy of Sciences of France, but he allowed his case to go by default, and the Academy knew no more of his claims to the practical use of ether in surgical operations than I did.

CHARLES T. JACKSON, M.D.[1]

Boston, *April* 3, 1861.

(VIII) LETTER FROM DR. DAVIS WRITTEN TWELVE YEARS AFTER A PATENT WAS GRANTED TO MORTON FOR HIS LETHEON, SHOWING THAT HE ATTEMPTED TO ENFORCE PATENT RIGHTS.

U.S. Marine Hospital, Chelsea, Massachusetts.

Dr. CRAWFORD W. LONG, Athens, Georgia. *April*, 1859.

Sir,—Hon. Judge Hyllier, Solicitor of Treasury Department, informed me about a year since, and recently repeated the same, that some years since you used sulphuric ether as an anæsthetic and had a record of the same. If it is not asking too much of you, I would be greatly obliged if at your earliest convenience you would forward me a statement of the facts.

I take the liberty to ask this of you because Mr. W. T. G. Morton, to whom in conjunction with Dr. C. T. Jackson a patent was granted in November, 1846, for using ether, has brought a suit against me as a Government Officer for an infringement of his patent.

Judge Hyllier was confident that you could furnish me with proof sufficient to satisfy a jury that you used it way before he or Jackson claimed to have made the discovery. I should have asked for these proofs through my attorney and had them properly witnessed, &c., but the Secretary of the Treasury having decided that I used the article on my own responsibility, and therefore the Government were not bound to defend me, I wish to save as much expense as possible.

Very respectfully,
(Signed) CHARLES A. DAVIS, M.D.,
Physician and Superintendent.

[1] The List of Operations as given by Dr. Jackson is not complete, as he has omitted the first operation on Venable, and a number of the later operations.

(IX) WILHITE'S LETTER CORRECTING DR. MARION SIMS'S MISSTATEMENT IN HIS PAPER ABOUT LONG AND THE FIRST ETHER EXPERIMENT.

Anderson, S.C.,
Dr. C. W. LONG. *June* 27, 1877.

Dear Doctor,—Yours of the 22nd inst. is at hand, and I have also just received a letter from Dr. J. M. Sims, which I will answer to-day. . . .

In my statement I did make a mistake in regard to my being present at the first or second operation, which mistake I will correct. But if you still prefer I will send a certificate. . . .

Let me know and I will give you any information or assistance in this great matter.

Yours truly, &c.,
(Signed) P. A. WILHITE.

(X) FROM DR. LONG'S FIRST STUDENT.

Cohutta, Ga.,
Mrs. FRANCES LONG TAYLOR. *December* 13, 1894.

Dear Madam,— . . . In 1844, soon after I attained my majority, I decided to adopt medicine as my profession, and began to think where and under whom I should begin the preparatory study. My father asked me to choose from among the number of physicians whom I knew the one I preferred to act as preceptor to me.

Knowing Dr. Long so well, and believing him to be a man of no ordinary ability, I at once fixed upon him as my choice.

I entered Dr. Long's office in May, 1844, as the first student ever under his care. As I progressed with my studies he saw fit to make known to me his discovery, by the use of which he could perform surgical operations without giving any pain to his patient. (Here follows a description of the first cases.)

Not satisfied, however, that there was not more to learn about this great discovery, he proposed that we test it further personally, which we did in his office, where with closed doors we administered it to each other to prove its perfect anæsthetic effect, and also to discover any bad effect to the subject etherized. Owing to the prejudice and ignorance of the populace, Dr. Long was prevented from using ether in as many cases as he might have.

Thus in the two years preceding my entering Dr. Long's office he had had only about six cases in which to try the anæsthetic effects of ether.

The first case that came under his care where its use was applicable after my going into his office was not till January 8, 1845, which was the case of a negro boy having two fingers to amputate, caused by neglected burn. I, as

the only student still with the doctor, he had me to accompany him to see the operation, and assist in the administration of the ether. The first finger was removed while under the influence of ether, the little fellow evincing no pain; the second without ether, the child suffered extremely. This was done to prove that insensibility to pain was due to the agent used.

Soon after this, in January, Mr. J. D. Long came into the office as a fellow-student. Later, toward spring, came P. A. Wilhite, and in August came Dr. Long's brother, H. R. J. Long. We four remained there at Dr. Long's office as students until the opening of the full term of the medical colleges. . . .

(Signed) J. F. GROVES, M.D.

Sworn and subscribed to before me, December 15, 1894.

WM. H. WILSON, N.P.

DISCUSSION.

Mr. RICHARD GILL remarked that the paper did not afford much opportunity for discussion, but members of the Section had all been extremely interested in the able way in which Dr. Dudley Buxton had put forward the claims of Dr. Long to fame. The mere fact that so many minds—four, he believed—should be evolving the same idea within the same decade, recalled a similar occurrence antecedent to the discovery of sulphuric ether as an anæsthetic agent—namely, the discovery of oxygen. Only a short interval, some five months, separated the discovery of it by our countryman Priestley, who was a native of Birmingham, and that of Scheele, the Dutchman. On investigation it would be found that in other branches of science discoveries were made which occupied several minds about the same time, though it often happened only to one to place it before the world in due shape. He believed the recent discovery of wireless telegraphy afforded another instance of the same thing, for only by a mere chance Sir Oliver Lodge missed the association of his name with that great event.

The PRESIDENT (Dr. W. J. McCardie) desired to add his personal appreciation to that expressed by Mr. Gill to Dr. Dudley Buxton for his paper. The author had bestowed a great deal of time and trouble on it, and had treated the Section to one or two most interesting pages from the history of medicine. The paper would add to Dr. Dudley Buxton's already great literary reputation, and would redound to the credit of that Section of the Royal Society of Medicine. He only wished that more papers on the history of medicine were contributed, and he would like to see established a University Chair of the History of Medicine. He believed a similar professorship existed in more than one German university.

Proposal to make Methylated Chloroform Official.

A LETTER from Dr. Nestor Tirard (Medical Editor of the "British Pharmacopœia") was laid before the Section. Dr. Tirard stated that as some of the London Hospitals used methylated chloroform he would be glad to have an expression of opinion from the Society as to the desirability of affording official recognition to this substance as well as to the present official preparation of chloroform made from ethylic alcohol.

DISCUSSION.

The PRESIDENT (Dr. W. J. McCardie) said that besides methylated chloroform, chloroform prepared from acetone was also very largely used in this country; in Germany he believed that a form prepared from chloral hydrate was also widely employed. He invited the Section to express its opinion as to the advisability of making chloroform from methyl alcohol official.

Dr. DUDLEY BUXTON said that chloroform made from methylated spirit had been in use in University College Hospital for a long time. He regarded acetone chloroform as being good, and comparative trials of the two varieties had failed to show any marked difference as far as danger and complication were concerned. He understood Dr. Tirard's letter to ask for an opinion as to importance of the methylated chloroform—i.e., the frequency of its use. It would be needless, he submitted, to enter upon a discussion as to the relative safety of one or another variety. It was undoubtedly a fact that both the chloroform derived from methylated alcohol and from acetone were widely used and so should be admitted in the British Pharmacopœia as official drugs.

Mrs. DICKINSON BERRY said that if methylated chloroform was to be official that made from acetone should also be official. Acetone chloroform had been used exclusively at the Royal Free Hospital during the last year. It was substituted for methyl chloroform because several complaints had been made about the latter. The acetone chloroform had been found satisfactory, and most of those who had used both at the hospital were of the opinion that the acetone was both safer and more reliable.

Dr. ORR said that at Westminster Hospital Flockhart's chloroform was now used. It was reverted to after a trial of methyl chloroform because the form of this which had been supplied to the hospital was inferior, being too weak,

so that the patients could not be readily got under. He could not distinguish between good methyl chloroform and the ordinary chloroform. He agreed with Mrs. Dickinson Berry regarding the value of acetone chloroform.

Dr. BARTON said he had had experience of three kinds of chloroform—methyl, ethyl and acetone. For choice he used the methylated form. The ethyl chloroform seemed to deteriorate more readily than the others. He had met samples of chloroform prepared from ethylic alcohol which he had to reject owing to the peculiar smell, and the effects on the patient had not seemed very good. He had tried acetone chloroform at the Throat Hospital, but his experience was that it produced somewhat uneven results. Sometimes an enormous amount was required to induce anæsthesia, and then suddenly the patient would appear to have had too much.

Mr. RICHARD GILL said his experience of ethyl and methyl chloroforms corresponded with that of Dr. Dudley Buxton—viz., that if both were pure very little distinction could be drawn between them from the practical point of view, and they seemed to produce identical results. Some thought they could distinguish them by a slight difference in odour, but his sense of smell was not sufficiently acute to detect that difference.

The PRESIDENT said that in the General Hospital at Birmingham acetone chloroform was chiefly employed. In private practice he used methyl or acetone chloroform indifferently, and he had not been able to detect a difference between these and the ethyl chloroform. So long as the end-product was pure he did not think it mattered much what it was derived from, but it must answer the requirements of the Pharmacopœia. Dr. Tirard asked for a definite expression of opinion as to the advisability of rendering methylated chloroform official. He thought the meeting might very well adopt Dr. Dickinson Berry's suggestion and add an opinion that acetone chloroform be also made official because it seemed to have been employed a good deal by members of the Section, and was probably used as extensively in this country as any other kind of chloroform.

Dr. H. J. SCHARLIEB said he had used chloroform prepared from each of the substances already mentioned, including that made in Germany from chloral; he had not seen any differences in the results, whatever the kind used.

Mr. APPERLY said that for animals he had used chloroform prepared from all three substances. Taking a series of animals and comparing the amounts injected he had found that although a small dose of the chloroform prepared from ethyl alcohol might prove fatal, it took a larger dose of that prepared from acetone to kill the animal. It was important to state that the substances were given by injection, not inhalation.

Mr. BOYLE remarked that Dr. Edkins, at St. Bartholomew's Hospital, told him that since adopting the use of ethyl chloroform for the animal experiments there had been far fewer deaths than when methylated chloroform was employed.

The PRESIDENT now put to the meeting the question whether methylated chloroform was sufficiently largely used to justify its inclusion in the Pharmacopœia.

It was unanimously agreed that methylated chloroform should be made official.

The PRESIDENT asked if the Section should at the same time recommend that acetone chloroform should also be made official.

This was carried by five to four.

Section of Anæsthetics.

February 2, 1912.

Dr. W. J. McCardie, President of the Section, in the Chair.

An Experimental Study of the After-effects of Chloroform.

By A. L. M. Muskens, M.D.

General anæsthetics were first employed by Jackson and Morton (1846). Only a few years afterwards the use of ether and chloroform became so common that scarcely a review of clinical surgery of that time fails to record the experiences of surgeons with these anæsthetics. The enthusiasm of the medical profession for general anæsthesia increased, the more evident it became that the patients' objections to surgical treatment were now done away with to a great extent, and in this way the field of surgery could be greatly extended.

Let it be said, however, to the great credit of the then living professional men, that they did not adopt the new method without a certain amount of hesitation. Both anæsthetics just mentioned are poisons, and not only would many soon come to the sad experience that a patient submitting to an operation under most favourable conditions died under chloroform, but also after the operation there were inexplicable cases of death which were attributed to the anæsthetic. As early as 1849 Giralde cites two cases in the *Gazette des hôpitaux*, in both of which he assumed that death followed from poisoning by chloroform and ether. Langenbeck likewise, in 1850, quotes a case in which, from a post-mortem examination, he attributed the fatal issue to chloroform; he therefore adopted the term "chronic chloroform poisoning," to distinguish it from the direct fatal effects of chloroform.

From this time forward the subject is dealt with more extensively. Sabarth mentions already 119 cases in 1866. But these statistics were

of little value, for, as Sabarth himself admits, only forty-eight cases could stand the test of criticism. As, however, amongst this number there were cases in which no post-mortem examination, or only a superficial one, took place, and, moreover, since a microscopical examination was made in not more than nine cases, it stands to reason that this cannot be accepted as evidence for the fatal after-effects of chloroform, much less can there be question of a clinical and anatomico-pathological concept.

For a quarter of a century after this little or no attention was given to the points raised in connexion with the fatal after-effects of chloroform. The state of the question in 1890 is typically illustrated by the two following facts: In that same year a book by the well-known French physiologist Dastre, "Les Anesthétiques," was published; his otherwise complete treatment of the application and physiology of anæsthetics makes no reference whatever to the fatal after-effects of chloroform. In the course of the same year (1890) a Berlin professor wrote, with every appearance of truth: "One glance at the current literature of the subject suffices to show conclusively that in these cases (chronic chloroform poisoning) the only cause of death was either a severe operation followed by high pyæmic fever or collapse."

The credit of arousing fresh interest in a question of such deep importance in medicine falls to Bastianelli and Fraenkel. Their treatises on the subject were followed up by a whole series of investigations, which for the most part bear evidence of careful study. Many even extended their researches to animals for a solution of the many problems which the after-effects of chloroform presented.

No convincing answer, however, can be found in the very extensive medical literature to the question, what is the real cause of death in cases of chronic chloroform poisoning? Opinion varies very much amongst those who have tried to solve the problem, and this diversity of opinion must be attributed to the very striking want of uniformity of research, both as regards method and also material. Experiments in which rabbits were twice chloroformed for a space of two hours each time, without any fatal result (Heintz), bear no comparison with experiments in which eight other rabbits after half an hour's chloroform (Offergeld) died, all succumbing on the same day. Much less can we compare the results obtained by Nothnagel, who injected chloroform, with the observations of Stommel, who placed the animals in an atmosphere of chloroform, without the anæsthetic producing narcosis. Nor could Auburtin come to any definite result in his experiments: "in fact,

hepatic lesions consequent on inhalation, even repeated, are fairly difficult to produce in the rabbit."

Nevertheless there seems to be a general consensus of opinion that the liver is the one organ which shows the most constant changes, although Ostertag and Offergeld, for instance, held the change to be infiltration of fat, and not fatty degeneration. Sally Jacobi noticed no change in the kidneys, while Offergeld, on the other hand, attributed all to degeneration of the kidney, and Heintz and Strassmann and Ostertag traced the cause of death to the heart. Wherever other organs than the liver, kidneys, and heart were subjected to examination (Ostertag and B. Müller) it became evident that they were not the seat of pathological processes, which might be looked upon as causing severe illness in animals.

As anatomical and pathological investigation of the results of these experiments gave scanty satisfaction, I wanted to see, by administering chloroform to animals under precisely the same circumstances as in our hospitals, how far I was justified in locating the cause of post-narcotic illness in one of the above-named organs. My first idea was to adopt, as nearly as possible, all the precautions usually taken in preparing patients for the anæsthetic. The animals were left without food for about twelve hours before administration. They were then strapped down and the anæsthetic given just as in the hospital. Administration was by means of drops, and 30 c.c. anæsthetic were given on an average for two hours' narcosis, 15 c.c. for one hour. The almost fixed quantity which I administered with success to the rabbits would prove that to determine the dose when applying drops does not appear impossible. All the animals in the first stage of narcosis held their breath on smelling the chloroform, and this period of narcosis was in consequence usually very protracted; on an average it lasted ten minutes before the reflex motion of the cornea disappeared. I tried, as far as possible, to keep the animal in the one condition, in which the corneal reflex disappeared and returned alternately, and the animals showed absolutely no reaction on being severely pinched. If, however, the corneal reflex disappeared for a time, and the pupil dilated, breathing became superficial and death was imminent. In one case only could we revive the animal by artificial breathing. If an examination was made immediately after breathing had ceased the heart was always found to be still beating.

The tongue frequently presented, in experiments carried out on animals, the same difficulties in breathing which are met with in

dealing with human cases. I have often prevented serious cyanosis by drawing the jaw forward, or by extending the tongue. Finally, the excessive secretion of saliva at the end of narcosis was often a cause for anxiety; it was then found necessary to constantly cleanse both mouth and throat (with a small ball of cotton-wool).

Of the twenty-five animals which I chloroformed, eight died of the after-effects of the anæsthetic. Eight were killed at different periods to study the various processes of change which took place. Of the ten animals that were kept under chloroform for an hour, two died; one appeared to suffer from myocarditis.

I confined my anatomico-pathological examination chiefly to the liver, kidneys and myocardium. Portions of these organs were removed; some were placed in a 10 per cent. solution of formalin, some in Fleming's fluid. The latter were left in the fluid for three or four days, then washed in water, and finally put in spirit. They were then enclosed in the usual way in celloidin. The preparations of formalin were coloured with hæmatoxylin-eosin, the rest with saffranin. To give a clear idea of the pathological changes after the administration of chloroform to rabbits we shall consider first the eight animals which died after the narcosis. The following table gives a general view of the alterations observed in the organs:—

TABLE A.

No.	Number of hours of narcosis	Number of hours till death	Liver	Kidneys	Heart
1	1	3	No change	—	Chronic myocarditis
2	1½	22	Central degeneration	Degeneration; no necrosis	Marked appearance of the fibrillæ
3	1	30	Central necrosis	Degeneration; no necrosis	Marked appearance of the fibrillæ
4	2	36	,, ,,	Little change	Marked appearance of the fibrillæ
5	1½	38	,, ,,	No change	Marked appearance of the fibrillæ
6	2	42	,, ,,	Almost no change	Marked appearance of the fibrillæ
7	1½	45	,, ,,	Slight degeneration	Almost no change
8	2	100	,, ,,	Degeneration; no necrosis	Beginning of fragmentation

In the last six specimens of this table a microscopical examination of the liver gave almost uniform results. Proceeding from the periphery

to the centre three zones, clearly distinct from each other, were observed in the acini. The parenchyma of the most peripheral zone was almost normal; the cells retained their normal shape, and contained only a few droplets of fat. The nuclei were often doubled and showed no signs of degeneration. The droplets of fat gradually increased towards the centre, finally filling the whole cell-body, in the middle of the central zone. The nuclei, as far as they could be seen in the large and small droplets of fat, were pale; the form, too, of the cell was changed, becoming spherical. Here signs of degeneration were observed for the first time, and they became more conspicuous towards the centre, until they reached the state of necrobiosis in the central zone. There we found portions of plasma without nuclei, coarse and granulated, disintegrated droplets of fat, débris of nuclei with some leucocytes and erythrocytes in hopeless confusion. As for the fat itself, one would say the cells had given out their supply, for the droplets were floating in the mass of detritus.

The various preparations taken from the parenchyma of the kidney and of the myocardium showed marked differences, in contrast with the uniformity of the liver preparations. Changes were found most constantly in the tubuli contorti of the kidneys; the nuclei were, with few exceptions, paler than in ordinary circumstances, and the cells contained some droplets of fat at their base. Two specimens had granulated cylinders in their tubuli. The tubuli recti, beyond a little fat in some specimens, showed no signs of degeneration. There was no apparent change in the glomeruli, except in one case; the epithelium of Bowman's capsule in this case was here and there separated, and the cells were somewhat swollen. Changes in the heart were even less conspicuous than in the kidneys. One case only presented dissociation of the myocardium; in general, degeneration was confined to a more marked appearance of the longitudinal striæ when compared with the transverse. The lungs of the three specimens were then examined, but only one case showed any alteration, and that was the eighth and last animal, which died a hundred hours after narcosis. The degeneration of the kidneys and heart was also much more conspicuous in this case. No alteration was observed in the transversely striated muscles, spleen, and spinal marrow. I should add, however, that in the case of the animal which died three hours after the administration of chloroform, chronic myocarditis was evidently the chief cause of death, for before chloroform could have had any after-effect upon the organs the heart

had probably ceased to function under the direct influence of the poison.

In order that the process of degeneration in the liver might be taken at different stages after narcosis eight rabbits were killed.

TABLE B.

No.	Number of hours of narcosis	Number of hours till death	Liver	Brain
1	2	—	Fat in the endothelia	No change
2	2	3	Fat in the endothelia	—
3	2	12	Fat in the endothelia and parenchyma; central degeneration	A little fat in the ganglia
4	2	12	Fat in the endothelia and parenchyma; central degeneration	—
5	2	24	Central degeneration	—
6	2	48	Central degeneration with necrosis	Fat in the ganglia cells
7	1½	5 days	Advanced degeneration with central necrosis	—
8	1	5 days	Little degeneration	—

We might add to the list the fourth rabbit mentioned in Table A. This animal died twenty-four hours after narcosis, just when a second administration of chloroform was being made.

The process of degeneration in the liver could be clearly followed in the specimens of the table given above. The livers of the first two rabbits showed no change in the parenchyma cells. The endothelium cells, however, contained droplets of fat of unequal size. Of the specimens examined after twelve hours, the liver cells in one case presented mostly vacuoles, and the nuclei near the vena centralis had absorbed less colouring material (here a Fleming preparation was not at our disposal). In the other case all cells, even the endothelial cells, contained droplets of fat: here, too, the nuclei near the vena centralis were paler than elsewhere.

The above-mentioned zones were very clearly seen in the animals examined after twenty-four and forty-eight hours. We can, then, easily suppose that this marks the stage of degeneration in the liver reached by those rabbits which died after narcosis. The degeneration, however, was not so far advanced in the specimens of the second table (B) as in those of the first table (A). Only the seventh

specimen in the second table showed symptoms which agreed perfectly with those of animals in the first series.

The experiments, however, furnished very few characteristic data from a clinical standpoint. After the administration the animals remained stupefied in their boxes and ate little. The urine presented an albuminous reaction in the seven cases which were examined. In three cases the reaction was marked, in four very slight. Cylinders were met with in only two cases; but the urine was never dark in colour, nor was there externally visible icterus (jaundice). In one particular instance the animal passed through a state of delirium, dying one hundred hours after narcosis. The liver, kidneys and heart were in a far-advanced stage of degeneration, but necrosis was observed only in the liver. But what was strongly in evidence in all these experiments was the parenchymatous change of the liver, so much so that, as far as my specimens were concerned, there could be no doubting which organ was mostly affected and caused death.

The after-effects of chloroform have been so far traced chiefly either to the degeneration of the kidneys or to a toxæmic condition following the retention of acids. Few solid arguments can be advanced, from a clinical standpoint, in support of degeneration of the kidneys. It is true statistics are quoted in which the percentage of post-narcotic albuminuria rises as high as 30, 40, or even 100, but no one has ever seriously inferred from these figures that the effect of chloroform on a healthy kidney is so destructive as to really cause death.

Experimental research gives different results. Offergeld confined his observations almost exclusively to the kidneys, and invariably found after narcosis fatty degeneration of the kidney. The results of Sally Jacobi are in flat contradiction to this opinion, and his researches could never bear out the theory of advanced degeneration. We could find in one case only a state of degeneration worth mentioning, which, however, could not be compared in any way with the destruction seen in the liver.

Then take toxæmia. Briefly, this is acid intoxication, which English and American authors prefer to accept as the explanation of post-narcotic symptoms. By acid intoxication is understood that form of acidosis in which clinical symptoms of poisoning occur. Coma diabeticum is now generally considered as an acid poisoning; in fact, the alkalescence of the blood strongly diminishes. Butylactic acid is strongly in evidence. Acidosis is a common symptom in diabetes. Patients can continue for months in a comparatively good state of

health with a fairly high percentage of acid in the blood. The acids (butylactic and diacetic acids) which are found in the blood are at present considered (Löwy, Magnus-Levy) as intermediary products of fat disintegration arising from incomplete combustion of the fat. This acidosis appears in various circumstances, such as fasting, abstaining from carbohydrates, in different states of fever, in poisoning by phosphorus, in eclampsia after the administration of anæsthetics. In coma diabeticum alone acidosis is generally assumed to have developed into acid intoxication. The quantity of acetone in the urine (butylactic acid, diacetic acid, and acetone) increases rapidly, and the amount is as much as 40 gr., even 140 gr., in the twenty-four hours.

There can be no doubt that acetonuria is present in conjunction with narcosis. The statistics of van Becker, Waldvogel, Telford and Falconer have established that point. Becker found acetonuria in 167 out of 251 cases examined, while Telford and Falconer found it 105 times in 118 instances. Both agree that the nature of the anæsthetic and its duration exercise no influence on the grade of acetonuria. The amount of acetone after narcosis varies from 20 to 60 mg. per 100 c.c. of urine (Beesley); but this quantity bears no comparison with the volume found in coma diabeticum. Bainbridge, in his general considerations on the pathology of acid poisoning, says in the chapter devoted to post-anæsthetic acetonuria: "I have found in other conditions, in which no symptoms of acid intoxication were present, an amount of acetone equal to that observed in cases of post-anæsthetic acetonuria." He, too, considers acid intoxication improbable. In this connexion it is well to quote the words of Magnus-Levy. After mentioning the great percentage of acetone in the urine in cases of coma diabeticum in which the daily percentage was observed to rise to 40—even 140—gr., he says: "What may we not infer from these cyphers when some authorities already draw such far-reaching conclusions from an increase or a decrease of the acetone limited to a few centigrammes."

Bainbridge rightly observes that chloroform poisoning in animals presents many similarities to phosphorus poisoning. In fact, both present, from a clinical and anatomico-pathological standpoint, sufficient points of resemblance to consider them under the same head. Seeing that Löwy, in Van Noorden's Handbook, concludes his chapter on chloroform with the observation that the chemical symptoms of chloroform poisoning are fully analogous to those poisons which—as prussic acid, phosphorus, &c.—destroy the oxidizing activities even of the

cells, then we might safely argue that acetonuria—or, better, acidosis—produces no greater result than in cases of poisoning through phosphorus, which either from its specific effect on the liver, or otherwise, causes a disturbance in the whole process of anabolism, but particularly in the oxidation of fats. Following the researches of Arnheim, glycogen disappears from the liver under the influence of chloroform, just as in poisoning by phosphorus, and thus the condition for the appearance of acetone—viz., the removal of carbohydrates—is brought about.

How important is the part which the liver takes in the formation of acetone is proved by the experiments of Van Embden and Kalberlah. They found that the liver was the only organ which formed acetone in the blood, whenever they allowed blood to flow through the organs immediately after death.

These considerations on toxæmia naturally bring us back to the liver, the one organ which, in almost all cases known in the literature of the subject, shows destructive changes: some even go so far as to speak of acute post-anæsthetic yellow atrophy of the liver (Bandler, Horsfall and Campbell). And experiments strengthen this conviction: not only do histological researches lay bare material destruction of the parenchyma of the liver, but chemical analysis of the urine and investigation of the changes which took place in metabolism, in several animals submitted to chloroform, led Nöel Paton to the conclusion that this is exclusively due to a diseased condition of the parenchyma of the liver. This process is usually described as fatty degeneration of the parenchyma, with necrobiosis extending peripherally. And these changes are not confined to animal specimens, for patients dying from the effects of chloroform present the same symptoms. The course of the disease in these particular cases can be traced from the animal specimens examined at different periods after narcosis. Where animals were examined immediately after administration the parenchyma of the liver showed no change; twelve hours later all the cells were found to contain a greater percentage of fat; after twenty-four hours the fat was no longer equally distributed—the zone near the vena centralis was rich in fat, while the percentage of fat decreased gradually towards the periphery; the central zone showed evident signs of necrobiosis, pyknosis, karyorrhexis, chromatolysis, and karyolysis. Later, the central zone is found to be entirely destroyed. The cells are disintegrated, the droplets of fat disappear. The peripheral cells, beyond a moderate supply of fat, show no signs of deterioration. The middle zone is noticeable for its percentage of fat, a percentage which increases from

the extremities towards the centre, while the process of degeneration proceeds in the opposite direction.

What relation is there between the appearance of fat in the liver cells and the process of degeneration which these cells undergo? Let us first see how fat originates. Rosenfeld's experiments show that the increase of fat in the liver after the administration of chloroform is supplied from the adipose tissue of the organism; and this becomes all the more evident from the fact that, as Reicher proves, lipæmia follows upon narcosis. And my own experiments fully bore out this point.

Under what circumstances does the adipose tissue lose its supply of fat? To answer this question it is necessary to see what becomes of the chloroform during and after its administration. Chloroform, as we know, is absorbed in the lungs by the blood and conveyed to the organs? The red corpuscles, on account of their greater percentage of lecithin, chiefly serve to transmit the chloroform. The proportion of chloroform in the serum to that contained in the fibrin is in the ratio of 5 to 1. The organs absorb chloroform in proportion to the quantity of fat or fatty substances which they contain. Thus Nicloux and Tissot found the greatest percentage in the fat of the abdomen, and then in the brain and spinal marrow. The fatty tissues of the abdomen, which are rich in blood, often contain four and five times as much as the nervous tissues; twenty times as much as the muscles. After narcosis the tissues gradually work off the narcotic; for venous blood contains during the first hours after narcosis a greater quantity of chloroform than arterial. I myself registered, after three hours, the following percentages in the different organs:—

Liver tissue	6 to 7 mg.	chloroform	for	every	100 gr.	of tissue
Brain	14	,,	,,	,,	,,	
Fatty tissue of abdomen ...	128	,,	,,	,,	,,	
Pararenal fat	200	,,	,,	,,	,,	

These figures prove that the percentage of chloroform is not released under the same conditions from the fatty tissues as from the other tissues. When chloroform is absorbed in the organs by lipoid bodies such as lecithin the red corpuscles easily remove it. It is evident, then, that the chances are less favourable for the fatty tissues. Nevertheless we find that, twelve or twenty-four hours later, chloroform, except for a few milligrammes, has disappeared from the fat of the abdomen. Since at the same time the fat adds substance to the blood, there must be a close connexion between these two facts, and the chloroform which

has disappeared will reappear in the fat drops absorbed by the blood.

At this juncture we also find that the blood fibrin is entirely free from chloroform, while the strongly opalescent serum contains a clearly visible quantity. As we have already mentioned, the fat distributed by the fatty tissues reappears in the parenchyma of the liver. It is clear that this fat, laden with chloroform, is no great gain to the liver cells, and we think that this affords an explanation of the fact that the liver cells are in a stage of degeneration proportionate to the amount of fat they contain. In one word, we are inclined to think that the after-effects of chloroform should be considered as an acute affection of the liver consequent upon its absorbing the chloroform-laden fatty substances from the blood.

One final observation, on prophylaxis and therapeutics. The propositions advanced so far rest chiefly on theoretical grounds. Those who sought for the cause of these ill-effects in the pathological condition of the kidneys naturally applied the clinical remedies in common use in diseases of those organs. If, on the other hand, it was assumed, with Waldvogel, that the cause was acid poisoning, as a preventive measure fats were excluded from the diet, and the alkalescence of the blood artificially increased by giving alkalis. Beddard, Wallace and Gillespie further recommended carbohydrates to prevent acetonuria. But it is difficult to say how far all these prescriptions guard against this far from imaginary danger of subsequent chloroform poisoning.

If our theory of the disease, its nature, and development proves to be the true explanation of its symptoms, there is little hope of applying, with any success, either prophylaxis or therapeutics. A diet into which no fat enters, and avoidance of all purgatives, of the nature of oily substances, may help as precautionary measures. As for the treatment itself; if cholagogues not only help to carry off the bile, but cause an abundant discharge of fat from the liver, their application might remove from that organ some of the injurious element. But will this method of treatment lessen the danger of chloroform? Only a deeper knowledge of physiology, pharmacology and pharmacodynamics can give a decisive answer.

The Significance of Acetonuria in Childhood.

By R. S. Frew, M.D.

At first sight it would appear as if the subject of this paper had but little bearing on the question of anæsthetics. One has been struck, however, both in conversation and in reading the literature, by the importance attached to this symptom, occurring after the administration of an anæsthetic, as indicating post-anæsthetic poisoning, and one feels that much of our lack of knowledge of that subject is due to this fallacious interpretation of the real meaning of the acetonuria; for whilst acetonuria is certainly a symptom of post-anæsthetic poisoning, yet it were as correct to diagnose this condition because acetone appeared in the urine during the few days following an anæsthetic as it would be, because of the vomiting that not infrequently occurs during that same period, to diagnose acute intestinal obstruction, or to regard the headache sometimes present at that time as indicative of meningitis. In the vast majority, I will not say in all, but certainly in the vast majority of cases, the acetonuria, whilst following the anæsthetic, is quite independent of it, its occurrence being due, I believe, to a totally different cause, and to that I should now like to direct your attention.

What follows is based upon observations which I have made during several years past, chiefly in the medical wards of the Hospital for Sick Children, Great Ormond Street, and thus refers to children only. During that time I examined the urines of 662 children; the cases were in no way selected, where a specimen was obtainable it was examined and a record kept. In most of the cases specimens were examined for at least three days after admission, and in many of them for a much longer time. In all cases *Rothera's test* was employed, viz., 5 c.c. of urine taken, an equal quantity of a saturated solution of ammonium sulphate added, then 2 c.c. of liq. ammon. fort. and a few drops of a freshly prepared 5 per cent. solution of sodium nitro-prusside. If acetone were present a slowly developing permanganate colour appeared, which was quite distinctive.

This test has no fallacy, unless one just pointed out, that in the case of a child who has had rhubarb or senna, a pink colour appears on the addition of the ammonia (due to the chrysophanic acid which is excreted), but this appears before the addition of the nitro-prusside solution.

This test is better than Lieben's, where one may have difficulty in distinguishing the faint smell of iodoform from the iodine; or Legal's, where it is necessary to appreciate the difference in the shade of colour on the addition of acetic acid.

The examination of the urine from these 662 children, varying in age from a few days to 12 years, showed acetonuria in 408—i.e., in 61·6 per cent. Further investigation of those 408 cases brought out certain interesting facts, which I shall now mention, and then discuss more fully.

(1) That in the great majority of the cases a specimen obtained during the first twelve hours following admission rarely showed the presence of acetone; that it usually first appeared about that time, attained its maximum about thirty-six hours after admission, and then gradually diminished, all trace being gone by the fourth day, usually.

(2) That the disease from which the child was suffering appeared to have little, if anything, to do with its causation.

(3) That its incidence was greater the younger the child, the percentage affected showing a steady decline from the youngest to the oldest child.

(4) That on the administration of dextrose it rapidly disappeared, and was completely gone within twelve hours.

Let us look at those facts a little more closely.

(1) As to the Time of its Occurrence.

As I have pointed out, the acetone did not usually appear before twelve hours after admission, and did not usually persist after three days, which indicated that we were dealing with some factor coming into operation after admission to hospital, a factor which ceased to operate after three days. In this connexion also I would add that during the past month or so I have been making similar observations on children out-patients, and though I have not yet had sufficient cases to give percentages, I may say that one finds acetone *extremely* rarely, and then usually there is an obvious cause.

(2) The Effect of Disease.

Judging from the literature on the subject, one would have looked to this part of the investigation with confidence to find the solution of the problem, and to have separated its causes into more or less

"water-tight compartments," e.g., diseases in which gastro-intestinal symptoms are prominent—pneumonia, tuberculous meningitis, infantile diarrhœa, &c.; in cases of so-called cyclical vomiting; in conditions in which fever is present; or the "toxic group," including diabetic coma, salicylic acid poisoning, post-anæsthetic poisoning, &c.; but, indeed, no such grouping could be made.

As far as possible I have grouped the diseases from which the children were suffering under the various systems most affected, and the result showed:—

Diseases of the nervous system	65 per cent.
,, ,, urinary ,, ...	63 ,,
,, ,, digestive ,, ...	63 ,,
,, ,, respiratory ,, ...	62 ,,
,, ,, circulatory ,, ...	61 ,,

The remarkable uniformity in the percentages for all classes of disease becomes at once evident. Moreover, in certain cases admitted where no disease could be detected, and in some simple surgical cases I examined, the percentage remained about 60.

Hence the conclusion was forced upon one that the disease from which the child was suffering was not the cause of the acetonuria.

(3) Age-incidence.

An analysis of the percentages for various ages showed, as I have already indicated, a nearly uniform decline from the youngest to the oldest. In the case of those infants under 1 year who were changed from breast to artificial feeding on admission, the percentage developing acetonuria was 100—

Between 3 and 4 years of age	84 per cent.
,, 4 ,, 5 ,, ...	72 ,,
,, 5 ,, 6 ,, ...	65 ,,
,, 6 ,, 7 ,, ...	73 ,,
,, 7 ,, 8 ,, ...	58 ,,
,, 8 ,, 9 ,, ...	63 ,,
,, 9 ,, 10 ,, ...	49 ,,
,, 10 ,, 11 ,, ...	50 ,,
,, 11 ,, 12 ,, ...	46 ,,

This gives an almost uniform drop from 100 to 46. (It will be noticed that I have omitted from this list the cases under 1 year, that were previously artificially fed, and also those between 1 and 2 and 2 and 3 years, the percentages here showing an apparent relative decline; but

this I shall refer to later, and explain why I believe it to be apparent and not real.)

The causation of this acetonuria, then, would appear to be some factor which is not present on admission to hospital, which operates during the first three days following admission, which is not related to disease, and which affects the child less as it grows older. This is the factor which we have to discover.

The fourth fact to which I have already referred concerned—

(4) The Effect of Dextrose Feeding.

I have already said that if at any stage of the acetonuria dextrose was supplied to the child the acetone disappeared within twelve hours. Before discussing this further I must digress for a little in order to render what follows a little more clear.

It is necessary to recollect that whatever excites the acetonuria it is primarily due to "carbohydrate starvation" with, as a result, excessive fat metabolism, and a corresponding excess of the products of fat metabolism, among which are the acetone bodies, including acetone itself, which are excreted in the urine. Such "carbohydrate starvation" may be brought about by: (*a*) deficiency of carbohydrates in the diet; (*b*) inability to digest the carbohydrates; (*c*) inability to absorb the carbohydrates; (*d*) inability to assimilate them. In one of those processes, then, we must seek the factor we are looking for.

(*a*) *Deficiency of Carbohydrates in the Diet.*—Such gross "carbohydrate starvation" will *invariably* produce acetonuria, and this has frequently been demonstrated. I will give two instances from my own observations. Two cases, a boy, aged 6½, and a girl, aged 5, were put on diabetic diet for twenty-four hours. In each case the urine was normal before this was done. The former gave acetone in the specimen passed nine hours after the commencement of this diet, the latter seventeen hours after. The acetonuria in each case increased until about fifty hours after this diet had been started, and then gradually diminished. That this was not the factor causing it in the 408 children, however, is obvious when it is pointed out that the diet for at least the first twenty-four hours after admission to hospital was almost entirely carbohydrate, consisting of milk only, or milk, bread, rice, pudding, &c.

We must then pass on to the next group.

(*b*) *Inability to digest Carbohydrates.*—In whatever form the carbohydrate is taken, it must be converted into a mono-saccharid before

absorption can take place. In an ordinary carbohydrate diet we find di-saccharids, represented by milk-sugar and cane-sugar, and poly-saccharids, represented by starch. In the small intestine the di-saccharids, milk-sugar and cane-sugar, undergo inversion into the mono-saccharids, dextrose and galactose, and dextrose and lævulose respectively, and these can be absorbed as such. Starch requires a more lengthy process, first undergoing digestion, with the production of maltose and dextrin, then these are inverted to dextrose, and as such absorbed. It follows, of course, that if dextrose (or glucose) is given directly it is at once ready for absorption.

To return to the original subject: I have stated that when the cases I was observing were given dextrose the acetone invariably disappeared within twelve hours, showing clearly that in those cases there is no inability either to absorb or to assimilate the carbohydrate. Therefore, as there is no deficiency in the carbohydrates in the diet, and no inability to absorb them, the fault must lie in the intermediate process—i.e., there is inability to digest the carbohydrates. But, as there is no acetonuria after three days there is no permanent loss of power, but merely a temporary inability on the part of the digestive processes to prepare the carbohydrate for absorption. Recollecting that this "temporary inability" does not give symptoms until about twelve hours after admission, and that this is about the time required to produce acetonuria in a "carbohydrate-free" diet, we naturally look to the diet for the cause. But the diet, as we have seen, is very rich in carbohydrates, and the only factor left common to all the cases is the change in diet from that to which they have been accustomed previous to admission. It therefore becomes evident that when the diet is changed the digestive apparatus requires some time to accommodate itself to this change, and it is during this period that there is carbohydrate starvation, with, as a result, acetonuria.

This, then, is the significance of acetonuria in childhood, that at that time of life the digestive stability is so slight that it is upset by merely changing the diet. A better example of this could not be found than that observed in the case of babies under 1 year of age. Of eleven such cases, where the diet was changed from breast to artificial feeding after admission, every one, or 100 per cent., developed acetonuria. Of thirty-eight babies who had been artificially fed before admission, and milk being practically the only diet, there would be but slight change afterwards, only 6 or 15 per cent. showed acetone in the urine. Moreover, this explains the relatively small percentages between 1 and 2 (53 per

cent.) and between 2 and 3 (64 per cent.), for milk and carbohydrates would form the chief constituent of the diet at that age, and the change would not be so great as it would be in children having a more complex diet. To supplement this clinical observation let me add what Professor Pawlow [5] has to say as a result of his experiments upon animals: "Digestive disturbances are often instituted if a change be suddenly made from one dietetic régime to another, especially from a sparse to a rich diet. These disturbances are expressions of the temporary insufficiency of the digestive glands to meet the new demands made upon them."

Let us now turn to the practical application of this observation. Change of diet is the first step in preparing the child for anæsthesia; moreover, the great majority of children admitted to hospital for operation receive their anæsthetic within three days of admission. But, and as if this were not sufficient, tradition steps in and says, "No solid food or milk for at least four hours before operation, and for a varying period afterwards," and any one of those, and certainly a combination of them, renders it certain that the child receives its anæsthetic during the period that its metabolism is deranged; one naturally expects to find the percentage developing acetonuria under such circumstances to be even higher than I have found from mere change of diet, and such is the case.

Becker [1] examined "between 1,400 and 1,500 specimens of urine after anæsthesia in children and adults, and found acetonuria in 64 per cent. of the cases," adding that children were more susceptible than "adults."

Nachod [4], working with children from 1 to 15 years of age, found acetonuria after anæsthesia seldom lasting for more than three days in thirty out of fifty-seven cases—i.e., 53 per cent.

Beesly [2], whose observations were made on children under 12 years of age, found that ether and chloroform invariably induce a temporary acetonuria, which, from his charts, appeared to last about three days.

Telford and Falconer [6] also working in young children, observed 118 cases, including those who had had chloroform only, chloroform with previous ethyl chloride, ethyl chloride only, and ether only, and found acetonuria in 89 per cent. of their cases.

All these observers attributed this acetonuria to a mild post-anæsthetic poisoning, but it seems to me it can be explained by being due, not to a mild degree of post-anæsthetic poisoning, but rather to a

severe degree of ante-anæsthetic preparation. In fairness to the last-named observers, it must be said that they used as a control twenty-five children who were prepared for, but did not receive, an anæsthetic. They state that no acetonuria occurred in these cases, but give no details as to the preparation, which specimens of urine were examined, &c. At any rate, their experience is diametrically opposed to my own, and I can but ask you to confirm those things for yourselves.

I have now spoken about its frequency after anæsthesia, and what I believe to be the cause of that frequency. The next question one has to ask oneself is—Is it harmful? It stands to reason that the nearer a child approaches the physiological standard at this time the better, and that it is not wise deliberately to upset its metabolism before submitting it to this ordeal. My own scanty observations on this point have further shown that children who have not been in this condition at the time of operation have displayed scarcely anything of what one knows as the after-effects of the anæsthetic. Further, in the condition known as post-anæsthetic poisoning the symptoms point to a marked, and in many cases, permanent disorder of metabolism. Without doubt, then, our aim should be to render the child's condition as far remote as possible from such disorder, and not deliberately to point the way to disaster, as it were.

Lastly, then, can this acetonuria be prevented? Most decidedly, yes. Beesly [2] recommended the exhibition of sodium bicarbonate before and after the anæsthetic for this purpose, but an alkali cannot in any way make up for the carbohydrate starvation, it can but neutralize the resulting acidosis, it does not prevent its continued formation, it serves but as a cloak, the hypodermic of morphia to the acute abdomen. Dr. Guthrie [3], to whom we are under a deep debt for bringing the subject of post-anæsthetic poisoning into prominence, has advocated the administration of nutrient enemata up to within two hours of operation, and if this enema contains enough absorbable carbohydrate, the effect would be obtained. What I believe to be the correct method is indicated already in this paper.

The "carbohydrate starvation" must be done away with—not by giving starchy foods, which are bulky, and where the latent period before they can be absorbed is long; nor yet by cane-sugar or milk-sugar, which are not bulky it is true, but where still some preparation for absorption is necessary, and hence some delay. Not to those, then, do we look, but to dextrose, where we have our ideal remedy—it is not bulky, it is ready for absorption without preliminary preparation; it

is readily absorbed, and even when the disorder of metabolism is already set up, it can cause a return to the normal within twelve hours.

Therefore let us see to it rather that there is "no carbohydrate starvation either within four hours of the operation or at any period afterwards," and thus have the child in as healthy a condition as possible both during the time of anæsthesia and in the period following it.

REFERENCES.

[1] Becker. *Virchow's Archiv*, Berl., 1895, cxl, p. 1.
[2] Beesly. *Brit. Med. Journ.*, 1906, i, p. 1142.
[3] Guthrie. *Clin. Journ.*, 1907, xxx, p. 129.
[4] Nachod. *Arch. f. klin. Chir.*, Berl., 1895, li, pp. 646-82.
[5] Pawlow. "The Work of the Digestive Glands" (English translation, 1902), p. 147.
[6] Telford and Falconer. *Lancet*, 1906, ii, p. 1341.

Microscopical Specimens of Kidney and Liver from Cases of Post-chloroform Poisoning.

By R. E. Apperly.

The first specimen was from a rabbit which had been given 1 c.c. of chloroform subcutaneously. After being quite anæsthetic it recovered in about three hours and took its food well; on the third day it became drowsy and died, and its liver showed marked fatty degeneration. The section was stained with hæmatoxylin and eosin. The second specimen was from the same rabbit, and was stained with Sudan III and hæmatoxylin, and the fat globules were stained yellow. The third section showed the kidney of a rabbit stained in the same way. Though this rabbit weighed twice as much as the last, it also had 1 c.c. of chloroform subcutaneously, and went through the same symptoms, dying on the third day. The fourth specimen was the liver of a rabbit which was not injected, but had a series of inhalations of chloroform. The animal was not previously prepared, but was put under a bell-jar, and five minutes were occupied in getting it under. It was then taken out and kept under by hand for half an hour. This was repeated each day for ten days, and the rabbit was killed on the eleventh day. The fatty change was seen to be very marked, especially as the anæsthesia had only lasted five hours. The last two sections were from the human liver. The first was that of a patient who had two anæsthetics—first gas and ether, and a day later chloroform—for the removal of a

hæmatoma which occurred in the scar of the first operation. The man died about eight hours after the second operation, never properly recovering consciousness. Post mortem it was found that he had had cerebellar hæmorrhage. The liver showed very marked fatty changes. Death was due to the cerebellar hæmorrhage. The last specimen was from a patient who had a very long pelvic operation, lasting ninety minutes. He would have liked to give her ether, but she had bronchitis and emphysema, so chloroform was given throughout. She gradually went downhill after the operation and died on the fourth day, apparently of cardiac failure. Post mortem the wound was clean, and the area of operation looked satisfactory, but the liver was yellow, and it could be seen to be loaded with fat; in fact, no healthy cells could be seen. The patient had had some suppuration, and a very foul mouth, from which no doubt toxins had been absorbed, as well as from the carcinoma of the cervix. He thought these factors helped the chloroform to bring about the end. He felt sure that in many cases where death had been thought to be due to cardiac failure it was really caused by a condition of liver such as that now seen.

A Case of Chloroform Toxæmia in a Child to whom Fat was Noxious.

By W. J. McCardie, M.B.

On July 7, 1908, I administered chloroform to an apparently healthy child, aged 2½, for an operation on a cleft palate. When three weeks old he had an anæsthetic for circumcision without ill-effect. When 12 months old I gave him chloroform for harelip operation; he afterwards vomited a good deal of blood, had a temperature of 101° F., was very restless, had some diarrhœa, and was somewhat ill for a few days.

The present operation was difficult and long, lasting about one and a quarter hours; the anæsthesia was borne perfectly well and apparently very little blood was swallowed. Very soon after the operation the child was sick, passed much blood *per rectum*, and at night was very collapsed, the temperature being 104·8° F., pulse 160, irregular and weak. He was very restless, and had much diarrhœa. Saline injections and brandy were administered *per rectum*, because the child was so ill that it was feared he might not live. On the next day, July 8, he was still very restless and ill, but at night he gradually improved. The palatine flaps all sloughed. On July 11 for the first time his urine

was examined, and gave a marked reaction of acetone and a moderate one of diacetic acid.

During the child's illness I obtained the following history: He had been bottle-, or rather spoon-fed on milk and barley- or lime-water from birth, later on Reeves's food for about one year, and then on Savory and Moore's food for nine months. He was always *upset by fats;* even a little cream (one drop) did not suit him, making him sick and his motions of a light colour (stopping biliary secretion). Cod-liver oil alone or in emulsion and Kepler's extract of malt at once upset him. Starchy foods were said to affect him like fats. Lately he had been gradually accustomed to take a little fat in some form or other. For the last six months he had had what were called attacks of "*liver chill*" —i.e., violent sickness and diarrhœa, the motions being very light. The attacks lasted once for a month and at other times for about a week. When ill he did best on Savory and Moore's food and raw meat juice. He had never been jaundiced.

On July 14, that is a week after his operation, I anæsthetized the child by E_2C_1 and continued with ether by means of Junker's inhaler for half an hour during operation for the repair of the broken-down flaps. Deep anæsthesia was maintained, there was very little bleeding, and the boy was apparently none the worse afterwards. The urine was then found to contain a trace of diacetic acid, but no acetone. Two days later, that is on July 16, E_2C_1, followed by ether, was again administered as before for nearly an hour. The child again was quite well afterwards, and his urine contained no trace of diacetic acid or acetone.

Noteworthy points: (1) The noxious effect of fats is remarkable in this case, as also that of chloroform, which sets free fat in the liver and other organs. (2) The liver attacks (compare Guthrie's theory). (3) The comparative harmlessness of ether.

Notes of a Case of (?) Post-anæsthetic Poisoning.

By Mrs. Dickinson Berry, M.D.

Eighteen months ago the patient, a girl, aged 16, was admitted to hospital suffering from acute abdominal pain and vomiting. She was operated upon early the following day, for torsion of a parovarian cyst. The operation was an easy one, and occupied less than half an hour;

the anæsthetic was taken extremely well, only a small quantity of chloroform being required. The vomiting stopped after the operation, and for two days the patient appeared to be comfortable and doing well, but was noticed to be somewhat apathetic. On the third day after the operation there was slight jaundice, and the apathy seemed to have increased; during the small hours of the next morning she had acute abdominal pain and vomiting, and the temperature rose rapidly from normal to 102·8° F. The abdomen was again opened, but nothing was found to explain the symptoms, so it was closed at once. The patient never recovered consciousness, but passed into coma, death occurring ten hours after the second operation. Post mortem, there was but little found to explain the cause of death; the pedicle looked healthy and there were no signs of peritonitis, but the liver was fatty and the lungs were œdematous and congested. It was diagnosed as post-anæsthetic poisoning. On microscopical examination chains of streptococci were found in the blood films, but it was suggested that this might have been a post-mortem result, as the post-mortem examination did not take place until twenty-four hours after death.

Mrs. Berry stated she had not seen this patient herself. She was told that the lips towards the end of her illness had been of a cherry-red hue, and that stains of the same colour had been observed on her back. This was suggestive of chloroform poisoning. She asked whether rise of temperature had been noted in other cases supposed to be due to post-anæsthetic poisoning.

DISCUSSION.

Mr. APPERLY said that Dr. Frew attributed the post-anæsthetic acetonuria to the difference in the diet, and the regulation of the diet, before the operation; but Mr. Apperly did not know how he could prove this, because one found a different amount of acetonuria according to whether ether or chloroform was given. He had quantitatively estimated a number of cases before and after operation, and in a straightforward case in which there was no suppuration, minute traces of acetone were found twenty-four hours before operation. Ether was then given, and there was a tremendous rise in the amount of acetone in the urine, up to fifty times the former quantity. It fell next day, and soon returned to normal. With chloroform as the anæsthetic in a similar kind of case there was a rise to about twenty times the quantity found before operation, with a diminution during the three or four subsequent days; it taking four days to reach the normal. After chloroform there was a marked effect on the kidney, as well as on the liver; and the kidney change

probably delayed the excretion of whatever acids caused the acid intoxication, so setting up that intoxication. But ether seemed to cause no kidney changes, and even in the liver there was not much change, apart from a little cloudy swelling. In regard to the acetone, the whole value lay in the quantitative estimation, as it was present in some quantity after every anæsthetic. In cases of delayed chloroform poisoning the acetone was small in amount, and it was because the excretion was delayed that the condition was set up. In post-operative vomiting, when the patient was vomiting violently six hours after the anæsthetic, and continued in that way for twenty-four hours, very little acetone would be found in the urine. Directly the acetone in the urine increased the vomiting stopped. He could confirm Dr. Frew's statement about the effect of glucose and dextrose in stopping symptoms. If the patient was having saline after the operation, and there was some post-operative vomiting, adding glucose to the saline would cause the vomiting to stop quickly, and the patient's condition would greatly improve.

Mr. FLEMMING said he could confirm the remark of Mr. Apperly as to the effect on the kidneys after chloroform poisoning, in contradistinction to the effect produced by ether. In sixty-eight cases he had carefully centrifugalized and examined the urine after the administration of chloroform and of ether respectively; and there could be no doubt that the percentage of kidney cells passed was much more increased after chloroform than after ether narcosis, and after chloroform some granular changes were evident in the cells which came away. The urine of these cases was very carefully standardized with regard to its specific gravity, then centrifugalized and the cells examined. He asked whether Dr. Frew considered that the percentage of fluid in the body might have anything to do with the causation of the disability to digest carbohydrates. He asked because so many of the complaints which were associated with marked acetonuria had for their symptoms a wasting of fluid, such as by vomiting and profuse sweating, diarrhœa, &c. And in some cases of acetonuria which he observed, he found that those cases which had had previous injections of saline twenty-four or forty-eight hours before the operation had very slight after-effects, and the urine showed very little acetone.

Dr. THURSFIELD desired to make a few remarks, chiefly on the pathology and ætiology of the condition. In the beginning of 1911, it had been the misfortune of the Children's Hospital, Great Ormond Street, to have a series of post-anæsthetic deaths, and in the course of the investigation which followed he had read a good deal of the literature on the subject. The facts concerning post-anæsthetic poisoning seemed to be quite clear. There was a general consensus of opinion that the administration of an anæsthetic, not necessarily, but usually, chloroform, almost invariably produced a condition of acidosis, and that in a certain number of cases this acidosis passed into a severe condition of acid intoxication. The main evidence for the existence of acidosis was the appearance of acetone in the urine. Dr. Frew's paper had shown that

acetonuria was a common condition in all children immediately after their admission to the hospital, and this and similar facts seemed to point to the conclusion that in the past too much stress had been laid on the phenomenon. In the speaker's opinion it was necessary to look for a further factor which converted the normal slight acidosis into the dangerous condition of poisoning. Carbohydrate starvation was undoubtedly a cause of acetonuria, and the suggestion that cases of post-anæsthetic poisoning should be treated with soluble carbohydrates was made on physiological grounds. It was, however, his impression that the records of this form of treatment were not very successful, and that when the condition was established the injection of an alkaline solution was more efficacious. Further, the administration of dextrose did not appear to prevent the appearance of acetone in the urine after an anæsthetic. The condition of the liver in these cases was not, in his opinion, specific; other patients dying of quite different diseases showed lesions in that organ which were indistinguishable from the degeneration found after death from post-anæsthetic poisoning. The fatty degeneration was an accompanying, not a causal, factor in the condition. In answer to a question whether acid intoxication followed ether and chloroform equally, he said that the recorded cases of death after the administration of ether were few in number, but that of course the number of patients who had ether administered was comparatively small.

Dr. G. W. GOODHART said he had done some work on the liver from the pathological aspect, and he was glad to hear Dr. Thursfield say he did not think the liver condition had anything to do with the causation of post-anæsthetic poisoning. But he disagreed with the remark that many other things would produce the same result. Many other things would cause central fatty degeneration, but he thought there was a distinct feature in post-chloroform necrosis of the liver, in that one found a very distinct differentiation into three zones: an outer peripheral zone of healthy cells, a middle zone of fatty degeneration, and, if the case went far enough, complete necrosis around the central vein. He doubted whether that sharp differentiation existed in anything except chloroform poisoning. During the last few years he had cut many post-mortem sections of livers in Guy's Hospital, and among them were twenty cases which died after operation. Of those there were two cases in children and one in an adult, which showed that differentiation very well. None of the cases showed clinically any signs of post-anæsthetic poisoning, though they all had chloroform; two had had it pure, and one had had A.C.E. mixture. In all the cases the anæsthesia lasted about one hour, and the patients died between one and three days after the operation. He would also defend the kidney, and he doubted whether the fatty degeneration attributed to the kidneys was due to the chloroform. It was necessary to be careful in talking about fatty degeneration of the kidneys, because so little was known about the amount of fat in the kidney under varying conditions. From the experimental point of view it was very difficult because fat occurred in great quantity in animals which were apparently healthy, and in his own

experience on animals he had not been able to demonstrate to his satisfaction that there was any increased quantity of fat compared with the normal animal. He asked whether anybody had done experimental work on the effect of dextrose. He had done some himself, but with very unsatisfactory results. He had given it to rabbits in large quantities 100 c.c. of the 10 per cent. solution, both by the mouth and intraperitoneally, and had given chloroform by inhalation, subcutaneously and intraperitoneally. In no series did he find any difference in the condition of the liver. The same remark applied to the giving of sodium bicarbonate; the animals had died just as speedily as if it had not been given, and the changes in the liver were identical with those in the control animals.

Mr. G. WAUGH expressed the opinion that "post-operative acetonæmia" was a better term than "delayed chloroform poisoning," and said that the sooner the latter term was abolished the better, as it was an obstacle to progress, and was a slur on the condition which was regarded as the prime cause. There were two problems to face. One was "what conditions induced the appearance of acetone in fatal quantities in the patient?"; and the second was "how did those conditions combine to produce the acetone?" Those two must be kept distinct in dealing with the subject. He believed that much confusion had been brought about by not insisting on that great difference. The occurrence of acetonæmia after the administration of an anæsthetic was pointed out originally by Dr. Guthrie, and until the present paper by Dr. Frew he did not think any contribution had been presented which threw any light on the problem of the occurrence of acetone in large quantities in any person. Two years ago he (the speaker) was much concerned both with the number of cases of post-operative acetonæmia which were occurring and the cause of them, and he was questioning the correctness of the tradition of surgery which prepared their patients after the classical manner. The surgeon was going to inflict an outrage upon an individual of a nature which led to much shock. He was going to poison him during that period by causing him to inhale a toxic vapour. On ordinary grounds it was absolutely necessary that the vitality of the patient should be conserved as a preliminary at its highest level. Tradition, on the other hand, had taught that those patients should be starved beforehand, that one should inflict an attack of acute diarrhœa upon them, and that they should be prohibited all nourishment for the next thirty-six hours. Two and a half years ago he boldly detached himself from that tradition and had abandoned that mode of procedure, and with gratifying results. He had abolished all purges, all enemas, and refused to cut short the patient's meals, except that the operation was substituted for the last meal. He enriched them with a large dose of soluble carbohydrate before the operation, and he had a large quantity of soluble carbohydrate administered by the rectum, or under the skin, or into the veins subcutaneously. That course had enabled him to steer clear of the danger of post-operative acetonæmia which was such a serious menace. Those who had listened to Dr. Frew's paper must see that the speaker's methods were a piece of empiricism which had anticipated

the now scientific basis which Dr. Frew had laid down as to the conditions which tended to produce acetone in excess in the patient, and that by adding ordinary surgical operative procedure to these conditions, acetone would be produced up to a fatal point. He mentioned his own methods beforehand, because both the author and he had been attacking the problem from different standpoints; Dr. Frew as the scientist, making his observations over numbers, statistically checked; himself as the empiricist, who had been working from a different point of view, but probably achieving the same result. His colleague, Dr. Thursfield, had referred to the cases of delayed chloroform poisoning which occurred in the hospital to which both Dr. Thursfield and himself were attached, and a Commission had been appointed to consider the question. Dr. Thursfield had not referred to one or two salient points, and on one point at least he was in disagreement with that gentleman. He (the speaker) would only refer to his own figures, because they were his property. During the time that the number of cases quoted of post-operative acetonæmia which were ending fatally occurred, as quoted by Dr. Thursfield, and which were so serious in others that a fatal issue was momentarily feared, he operated on over 2,000 children who had been prepared on the method he had indicated. The chloroform was given by a variety of administrators, in varying quantity, of varied quality, other anæsthetics besides chloroform were used; and yet whilst that considerable epidemic of grave acetonæmia was occurring, there was no instance among the 2,000 cases upon whom he had operated during that time. He regarded that as a striking testimony to the value of Dr. Frew's observations on the preliminary induction of acetone formation in the individual by carbohydrate starvation. The severity was determined by the fatal issue or by the fact that the patients hung between life and death during several days. But he had had no case which caused the slightest anxiety, nor was there any reason to anticipate that anxiety when the condition of carbohydrate starvation was attended to and avoided.

Dr. Thursfield referred to the value of bicarbonate of soda, and he (Mr. Waugh) could relate two cases which bore strongly upon that, as he personally had not convinced himself of its value at all. Before he began to alter his mode of preparing the patients he operated upon a healthy woman, aged 25, taking out her appendix during a quiescent period. There was nothing unusual about the operation, and as he was told she was well, he did not see her for thirty-six hours after the operation. When he did go to see her he was shown a dying woman. When the reason was asked he was told that the serious condition had developed only in the last two hours. She was cyanosed, and so drowsy that she could only be roused with difficulty. The pupils were large and only reacting very sluggishly, her pulse-rate was 130, and she had vomited only once. It had been stated that 80 per cent. occurrences of vomiting took place after a carbohydrate administration. But vomiting was totally irrelevant to the question, as there might be acetonæmia without vomiting, and there might be profound vomiting without acetonæmia. This woman had vomited only once, her urine was laden with

acetone, and there was its characteristic odour. He gave her 500 gr. bicarbonate of soda immediately, and telephoned later to know how she was. In three hours the report came that she was very much worse. He therefore ordered that she should be given ounce doses of dextrose hourly by mouth, and in three or four hours she was out of danger. That was open to the criticism that the bicarbonate already given might have acted subsequently; but for three hours whilst it was inside her she became worse. He took the stitches out on the tenth day and the wound was healed. There was no question of sepsis. Lately in abdominal surgery the use of the Fowler-Murphy method of treating patients by rectal irrigation with saline had been enormously on the increase. He thought it had increased the liability to the occurrence of post-operative acetonæmia, because of the carbohydrate starvation established by it. He had therefore modified it, and to all his saline rectal irrigations 2 per cent. of dextrose was added as a routine. He would quote two more cases. Six weeks ago two children developed signs of post-operative acetonæmia while under the care of a colleague. As that colleague was not available, he (Mr. Waugh) was asked to see them. He thought there was no hope for either of them, as they were clearly dying; the patients were cyanosed and unconscious, their pulses were uncountable, the urine was loaded with acetone and their breath reeked of it. He advised infusion into the veins with a 2 per cent. solution of dextrose. One of the patients died, but in spite of the apparently hopeless condition, the other immediately got well. There was still a quantitative aspect of the problem to face. If a certain degree of acetonæmic poisoning had been reached no antidotes were likely to be effective. In these two cases the two children were indistinguishable in the degree of their moribundity, but he considered that he was able to get enough carbohydrate into one child to stave off the disease, but in the other the stage reached was such that remedies had no effect.

He thought Dr. Frew could rightly claim that there was a scientific basis for the procedure, and although he had not attempted to solve the problem as to how carbohydrate starvation was likely to induce the excessive acetone formation in the individual, he had rightly said that carbohydrate starvation was the main condition which led to the state of affairs under discussion, and that the responsibility lay with the medical man to see that neither before nor subsequent to operation were any patients exposed to the danger of such starvation. He already felt enormously indebted to Dr. Frew for his paper, and he felt that that gentleman had justified the methods which had obtained for him (Mr. Waugh) a freedom from those disasters, and he hoped that Dr. Frew's work would receive a wide recognition, as he was most firmly convinced not only of its scientific value but also of the enormous value of its practical application to one of the gravest dangers that beset the path both of the anæsthetist and of the surgeon.

Dr. SILK wished to express his thanks to the members of the Great Ormond Street Hospital Staff who had kindly come forward to lay before the Section their views on this interesting subject. He was cordially in

agreement with the remark of Mr. Waugh, that it was about time that the label "post-anæsthetic poisoning" was discarded. The idea that all these cases were directly due to the administration of anæsthetics had been demolished by Dr. Frew's observations. It was clear that post-operative acetonuria was dependent upon a series of very complicated causes, of which only one was the chloroform.

Dr. FREW, in reply, said he started out to show that the change of diet in children was sufficient to cause acetonuria, and it had already been shown by numerous observers that gross carbohydrate starvation was sufficient to cause acetonuria in 100 per cent. of the cases, and that both those factors were operating in most cases which were given an anæsthetic. Telford and Faulkner, in their paper, gave percentages under the various anæsthetics, and they used chloroform and ether only, and the mixtures mentioned in his paper. They were working on children only, and their figures did not bear out what Mr. Apperly had found in his experience. The percentages were practically identical for every anæsthetic, about 89 per cent. in each. The quantitative estimation of the substances was not very important from his point of view, though at first quantitative estimations were made of β-oxybutyric acid. It was found that the test used was roughly quantitative by the depth of colour obtained. With regard to dextrose feeding, he had only had experience of cases which were moved from the medical side for operation. To some of those dextrose was given, to others not. In order for dextrose feeding to be efficacious it must be given for some time beforehand, because in 60 per cent. there was acetonuria already present. A drachm of dextrose was given per hour, and it was interesting to note that some of the children who had acetonuria took it readily, but when the urine became free from that substance the children refused to take their glucose. With regard to the question whether the percentage of fluid in the body had anything to do with the acetonuria, he had not observed that point particularly; 100 per cent. of the babies taken from the breast and fed artificially developed acetonuria. It seemed to be more a question of age, and change of diet, than of anything about the child's general condition. Mr. Waugh had largely answered Dr. Thursfield's remarks. Dr. Thursfield said that dextrose feeding had no effect on the acetonuria. He (Dr. Frew) had the opportunity of seeing some of the dextrose feeding, and he considered it ineffective for the purpose. With regard to the cases which Dr. Thursfield said were kept in the ward for three days before being operated upon, of course that would rule out the 60 per cent. of cases due to change of diet, but the preparation for the operation, and the long period of starvation afterwards would have produced acetonuria in 100 per cent. of the cases, and so this was not necessarily the result of the anæsthetic.

Section of Anæsthetics.

March 1, 1912.

Dr. W. J. McCardie, President of the Section, in the Chair.

Ether Infusion Anæsthesia.

By Felix Rood, M.B.

The progress in the principles of general anæsthesia has been marked recently by two considerable advances. The first of these was based upon the demonstration of the fact that the inhalation of a 2 per cent. chloroform vapour was practically free from danger. The recognition of this fact led to the invention of numerous regulating inhalers. Almost as soon as the advantage of these methods was appreciated it was recognized that there are dangers connected with the administration of chloroform other than those occurring during the actual operation. It came to be realized that chloroform might be responsible for deaths occurring several days after it had been administered, and although unanimity upon this question is not yet reached, the conditions under which post-anæsthetic acidosis is most likely to occur are now fairly well defined.

The increasing importance which is being attached to the possibilities of acidosis has led to greater attention being directed to the use of ether. It was soon found that the closed inhaler formerly in use for the administration of this drug was by no means so indispensable as had been supposed. The administration of ether by the open method proved to be comparatively easy and very satisfactory in a large portion of patients.

The second great step in the development of anæsthesia was the discovery that certain narcotics and atropine could be combined with the administration of ether by the open method, and that an anæsthesia could then be produced which had all the advantages of chloroform, and none of its special dangers. In regard to the safety of the patient during

the operation, and in regard to post-anæsthetic acidosis and finally in regard to post-anæsthetic gastric and pulmonary complications, this method when applied to all classes of surgical practice gives, on the whole, results decidedly better than those of chloroform. It is probable that this opinion would be most likely to meet with opposition in relation to post-anæsthetic lung complications. That it is, however, not unsupported by evidence is shown by the following record by some of the German surgical clinics:—

ANÆSTHESIA: CHLOROFORM OR ETHER BY CLOSED METHOD.

Surgeon	Number of laparotomies	Per cent. of lung complications
Körte	3,909	7·2
Czerny	1,302	3·9
Kümmel	1,754	2·5
Trendelenburg	1,829	5·4

ANÆSTHESIA PRODUCED BY ETHER: OPEN METHOD.

Krönlein	1,409	0·56

These figures show an advantage on the side of the use of ether by the open method so great as to be unmistakable. Lately the superiority even of ether given by the open method has not remained unchallenged. The necessity of using it in enormous amounts brings into prominence the disadvantages inherent in what may be called the respiratory method of administration.

An attempt may now, perhaps, be made to enumerate some of the more obvious difficulties necessarily contingent upon the use of the respiratory method:—

(1) It limits one entirely to the use of highly volatile substances, and it would seem probable that if solubility rather than volatility were the only physical character demanded for the administration of an anæsthetic, pharmacological research might be able considerably to enlarge the number of available drugs.

(2) The respiratory method must always in essence be indirect, the anæsthetic being absorbed by the vapour introduced into the air-passages, so that the dose actually entering the blood is rendered variable by the notorious variability with which it enters the chest, being subject to all the accidents of laryngeal spasm, of respiratory spasm and of variations in the depth of respiration.

(3) The exposure of a very great surface of the respiratory tract to an irritating vapour cannot but be regarded as a serious disadvantage and danger. To anyone who has inhaled a breath or two of ether or

chloroform vapour, it must always be a source of surprise that serious complications are not invariably produced by a prolonged anæsthetic, as we know they quite frequently are.

(4) To produce anæsthesia, the anæsthetic drug must be introduced into the blood and maintained there at a certain percentage. Any further amount than this present in the body will act as a reservoir, upon which the patient continues to draw after the administration has been stopped. Hence it follows as a disadvantage of the respiratory method that the absorption of the drug cannot be stopped abruptly, but must go on until the patient has rid himself of the vapour which fills his lungs, and the anæsthetic-soddened mucus contained in his bronchial tubes and usually in his stomach as well. Many an anæsthetist has had cause to deplore the existence of this reservoir when during the administration of chloroform the air-way has become obstructed, and the patient has continued fatally to absorb the chloroform which was shut up in his lungs.

(5) The operations on the mouth, jaws or pharynx: The difficulties of the surgeon are much increased by his having to share the field of operation with the anæsthetist and it frequently happens that one or other has to be content with a less degree of technical perfection than he would have attained if he had not been embarrassed by the necessity of the other.

It is clear that a direct administration of an anæsthetic by the blood disposes of all these difficulties. The administration can be reduced to the minimum amount necessary, the drug can be introduced with perfect regularity in an exactly measured dose, and the administration can be stopped instantly, and there is no reservoir of the drug upon which the patient can continue to draw, while, finally, the lungs are not exposed to irritation.

The method of introducing ether intravenously was introduced by Burkhardt. His immediate successes encouraged others to follow. At the Berlin Surgical Congress, Kümmel was able to report ninety cases. In no case of the ninety did post-anæsthetic vomiting occur, although in a large number of the cases the operations were done for abdominal diseases of the gravest kind.

Technique.

Burckhardt used an ordinary infusing apparatus containing a 5 per cent. solution of ether in normal saline. He allowed enough of the solution to run in to produce anæsthesia, then interrupted the stream until the patient showed signs of coming round, when the dose was

repeated. It was soon realized that the interruption of the stream brought with it a tendency to thrombosis in and about the cannula. To counteract this disadvantage two vessels were arranged, either of which could be connected with the cannula separately; one contained ether solution, and the other normal saline. In this case the continuity of the stream was maintained with the saline in the intervals between the administrations of ether. The uneven anæsthesia produced in this way had disadvantages from the point of the anæsthetist as well as that of the surgeon. The great objection to methods of administration by periodic doses is that it is necessary to produce a greater concentration of the anæsthetic in the blood after each dose than is essential for adequate anæsthesia. This principle has been so fully emphasized by those to whom we owe the introduction of the Vernon Harcourt apparatus, that it is not necessary for me to labour it further here. Aware of the importance of this principle, one soon recognized quite early that by the application of it the infusion method might be freed from a defect inconsistent with the best modern practice. The chief modification therefore which is introduced into the apparatus is an indicator rendering possible a minute graduation of the inflowing stream.

The apparatus consists of an upper reservoir to contain a supply of the solution. This reservoir is connected by a rubber tube to a regulating chamber. This chamber besides acting as a regulator also acts as an indicator. And, although the stream of fluid is interrupted so that its rate of flow can be seen, the pressure at the same time is not interrupted, but is transmitted continuously by the cushion of air contained above the fluid in the regulating chamber. The fluid then passes from the regulating chamber to the warming chamber and so on to the cannula.

The Solution.—Ether is soluble in normal solution to the extent of 10·8 per cent. by volume. In the first few cases of this series a 10 per cent. solution of ether, that is, 2 oz. of ether to the pint of saline, was used. It was found that with this concentration a transient hæmoglobinuria occurred. This was usually present in the first specimen of urine passed after the operation, and not afterwards. On account of this a 5 per cent. solution, 1 oz. to the pint, was tried. No hæmoglobinuria followed. In order to be still more certain that there was no blood destruction, a number of specimens of the blood were taken at intervals throughout several administrations. These were examined microscopically, and a count of the red corpuscles made—this showed no

hæmolysis. Finally, the 5 per cent. ether solution was mixed with blood and examined, both in a test-tube and microscopically; this also showing no laking. I think that these are sufficient grounds for assuming that there is no blood destruction. Later experiments on the same lines with a solution containing 1½ oz. of ether to the pint of saline showed that this concentration was equally free from risk.

Technique of Induction.

The saline solution is prepared in flasks, in which it is boiled; the neck of the flask is then plugged with sterile gauze, and the solution allowed to cool. Shortly before required the ether is poured into the cold sterile saline. The flask is then well shaken—the solution becomes opaque for a moment, then gets quite clear; it is obvious that the ether is completely dissolved. The whole apparatus is boiled previous to use. It is then fitted to the stand and filled with the ether solution.

The patient is prepared for the anæsthetic in the ordinary way—also a hypodermic injection of atropine, atropine and morphia, or atropine, morphia and scopolamine. The exact dose and combination given depend, of course, on the age and condition of the patient and also upon the pathological conditions present. The next step is to introduce the cannula into the vein. The most convenient vein is either the median basilic or the cephalic—in the arm. It requires so much greater pressure to make the solution flow in the veins of the leg, that it is hardly possible to use them. The skin of the arm is prepared. A solution of iodine in ethyline dichloride is a very convenient method. A little eucaine solution is injected and the vein exposed and the cannula tied in. For this part of the administration it is absolutely necessary to have an assistant, as the taps of the apparatus have to be manipulated at a time when the administrator is occupied in tying the cannula into the vein, a proceeding that requires the strictest asepsis, as a wound of this character left exposed so long and irritated by the presence of the cannula is peculiarly liable to go septic, which may be followed by the gravest possible consequences. When the cannula is fixed into the vein the solution is at first allowed to flow in rapidly, the regulating tap being turned full on. Induction of anæsthesia is usually quite smooth and rapid, three or four minutes being the average time. Struggling during induction is rare, and if it does occur is easily controlled, as one is not dependent upon respiration for the introduction of the drug. The ordinary signs of anæsthesia, of course, occur — automatic respiration,

muscular relaxation, and abolition of reflexes. As soon as smooth anæsthesia is established, a little experiment will enable one to arrive at the minimum amount required, and the apparatus can be set to deliver this amount. Of course, during a long operation this can be gradually reduced. The noticeable features of the anæsthesia are regularity and smoothness, also the ease with which it can be graduated and the great rapidity with which patients respond to slight alterations in dosage.

Before endeavouring to indicate what I believe to be the advantage of this method, it will be perhaps best to give a few details of a number of cases in which I have been able to use this method. I have tabulated the 136 cases which I have under six headings :—

Nature of case	Condition before operation	Induction	Course of anæsthesia	Dose	After-effects
Laparotomies, 62 :— Abdomino-perineal 2; Perforated gastric ulcer ... ... 2; Partial gastrectomy 3; Gastric entero-stomy ... ... 15; Excision of portions of gut ... 19; Other abdominal 11; Total ... 62	23 cases were in a bad state before operation	Smooth; no vomiting or cyanosis	No vomiting; smooth and regular anæsthesia; severe shock developed in 4 cases in which long and severe operations had been done	Variable; ¾ to 2½ pints in first hour; most given, 4¾ pints in three hours	3 patients vomited once or twice; 1 developed pleurisy, but the operation was in this case for a septic condition in the upper abdomen
Operation upon the mouth and jaws: 53 cases	1 case was in a state of extreme inanition before operation	Excitement occurred in 6 cases; no vomiting or cyanosis	No vomiting; smooth, regular anæsthesia; shock developed in 9 cases, which were prolonged operations lasting over two hours for malignant disease	¾ to 2½ pints in the first hour	3 vomited slightly; 1 case of malignant disease of mouth with foul, septic ulcers developed septic broncho-pneumonia
Operations on limbs and thorax: 21 cases	2 cases bad before operation	No vomiting; excitement in 1 case	No vomiting; shock in 1 case	¾ to 1½ pints in the first hour	No vomiting

It is, of course, dangerous to make any definite statements as to advantages, indications, &c., of a method based on so few cases. But I think it is only fair to consider that we are not really dealing with an entirely new method of anæsthesia. We know, by experience, that ether is a safe drug; we know that saline infusion is also a safe procedure. We have, therefore, reasons for believing the method to be a safe one, which we should not possess were the drug employed an unknown one.

As far as my own experience goes the most striking advantages of the method are seen in those cases where the patient is in a condition of extreme inanimation—viz., cases of abdominal malignant disease. These patients often leave the table in a much better state than they were before. So that in any case in which the patient is likely to be benefited by a saline infusion—either as a means of relieving shock or hæmorrhage, or because shock is expected—this method has given excellent results. Similarly, the results have been good with acute abdominal conditions generally and especially so in cases of ruptured gastric or duodenal ulcer.

A degree of relaxation of the abdominal wall quite equal to that produced by any other anæsthetic, excepting stovaine, can be obtained by this method. It frequently happens with the inhalation method of administration that although it is otherwise indicated, ether cannot be given because some form of pulmonary disease exists; the infusion method is very useful in these cases and can be given without risk of irritation. Then there is that large class of cases involving operations upon the mouth and jaws. Many of these cases involve long and extensive dissections associated with much shock. It is therefore a distinct advantage to be able to substitute ether for chloroform with equally good results. The saline infusion is also beneficial, as these proceedings often involve considerable loss of blood. And also even in minor operations of this type, the separation of the spheres of activity of the surgeon and anæsthetist cannot but make for comfort, convenience and asepsis. In certain operations upon the throat and nose a more or less vertical posture is essential—under these conditions a smooth, even narcosis can be obtained with this method without congestion or danger of syncope.

For any type of case in which ether as an anæsthetic is not contra-indicated, this method has the one great advantage of absence of unpleasant after-effects. Some of the records of the German surgical clinics show that this is a marked feature of infusion anæsthesia. Kümmel reports ninety cases, and in no case did post-anæsthetic vomiting occur. Personally, I have not been so fortunate as this, but out of 136 cases only six vomited at all, and of these, three were cases in which blood had been swallowed during the operation.

With regard to the use of this method of anæsthesia in children, my experience is limited to three cases, one aged 8 years, one 4 years, and one 4 months. In each case the result was very encouraging, although two of the children were practically *in extremis* at the time of

operation. The amount of solution required was very small. Of course, atropine alone was given previously, and the patients "came round" almost immediately the infusion was discontinued.

As a general criticism of the method it has been suggested that it is a complicated one. I think that this must be admitted. It is obvious that from its nature the method must be carried out under the strictest aseptic precautions, and I cannot but feel that any great simplification of technique or apparatus might lead to the adoption of the method in a somewhat impromptu way, with possible disastrous results. Although at present the method is, comparatively speaking, a new one—and we can only go on recording cases and judging by results—yet it seems certain from the experiences up to the present, which have everywhere been so encouraging, that the method will certainly take its place as one of the recognized means of inducing anæsthesia.

Hedonal Infusion Anæsthesia; a Report on Seventy-five Cases.

By C. M. Page, M.S.

The induction of general anæsthesia by the intravenous injection of drugs was first effected in man in 1872 by Oré [8]; the drug employed was chloral, fifty-three cases were reported, in fifty-one of which satisfactory results were obtained. Further experience, however, proved that the procedure was not without danger, and the method does not seem to have been further developed till 1909. In that year Burkhardt [2] published his application of the principle, using solutions of chloroform first and then ether.

In 1910, Professor Federoff, of St. Petersburg, suggested the intravenous administration of hedonal. Since 1900 this drug had had some vogue as a hypnotic; it had also been given by the mouth (in several Russian clinics) as a precursor of chloroform and ether with satisfactory results. Reports of its use as a general anæsthetic first appeared in Russian journals [7], a preliminary note was published in German in 1910 [9], and in the following year Jeremitsch [5] described the technique employed, with details of sixty-five cases. At last year's Congress of the German Surgical Association, Federoff [4] contributed a brief account of 530 cases collected from three Russian clinics: no

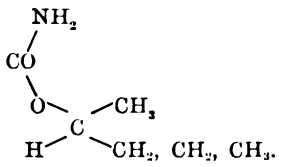
death directly attributable to the anæsthetic was reported among the series.

The method adopted is one of interrupted injections. A 0·75 per cent. solution of hedonal in normal saline is placed in a flask from which it can be forced by air-pressure; leading from the flask is a long rubber tube terminating in a fine hollow needle. A superficial vein is exposed, punctured by the needle, and a volume of fluid sufficient to produce general anæsthesia injected; the needle is then withdrawn and re-inserted for the purpose of further injections as they become necessary. In the course of an operation lasting one hour and fifty-one minutes, five such injections were required. In Jeremitsch's sixty-five cases, the total amount of fluid injected in any one instance varied from 325 c.c. to 1,100 c.c. In two cases satisfactory anæsthesia was not obtained; in one the vein isolated was very small, and the injection was carried out with difficulty; in the other instance the injection was made into the internal saphenous vein; anæsthesia was not induced after the injection of 800 c.c. of fluid; the case was one of varicose veins. As a result of this latter experience, Jeremitsch advises against infusion into the veins of the lower extremity. The only complication of anæsthesia which he records is respiratory depression. This appeared in one of his cases, and was attributed to a too rapid injection of the fluid; Federoff notes that respiration was affected in eight out of the 530 cases he collected. Jeremitsch states that vomiting or headache were usually absent in the post-operative period; he detected no evidence of irritation of the kidneys, and had no case of local thrombosis in the veins.

Hedonal anæsthesia has been employed at St. Thomas's Hospital in over 100 cases; in all instances satisfactory anæsthesia has been obtained. I have details of the first seventy-five cases in which it was used. On these seventy-five cases the statements in this paper have been based. I have adopted a method of continuous gravity-fed infusion. Before going into details of the technique and the general results I will give some account of the characteristics of hedonal.

Properties of Hedonal.

Hedonal or methyl-propyl-carbinol-methane has the following formula:—



It is a white crystalline solid, stable at ordinary temperatures. Its solubility in water at 100° F. is 1 in 100; it readily crystallizes out of a saturated solution in long, fine needles; the solution can be boiled without the properties of the drug being affected. Prolonged heating diminishes the concentration owing to the volatilization of a certain amount of the compound. It does not appear to alter in composition when kept for some months in the dry state.

Experiments on animals by Dreser [3] showed that hedonal had ten times the hypnotic effect of urethane, and twice that of chloral; he found that the respiration was not materially affected in conditions of deep narcosis; it caused a slight fall in the blood-pressure. Dreser noticed that the drug had a diuretic action, which is attributed to the theory that in the body the substance breaks up into urea, carbon dioxide, and water. Lampsakow (quoted by Krawkow) [6] carried out further experiments, and found that in dogs and rabbits a dose of 0·4 to 0·5 grm. to the kilo of body-weight produced complete general anæsthesia; the respiratory and cardiac rhythm remained regular. If toxic doses were given, the heart continued beating, after paralysis of the vasomotor centre. He considered that the presence of the amino radicle antagonized the paralytic action of the drug.

No fatalities attributed to the use of hedonal as a narcotic have been recorded.

Preparation of Solution.

Hedonal, as supplied by Bayer, is dissolved in normal saline at a temperature of 70° F. to make a 0·75 per cent. solution; the resultant fluid is filtered once, boiled for five minutes, and stored in sterile flasks.

Technique of Induction.

The contents of a flask are warmed up to about 140° F. and poured in the tank, which is furnished with a thermometer gauge and tapped exit tube. From the latter runs a yard of pressure tubing; in the course of this a dropper is inserted; it terminates in an ordinary fine infusion cannula. The whole apparatus has been made for me by the Holborn Instrument Co. Before infusing, the temperature of the fluid in the tank is reduced to about 115° F. by pouring in cold solution if necessary; for long operations the tank may be placed in a heat-retaining jacket, but usually the fall in temperature is not great enough to be of importance. A good-sized superficial vein is then exposed after the

injection of ½ per cent. novocain; the cannula is tied in as for an ordinary venous infusion. The fluid is run in at a rate of 50 to 150 c.c. in the minute; the rapidity of flow can be accurately controlled by regulating the tap of the tank and watching the flow through the dropper. In the first fifty cases we infused into the median basilic or cephalic vein; for this to be effected, the arm chosen must be fixed, either to a bracket of the operation table or to a separate stand. Latterly we have usually injected the internal saphenous vein where it passes over the internal malleolus; this has the advantage that the leg requires no special fixation, the scar left is out of sight, and the anæsthetist and his apparatus is well out of the way of the surgeon.

Phenomena of Anæsthesia.

Within a minute or so of the commencement of the infusion the patient becomes drowsy, and often yawns; he has a subjective feeling of warmth and well-being, the face flushes, and if the infusion is too rapid a certain degree of cyanosis may arise; then follows a period in which the patient appears to be in a state of deep, natural sleep; this state merges rapidly into one of complete general anæsthesia, the deep reflexes are absent, and the corneal reflex absent or sluggish; the pupil usually remains small. In none of the cases has there been any struggling, a few have shown slight movements of the extremities, and some talk incoherently during induction. The respiration remains quiet and regular unless it is obstructed by the falling back of the tongue, an emergency which must be guarded against; the pulse remains full and steady; the blood-pressure drops slightly, and then remains fairly constant.

Course of Anæsthesia.

The amount of fluid necessary to induce anæsthesia varies considerably—e.g., 40 c.c. sufficed in a child aged 10 months; 1,000 c.c. were necessary in a heavily built man aged 25; the average dose necessary in an adult is 500 c.c. The period of induction has varied from two to thirteen minutes. As soon as anæsthesia is established the rate of flow of the fluid is cut down to a slow drip: the necessary rate varies with the type of patient, and is soon gauged by experience; as soon as the reflexes show signs of returning the rate of flow is increased again.

It is noticeable that, with this anæsthesia, the patient may react to cutaneous stimulation, at a time when the laxness of the muscles and the absence of conjunctival reflex would lead one to believe the subject to be deeply narcotized.

The largest total amount of solution infused in any one case was 1,750 c.c., given in Case 31, a man, aged 61; anæsthesia lasted over an hour. The longest duration of anæsthesia was one hour and thirty-five minutes, in Case 60, a woman, aged 36; 1,125 c.c. of solution were used. It is noticeable that old people require less of the drug than do younger ones of similar body-weight.

Post-operative Course.

The rate of return of complete consciousness is slow, though variable. Usually the patient sleeps for six to twelve hours after the cessation of the anæsthetic and then awakes. Drowsiness may persist for twenty-four hours; in three cases there has been some emotional disturbance at this stage. In some instances (seventeen in this series) a period of excitement comes on an hour or so after the operation; the patient, however, soon settles off to sleep if morphia is given in the usual dosage; when consciousness completely returns there is no memory of this stage. Vomiting is exceptional unless there is some irritative cause for it. It occurred in twelve cases, but in no instance was it persistent. Headache of slight degree and lasting part of a day was complained of in fifteen cases. The urine showed no change in any of the cases in which it was tested for the common abnormalities. The blood was examined before, during and after anæsthesia in six cases by Dr. H. B. Weir; he did not find any evidence of hæmolysis. In no case has there been any evidence of local thrombosis in the veins.

Dangers of Hedonal Anæsthesia.

The only symptom in this series which has given cause for alarm has been cyanosis. A certain degree is common during the initial stage of anæsthesia, and passes off if the rate of flow of the fluid is diminished. It has appeared during the course of the operation in six instances; in all except one instance it was caused by obstruction of the air-passages, either from the falling back of the tongue and jaw, or from pressure on the trachea; in Case 54 a large dose of fluid had been infused unusually rapidly, and the respiratory movements became slow and shallow; the normal rhythm returned in a few minutes after the inflow had been cut down.

Federoff [4] reports that temporary stoppage of respiration occurred in eight of 530 cases; in each instance normal breathing set in again after a few movements of artificial respiration.

Disadvantages of the Method.

The exposure of an adequate vein sometimes gives difficulty; if the novocain injection is properly made the procedure is painless. In children the vein is necessarily small; but in Cases 52 and 66 the internal saphenous vein, where it lay over the internal malleolus, proved to be of sufficient size. Induction of anæsthesia may be slow if the fluid does not flow freely; such a state of affairs may be due to the use of too small a cannula, a partial block in the tubing, or to the selection of a vein of exceptionally narrow calibre. In one or two instances where infusion was carried out in the leg, slow induction has seemed to be associated with a varicose condition of the veins, the fluid percolating all the vessels of the lower extremity first, and, as it were, remaining in a backwater of the general circulation for a time. The prolongation of narcosis after the operation may be undesirable in trivial cases; it might prove dangerous from the inhalation of vomitus in cases of unrelieved intestinal obstruction.

Advantages.

The technique is simple, the solution is readily handled and sterilized, and the active part is not volatile. The anæsthesia attained is of a steady and complete type, the relaxation of the muscles is remarkable, being often as good as that given by spinal anæsthesia. The respiratory movements are steady and quiet. The removal of the anæsthetist from the scene of the operation in head and neck cases is a distinct advantage. In cases of hæmorrhage or collapse the combination of the anæsthetic with infusion is very valuable. The absence of any inflammatory complication in the lungs has been a noticeable feature in this series; in only three instances have any been observed. The quiet sleep, insensitiveness to pain, and absence of straining, conditions which usually last for six or eight hours after operation, are certainly very desirable in many cases. Owing to the comparatively slow rate at which the drug is excreted, very small quantities of solution suffice to maintain anæsthesia when once it has been completely established; thus, in Cases 51 and 60 the operation lasted over an hour and a half, and the total amounts of fluids infused were 1,180 c.c. and 1,125 c.c. respectively. In operations on the thyroid and larynx (Cases 31, 35, 57 and 67) the quiet, even anæsthesia is very satisfactory.

Contra-indications to the Method.

In conditions where there is a marked engorgement of the lungs, or when the cardiac action is not properly compensated, infusion anæsthesia is likely to put a dangerous strain on the right side of the heart. In cases of nephritis it might be expected that the kidneys could not adequately deal with the infused solution, but in three cases in which there was evidence of chronic nephritis no ill-result ensued; in Case 68 the right kidney was excised from a patient suffering from general tuberculosis of the urinary system; there were no uræmic signs at any time, and she has made a good recovery. It is noticeable that the amount of fluid necessary to produce anæsthesia in these cases was not very large. Arterio-sclerosis, and conditions in which the blood-pressure is high, do not seem to be absolute contra-indications to the use of the method.

A few attempts were made to reduce the amount of fluid infused by administering other drugs before the induction of anæsthesia. In Cases 67 and 69, an injection of scopolamine $\frac{1}{100}$ gr., morphine sulphate $\frac{1}{6}$ gr., and atropine sulphate $\frac{1}{100}$ gr., was given half an hour before the operation. In Cases 70, 71 and 72, 4 grm. of hedonal were given in an emulsion about three-quarters of an hour before operation, and followed by a hot drink. In the first two cases the amount of solution necessary to produce anæsthesia was rather less than might have been expected, but in most instances there was more cyanosis than usual. In one of these, in which a thyroid adenoma was enucleated, this was chiefly due to mechanical pressure on the trachea. Of the latter three cases, two were partly narcotized before the commencement of the infusion as a result of the dose by the mouth, and required much less than usual of the solution. The other was in a state closely resembling the excitation stage of ether anæsthesia, but required a considerable dose to induce complete narcosis. All three slept heavily after the operation and seemed somnolent and somewhat depressed during the following day. I think that for strong, healthy patients a dose of 3 grm. of hedonal given about two hours before the operation would give better results.

Analysis of Cases which Terminated Fatally.

In this series no death has occurred which could be attributed to the anæsthetics; five died at some period subsequent to the operation while still in hospital. An account of the course of these cases follows:—

Case 19.—F., aged 60. Compound comminuted fracture of the leg; amputation below the knee. The wound healed well, and the patient appeared to be in a state of good general health till three weeks after the operation, when she died suddenly with the signs of pulmonary embolism. Post mortem: A large ante-mortem clot was found in the pulmonary artery. There was no evidence of local thrombosis in the vein (median basilic) which had been opened for the infusion.

Case 21.—F., aged 71. Senile gangrene of feet; amputation in thigh. Death one month later from spreading gangrene of the remaining leg.

Case 26.—M., aged 71. Diabetic gangrene of foot; amputation in thigh. The patient was in a drowsy state before operation and died in coma thirty-two hours afterwards. The urine contained much sugar, diacetic acid, and albumin in fair quantities.

Case 27.—F., aged 24. Suppurating hydatid cyst of liver; cholecystotomy. Death one month later. Post mortem: Suppurative cholangitis, suppurating cyst of liver, empyema and general peritonitis.

Case 65.—F., aged 65. Carcinoma of rectum. Laparotomy for acute intestinal obstruction. At the operation general peritonitis was found following a perforation in the large bowel. The patient was in a state of extreme collapse before the operation; she improved after the infusion, but sank again afterwards and died seven hours later.

Since writing the above a death from syncope has occurred in a feeble old man, half an hour after the administration of hedonal. A gastrostomy was performed; signs of heart failure set in directly after the administration of a feed. Post mortem: Carcinoma of the œsophagus opposite the bifurcation of the trachea, from which an abscess had spread into the mediastinum. The aortic valves were grossly diseased and incompetent.

Summary.

(1) The intravenous infusion of a 0·75 per cent. solution of hedonal in normal saline produces general anæsthesia.

(2) Administration of the solution by continuous infusion gives good results.

(3) The anæsthesia is steady and complete, is associated with great relaxation of the muscles, and has a wide margin of safety.

(4) During anæsthesia the respiration remains steady; the pulse remains good; the blood-pressure usually falls slightly.

(5) The induction of anæsthesia is subjectively very pleasant to the patient. Little if any excitement occurs during this stage.

(6) Anæsthesia is established in from five to ten minutes. The rate of inflow of the fluid should be from 50 to 100 c.m. to the minute; a slower rate greatly delays the induction of anæsthesia; a more rapid one may produce signs of cyanosis.

(7) The comparatively slow rate at which the drug is excreted makes it possible to maintain anæsthesia for prolonged periods without infusing a very large volume of fluid.

(8) The anæsthetic stage usually merges into one of deep sleep which lasts from six to twelve hours.

(9) Vomiting or headache in the post-operative period are uncommon.

(10) Pulmonary complications are rare.

(11) The dangers which may arise during anæsthesia are—respiratory depression from an overdose of the drug and respiratory obstruction from falling back of the tongue and jaw.

(12) The method is very suitable for operations about the head and neck. The muscular relaxation and quietness of the respiratory movements make it a valuable anæsthetic for operations in the upper part of the abdomen.

In conclusion, I wish to acknowledge the courtesy of the members of the surgical staff of St. Thomas's Hospital in permitting me to make use of the cases admitted under their care. I am indebted to several of my colleagues for much assistance in carrying out these observations, more especially to Mr. B. C. Maybury, Surgical Registrar, who has done a great deal of work in the matter.

REFERENCES.

[1] ARNDT. *Therap. Monatschr.*, Berl., 1901, xv, p. 179.
[2] BURKHARDT. *Münch. med. Wochenschr.*, 1909, lvi, pp. 1678, 2365.
[3] DRESER. *Verhandl. Gesellsch. deutsch. Naturforscher u. Ärzte, 71 Versamml. zu München*, Munich, 1899, Teil ii, 2 Hälfte, pp. 46-49.
[4] FEDEROFF. *Verhandl. d. deutsch. Gesellsch. f. Chir.*, 1911, xl, part 2, p. 222.
[5] JEREMITSCH. *Deutsch. Zeitschr. f. Chir.*, Leipz., 1911, cviii, p. 553.
[6] KRAWKOW. *Arch. f. exp. Path. u. Pharm.*, Leipz., 1908, Suppl. Band., p. 317.
[7] LICHKOVSKI. *Russk. Vrach*, St. Petersb., 1910, ix, pp. 1447-49; POLÏENOFF, *Vrach Gaz.*, St. Petersb., 1910, xvii, p. 1331-33.
[8] ORÉ. *Compt. rend. hebd. des Séances de l'Acad. des Sci.*, 1874, lxxviii, pp. 515, 651; lxxix, pp. 531, 1014, 1416; 1876, lxxxii, pp. 1215, 1272; lxxxiii, p. 837.
[9] SIDORENKO. *Zentralbl. f. Chir.*, Leipz., 1910, xxxvii, pp. 1219-21.

DISCUSSION.

Mr. BARRINGTON WARD related a case of death during hedonal anæsthesia at the Hospital for Sick Children, Great Ormond Street. The case was that of a little girl, aged 8, under the care of Mr. Corner. She was of fair size and development, and, so far as could be determined, quite normal except for tuberculous glands of the neck and enlarged tonsils. The method used was that described by Mr. Page, who had kindly allowed him to see some cases of his. The injection was normal, the left saphenous vein being selected. The operation was commenced ten minutes after the anæsthesia was begun, and at that time 300 c.c. of hedonal solution (0·75 per cent.) had been used. He did not think the child was deeply anæsthetized, and she was of quite good colour. The operation had lasted another twenty minutes, and had practically been completed. As the skin stitches were being put in, another 200 c.c. of the solution having been given, without any warning the patient stopped breathing, which was the first untoward sign he noticed, as a good colour was still maintained. There was no respiratory obstruction. Efforts at resuscitation were made by artificial respiration, bleeding, injection of strychnine, atropine, and ether, and massage of the heart through the abdomen, but, unfortunately, without success, though they were persisted in for one and a half hours. The heart could not be said to have resumed its beating. At the request of the coroner the autopsy was made by Dr. Spilsbury, when the condition found was an enlargement of lymphoid tissue generally all over the body. The thymus was distinctly enlarged, and it enwrapped the great vessels at the base of the heart. The Malpighian corpuscles of the spleen were prominent, and there was a mass of lymphoid tissue about the ileum and cæcum; the tonsils were large, and a greatly hypertrophied ring of lymphoid tissue was found about the base of the tongue. The heart was dilated, did not look abnormal in colour, but on subsequent section and microscopical examination it was found to exhibit fatty degeneration. The bases of the lungs were slightly œdematous, and there was an excess of free fluid in the pericardial sac. Dr. Spilsbury considered it to be a case of status lymphaticus. He did not propose to say more about the general subject of this anæsthetic, as the cases of it at that hospital had so far been few, twenty to thirty, and he had had no trouble with it except in this case.

The PRESIDENT (Dr. W. J. McCardie) said the Section was fortunate in having had so much recent good work brought before it at one sitting, and both Mr. Rood and Mr. Page could be congratulated on the number of their cases, their results, and the improvement in technique which they had effected. They were also indebted to Mr. Ward for the clear account he had given of his fatality. He only wished equally good reports of all the fatalities during and after anæsthesia were furnished; more would then be learned about the causes of such fatalities.

Dr. BARTON had listened to the paper with special pleasure, because two years ago, at a discussion opened by Dr. Buxton on the choice of an anæsthetic, he expressed the hope that the method would be given a trial in this country. But he did not think the field for the method was quite such a large one as Mr. Rood seemed to make out; it had its limitations. Nor did he consider that it offered, in the ordinary way, better results than did scopolamine, morphine, and open ether, or the modification of it which he generally used. Mr. Rood said very little vomiting followed in his cases; this was also true of the method just named, and all the advantages the author claimed for infusion the speaker obtained by inhalation. But there were cases in which it was not advisable to give an anæsthetic by inhalation, such as some operations on the upper air-passages, and there infusion would be useful. There were one or two minor drawbacks in connexion with infusion, one of which was that two men were required, one to watch the apparatus and the other to watch the patient; this might be a matter of importance in private cases. So far the method appeared to have been a safe one, but as time went on dangers might develop, perhaps through momentary want of attention or through some fault occurring in the apparatus, by which air-bubbles might get into the veins. He had heard of large amounts of saline being injected, with not very good anæsthetic results. Possibly owing to this there might be cases of œdema of the lungs, and there was also the risk of thrombosis occurring in a vein, which was greatly obviated by the useful device shown. With regard to technique, he understood that the solution used had been 5 per cent., and that 10 per cent. had been found too strong to be safe. But he was glad to hear that $7\frac{1}{2}$ per cent. strength might be used, because anæsthetists had reported that large amounts of saline had to be injected. The apparatus shown seemed to be very ingenious, but it was rather expensive. In giving ether in this way the process was not so simple as the intravenous injection of saline solution or "606"; it required a more elaborate apparatus, but he thought the present apparatus was capable of being simplified. Dr. Maguire had brought out a very simple apparatus for carrying out his formalin injections, and he thought that, with slight modifications, it might be used in these cases.

Dr. SCHARLIEB said he could assure Dr. Barton that it was easy to carry out ether infusion anæsthesia single-handed; he saw no chance of injecting air-bubbles. If the solution were warmed too much, ether bubbles went into the vein, but they did no traceable damage. With regard to the large quantities Dr. Barton had heard of, the largest he had used was 8 pints of $7\frac{1}{2}$ per cent. solution in two and a half hours, the operation being a Wertheim; but that was due to the fact that the apparatus was not working quite correctly, because the stopper was not air-tight. The patient had no œdema of her lungs, she was carefully watched, and though she left the table looking like the subject of acute nephritis, there was a total absence of trouble afterwards. The expense of the apparatus was chiefly in the stand.

Dr. HAROLD LOW said he had had no experience of ether infusion, but he had seen the hedonal method carried out in a considerable number of cases at St. Thomas's Hospital. The number of cases in which it had been used in England did not enable one, so far, to dogmatize as to its safety. They had heard of a fatal case, but the patient was the subject of status lymphaticus, in which condition the patient might die though no anæsthetic was administered at all. There had been no fatal cases at St. Thomas's Hospital. In certain cases it was a great advantage, from the surgical point of view, to remove the anæsthetist from the field of operation; also in operations on the air-passages and the mouth the inhalation of ether was difficult, and for them they were accustomed to use the more potent chloroform. Patients preferred injection into the vein to the inhalation of ether or chloroform; they went under in a perfectly quiet way—they went to sleep. The after-effects were very considerably less than those after ordinary inhalation anæsthesia. There was not so much vomiting, and the prolonged sleep afterwards did the patient good; pain was not felt for some considerable time after the patient had been put back to bed. With regard to the maintenance of the anæsthesia, that which he had seen after hedonal was perfect of its type; the patient remained quiet, the breathing was the same throughout, and the muscles were completely relaxed. With regard to the detrimental effects, in two or three cases he had seen considerable cyanosis, and he wished to insist that the anæsthetist's duty was just as arduous and continuous when giving an anæsthetic by the vein as by inhalation. This was a drawback when the injection was done into a vein of the leg, for the anæsthetist should be at the patient's head, to watch the breathing and the colour of the face. He had noticed cyanosis if there was any obstruction to respiration. In one case the surgeon had to remove a cyst of the thyroid, which cyst had, by pressure, reduced the trachea to a keel shape, and when the tumour escaped from the capsule it dragged on the apex of the trachea, and reduced the passage to a mere chink; the patient became very cyanosed, more so than would have been the case under ordinary anæsthesia. He thought it was due to a mixture of the drug circulating in the body *plus* the want of air. In several cases he had used free inhalation of oxygen, either throughout the operation or when the patient became cyanosed. In another case which he saw, he believed cyanosis was due to the entrance of the drug having been too rapid. Therefore the respiration should be watched very closely. He did not think that either of the new methods would take the place of the routine method of anæsthesia, but they would take their special place for suitable cases.

Dr. SILK said that his experience of the methods of anæsthesia under discussion was limited to some seventeen or eighteen cases, and it was entirely due to the kindness of Mr. Rood in showing him the details of the procedure that he had been able to carry it out at all. As was usual with all beginners, he had met with difficulties. As had been remarked by Mr. Rood himself, it was not easy to work the method single-handed. For aseptic reasons he

thought that it was a moot point whether it should not be considered the duty of the surgeon to find the vein and tie in the cannula, leaving the anæsthetist to watch the patient and regulate the flow of fluid. He had not yet quite made up his mind upon this. In a considerable number of his cases there had been more or less lividity at the outset; this he had attributed to the sudden increase in the amount of fluid in the cardio-vascular system, and it was usually quite transient. It had been stated on all sides that this form of anæsthesia was of distinct advantage in throat and mouth cases. It so happened that most of his cases so far had been of this kind, and he had come to the conclusion that the advantages of this method over the older plans had been somewhat over-rated. It was true of course that the anæsthetist and his apparatus were removed from the area of operation; but was this always an advantage? If the anæsthetist could not be at the head of the patient, but had to attend to the apparatus, an extra assistant had to be turned on to hold the jaw forward and swab out the mouth, duties which were usually performed by the anæsthetist. In course of time perhaps the surgeon would realize that it was not always good to have the anæsthetist at the foot of the bed. With regard to hæmorrhage, he would like to ask Mr. Rood whether he did not think that this was often increased, as for instance in big upper jaw operations? Two of his most recent cases indicated fairly the sort of difficulties that arose in this method. The first was in a big, burly, North Countryman, aged about 70, upon whom a very extensive operation for removal of the right upper jaw was performed. The operation lasted for nearly two hours, and about 2 pints 15 oz. of the 5 per cent. solution of ether were injected; not an excessive quantity considering the size of the patient. The day following he seemed to be doing very well, but the day following that he had an attack of tachycardia, œdema of the whole head and surgical emphysema of the neck and upper part of the chest. The cardiac symptoms gradually became more pronounced and permanent, and he died on the sixth day with every indication of cerebral thrombosis. The surgical procedures were severe enough in his opinion to account for the fatal termination of this case, but it had been suggested that the thrombus was swept into the cerebral circulation by the intravenous injection. The second case occurred in connexion with the removal of a malignant thyroid; a typical case apparently for intravenous injection. Very early in the proceedings someone had to be told off to hold the jaw forward. Then the larynx and trachea became blocked with blood, the patient got very cyanosed, and stopped breathing, tracheotomy had to be performed, and even then he had to do artificial respiration for some time. He quite agreed with Dr. Low, that this method would find its level, and would not be used as a matter of routine. It was for anæsthetists and surgeons to determine what that level was. So far, it seemed to him that it was chiefly of service in those cases where a dose of salt solution would be given in any event.

Dr. HAROLD LOW explained that he did not mean that the anæsthetist himself should be away from the field of operation, but his apparatus. Of course the place for the anæsthetist was at the head of the patient. With reference to the cyanosis in the thyroid case, the colour did not return for some time after the obstruction to the breathing had been removed.

Mrs. DICKINSON BERRY asked Mr. Page whether he considered there were any advantages in hedonal infusion over ether infusion.

Dr. BLUMFELD said it was owing to Mr. Rood's kindness that he had had experience with the method, and he was impressed with the first case in which he saw it used—a very long abdominal section in a stout lady, from whom many sections of bowel were removed. Her condition seemed so admirable that the method seemed well worth trying. Since then his experience had been satisfactory, but he had selected the cases for it on the lines which Dr. Low mentioned, and on the principle of selection which he believed would be followed in the future. The patients had been those in whom the addition of fluid was in itself an advantage. He would like information as to the rapidity with which the injection of the anæsthetic was carried out. He thought he himself had put in the fluid too slowly, and would be glad to know how long Mr. Rood took to get in the first pint of fluid. An allied question concerned the strength of the solution. He also had felt that as anæsthesia took so long to produce with a 5 per cent. solution, a stronger form might be tried. He never took less than fifteen minutes to get the patient under. His experience of after-effects was the same as the author's, and the patients had been free from vomiting. But he did not see how one could expect these patients to be free from after-effects on the lungs, because the action of the ether was the same, however given, and it must traverse the lung. The difference was that when injected it was not inhaled into the alveoli, but presumably it went into the pulmonary circulation and the patient's breath smelt strongly of ether. With larger experience he believed it would be found that the ether would produce bronchitis when injected, though perhaps not so much as when inhaled.

The PRESIDENT said the Section had heard a most interesting account of the new methods. He had read that Kümmel had used the method successfully in ninety cases; Burkhardt employed it in 250, and spoke well of it. Some German authors, however, did not speak so well of intravenous infusion. One used it in only six cases and had imperfect anæsthesia in five. Pikin used it in sixteen cases, and had one death, the patient dying a few minutes from the commencement of the infusion; therefore he gave up using the method. Küttner warned against its employment because of the danger of thrombosis. In three of the cases infiltration of the lung occurred. Janssen, experimenting with the method on dogs, found that one died from embolism in the lung. He had used various anæsthetics and had given up

the infusion. Schmitz Pfeiffer used it in forty cases, and he only recommended it in cases where other methods of producing anæsthesia were unsuitable. He (the President) asked whether in Mr. Rood's cases any mucus was excreted or cough excited when no atropine, scopolamine or morphine had been previously given. The Section was much indebted to Mr. Page for his account of the physiological and pharmacological action of hedonal. It was a drug about which very little was known, and of the same group as urethane, and Federoff's 530 cases passed off without undesirable complications. He (the President) wished to know whether scopolamine and morphine were used before the infusion of hedonal, and if not whether any mucous secretion was noted in the air-passages. In this method anæsthetists possessed an addition to their armamentarium which might be very useful in selected cases for operations on the upper part of the body, just as spinal anæsthesia was specially applicable in certain selected cases for operation on the lower regions of the body.

Dr. PROBYN-WILLIAMS asked in what proportion of Mr. Rood's cases ether alone was given, and in how many morphine and scopolamine were given beforehand. He would also like to know what experience Mr. Rood had had of clotting in the cannula, and in what way he overcame it.

Mr. ROOD, in reply, said it had been suggested in the discussion that the after-results obtained by infusion anæsthesia were possibly no better than those obtained with the open ether when scopolamine, morphine and atropine were injected first. But speaking from the records of the German clinic, Kümmel reported ninety cases in which there was no after-vomiting. Of his own 136 cases, which were carefully watched with the idea of detecting after-sickness, only six vomited, and three of those had swallowed blood during the operation. With regard to bronchitis, the ether was excreted from the lungs, but he thought the amount of that drug in contact with the pulmonary alveoli must be less when it was passing outwards from the blood into the air than it was when being absorbed by the blood from a highly concentrated vapour in the alveoli. The method did require surgical cleanliness, but the cannula and tube could be boiled with the instruments, and left in a dish at the side so that the anæsthetist could expose the vein, pick up the cannula and insert it into the vein. The tube connecting the warming apparatus and the cannula could be fixed and the tap turned on by the assistant. Although a competent assistant was a very great advantage in an emergency, a nurse could attend to the necessary details. The largest amount of saline he had used was between 4 and 5 pints, but apparently larger amounts had been employed over a long period, and the blood-pressure was never raised, and so long as the saline was run in slow enough there was no danger. Admittedly the apparatus was cumbersome; indeed, that might be an advantage in that it might determine that it should only be used where rigid asepsis was likely to be carried out. With regard to Dr. Silk's

suggestion that the surgeon should find the vein and do that part of the procedure, that would be an added task for the surgeon, and would necessitate the surgeon being ready to operate some time before the patient was anæsthetized. With regard to throat and mouth cases, he agreed with Dr. Silk that it was not always an advantage to have the anæsthetist away from the field of operation. But there were certain throat and mouth cases which were now being performed with such a rigid system of asepsis that the anæsthetist was obliterated almost completely, even when giving the anæsthetic by the inhalation method. The alternative anæsthesia for throat and face operations was chloroform, and with the latter drug the hæmorrhage would be less than with ether. In some of the cases he had seen, the hæmorrhage was considerable, but in others not so great. With regard to the rate of injection, after the first thirty or forty cases, in which 5 per cent. solution was used, as the anæsthesia was slow it was found that the strength of the solution might be increased from 1 oz. to $1\frac{1}{2}$ oz. of ether to the pint with complete safety. The period of induction was about four minutes for a fairly lusty individual. The tap was turned full on during the period of induction, and for a healthy adult man about 12 oz. would be required to get him under. For weakly persons about half that quantity would be used. Of course, more of the anæsthetic was used for the first few minutes; it was then much reduced, for fear of an overdose. He only gave it in one case out of the 136 without the previous injection of atropine, and in that one case the patient was practically *in extremis* with ruptured ectopic gestation. She certainly salivated with the infusion, but not to a great degree. In all the other cases atropine, atropine and morphia, or atropine, morphia and scopolamine were given previously, the exact dose and combination employed depending upon the condition of the patient.

Mr. PAGE, in reply, said he did not agree that in thyroid cases anæsthetized with hedonal, cyanosis occurred more than with inhalation anæsthesia; he believed it was less, and that there was less congestion in the air-passages. The anæsthetist need not be at the head of the patient in mouth cases if Kühne's intubation tube was used, but cyanosis had always to be looked out for. If the air-passages were clear and the pulse could be felt, all was well with the patient. After two cases of carcinoma of the jaw and cheek which he operated upon, it was very striking how well the air-passages were kept open by this method. The pharynx was packed off, and the intubation tube remained in position while the operation was carried out. In answer to Mrs. Berry concerning the advantages possessed by hedonal over ether anæsthesia, he had not had much experience of the latter, but the hedonal infusion apparatus was more simple and was composed of metal, which was unbreakable; it did not easily get out of order, there was only one tap, and a dropper which could be carried separately, and the whole apparatus was capable of being boiled in the average hospital sterilizer. The stand was a matter of relative unimportance. In one

case where it was the method chosen the apparatus was put on a shelf. The solution could be boiled and remained stable; the active principle was not volatile, and therefore the solution could be put into the vein at the body temperature or a little over. But ether often entered the body comparatively cold, and that fact might account for some of the shock that occurred when very much of it was used. He understood that hedonal solution was much less irritating to the human tissues than was a solution of ether. There was no evidence of irritation about the wound when hedonal was given, but there was in some cases of ether infusion. Dr. Dixon considered that urethane was an ideal anæsthetic, and hedonal was a derivative of it. It was not excreted by the lungs, but was broken up and excreted by the kidneys. This meant that there was a minimum of irritation of the lungs. For patients to have eight hours' sleep following big operations on joints and on the abdomen was a great advantage. Hedonal was excreted comparatively slowly, and, therefore, when anæsthesia had been established no large quantity of fluid was required to maintain anæsthesia. In two cases scopolamine and morphine were given before hedonal, and in both the cyanosis was more marked than in the cases in which it was not used. In three cases he gave hedonal by the mouth one and a half hours before the operation, and followed it by a drink to the extent of half a pint. The patient came to the operating theatre in a sleep, and much less fluid was necessary to induce anæsthesia. This seemed a desirable method in cases where it was advisable to limit the amount of fluid. What appeared a disadvantage was that the period of slumber lasted for twenty-four hours after the operation in these cases.

PROCEEDINGS

OF THE

ROYAL SOCIETY OF MEDICINE

VOLUME THE FIFTH

COMPRISING THE REPORT OF THE PROCEEDINGS FOR THE SESSION 1911-12

BALNEOLOGICAL & CLIMATOLOGICAL SECTION



LONDON
LONGMANS, GREEN & CO., PATERNOSTER ROW
1912

Balneological and Climatological Section.

OFFICERS FOR THE SESSION 1911-12.

President—

G. Hobson Thompson (Buxton).

Chairman of Council—

Alfred F. Street, M.D. (Westgate-on-Sea).

Vice-Presidents—

Samuel D. Clippingdale, M.D. (London).
Charles Henry East, M.D. (Great Malvern).
J. Morgan Evans (Llandrindod Wells).
T. Fred Gardner, M.D. (Bournemouth).
A. C. Gurney, M.D. (Eastbourne).
John Hackney, M.D. (Hythe).
Percy G. Lewis, M.D. (Folkestone).
R. L. Jones Llewellyn, M.B. (Bath).
A. L. Mackenzie (Bath).
A. G. S. Mahomed (Bournemouth).
F. A. de T. Mouillot, M.D. (Harrogate).
F. M. Sandwith, M.D. (London).

Hon. Secretaries—

A. Manuel, M.B. (London).
F. A. de T. Mouillot, M.D. (Harrogate).

Other Members of Council—

R. Ackerley, M.B. (Llandrindod Wells).
Frederic Bagshawe, M.D. (St. Leonards).
W. Bain, M.D. (Harrogate).
Charles Begg, M.B. (Bath).
J. B. Berry (Ramsgate).
Charles W. Buckley, M.D. (Buxton).
G. W. Hamilton Cumming, M.D. (Torquay).
W. Edgecombe, M.D. (Harrogate).
N. Hay Forbes, F.R.C.S. (Church Stretton).
R. Fortescue Fox, M.D. (London).
G. R. Green, (Ripon).
Wm. Black Jones, M.D. (Llangammarch Wells).
A. Hill Joseph, M.D. (Bexhill-on-Sea).
Preston King, M.D. (Bath).
T. Pagan Lowe (Bath).
Arthur P. Luff, M.D. (London).
Henry McClure, M.D. (London).
A. Mantle, M.D. (Harrogate).
Alexander Morison, M.D. (London).
George Oliver, M.D. (Farnham, Surrey).
H. Shirley-Jones (Droitwich).
Ernest Solly, F.R.C.S. (Harrogate).
Septimus Sunderland, M.D. (London).
W. J. Tyson, M.D. (Folkestone).
Cyril J. Williams (Woodhall Spa).
Leonard Williams, M.D. (London).
Neville Wood, M.D. (London).

Representative on Editorial Committee—

R. Fortescue Fox, M.D. (London).

Representative on Library Committee—

Septimus Sunderland, M.D. (London).

BALNEOLOGICAL & CLIMATOLOGICAL SECTION.

CONTENTS.

October 27, 1911.

PAGE

Presidential Address: Baths and Climate in relation to Spa Treatment; some Reflections and Suggestions. By G. H. THOMPSON 1

December 13, 1911.

The Treatment of Gastro-hepatic Dyspepsia at Vichy, Carlsbad, and Cheltenham. By GUSTAVE MONOD, M.D. 7

January 25, 1912.

Bubonic Plague. By F. M. SANDWITH, M.D. 17

March 6, 1912.

The Spa Treatment of Neuritis. By WM. ARMSTRONG 21

April 17, 1912.

A Discussion on Radium Emanation in Mineral Waters. Opened by T. PAGAN LOWE 29

Mr. G. H. THOMPSON (p. 38)—Dr. ARMSTRONG (p. 39)—Dr. BUCKLEY (p. 41)—Dr. PRESTON KING (p. 43)—Dr. ACKERLEY (p. 44)—Dr. JONES LLEWELLYN (p. 45)—Dr. MANTLE (p. 46)—Dr. FORTESCUE FOX (p. 46)—Mr. PAGAN LOWE (reply) (p. 47).

The Council think it right to state that the Society does not hold itself in any way responsible for the statements made or the views put forward in the various papers.

LONDON:
JOHN BALE, SONS AND DANIELSSON, LTD.,
OXFORD HOUSE,
GREAT TITCHFIELD STREET, OXFORD STREET, W.

Balneological and Climatological Section.

October 27, 1911.

Mr. G. H. THOMPSON, President of the Section, in the Chair.

PRESIDENTIAL ADDRESS.

Baths and Climate in relation to Spa Treatment; some Reflections and Suggestions.

THE use of baths and, indeed, of water in all its forms carries us back to the dawn of civilization and medical treatment, and it fills, and must ever fill, the place of the most widely used of all remedies. Such a universally prevalent body would early appeal to the intelligence of pioneer sanitarians, and no doubt its vital position in the life of man would reveal its remarkable properties, its great capacity for the absorption and communication of heat, its power as a solvent agent, and its different and easily changed physical conditions. Its specific heat, which is the standard, is the greatest of all bodies, and is the most nearly approached by that of the human body. The physical conditions of solid, liquid and gas, its solvent powers and heat capacity, renders water a vital necessity in disease as well as in health. Internally, as a medium in which solid bodies are carried to the various tissues, and externally, as an application, with its easily varied temperature, it is well known and universally used.

Air, another important factor in climate, plays a principal part in the human economy. Although its heat effects are not so great as water, its rapid change of temperature, quick circulation, effect in the evaporation of the skin, varying vapour capacity, and the part it plays in respiration, render it a most vital agent to man. Though the grouping of mankind into populous centres has in so many ways led to the advancement of civilization, in many other respects it is productive of causes that make for disease, such as the vitiation of the

air by overcrowding. It was natural under such circumstances for man to again seek out resorts renowned for their health-giving breezes and pure air, more especially when they were associated with some reputed miraculous fountain or well-known curative spring. These springs, with their bathing establishments, though of remote date, were the forerunners of the modern spa.

Baths and bathing were in use from the most ancient times in civilized countries. In Egypt and Persia, and later in Greece, bathing was practised. Hippocrates was well acquainted with the physiological action of waters and their use. The Lacedæmonian was the inventor of the hot-air bath, called after him. The Greeks had private baths, and later public baths were also built.

But it was left to the omnipotent and omnipresent Roman to found a system of baths and bathing, which has probably never been excelled in luxury and completeness, even up to the most recent times. Mæcenas first built public baths, called thermae, at his own expense, and successive Emperors, Agrippa, Nero and others, aimed at securing popularity by following his example. The ruins of the baths at Caracalla attest the character and magnificence of these structures. Hot and cold baths, douches, compresses, &c., were employed at the time of Augustus, and Antonius Musa, a pupil of Asclepiades, obtained notoriety by curing the Emperor of a chronic catarrh with the cold bath. The poet Horace, another of his illustrious patients, was also benefited. Wherever the Roman found springs he made use of them, and generally erected thermal establishments. That he appreciated climatic and spa treatment there is ample evidence. The baths at Baiae were much patronized, and the remains at Pompeii point to a similar share of popularity. Later these public baths became associated with places of amusement, and during the early Christian era fell somewhat into disrepute, nor was their glory ever fully revived.

It is certain that for a long period balneological procedures languished, and only slowly revived again in the Middle Ages. During this period springs were often under the patronage of some tutelary saint, and it was customary to leave offerings as evidence of gratitude. So struck were the people by the wonderful cures of these natural waters that they often attributed their power to a divine origin. But the zeal of the reformers laid a heavy hand on these relicts, as witness a letter from Sir William Bassett to Lord Cromwell:—

> I have sent your Lordship the image of St. Anne of Buxkston . . . which images I did take from the places where they did stand, and, for

that they should be no more idolatry and superstition there used, I did not only deface the tabernacles and places where they did stand, but did also take away crutches, shirts, and shifts with wax offered.

My Lord, I have locked up and sealed the baths and wells at Buckstons, that none shall enter to wash there until the King's and your Lordship's pleasure be further known.

In modern times a divine origin is still sometimes claimed for a spring, witness, Lourdes, yearly drawing a large number of votaries to its shrine.

Bathing was again revived in Italy and France in the fifteenth and sixteenth centuries, and in the seventeenth century in England, and later in Germany. In 1697, Sir John Floyer wrote a "History of Cold Bathing." Towards the end of the eighteenth century two English physicians, Currie and Jackson, made a scientific study of the use of water in fevers, which attracted much attention, but the work of these able pioneers was lost sight of.

Priessnitz, an Austrian peasant born in 1790, first began to treat every kind of ailment, chronic or acute, with various hydropathic methods, most of which were known to the Austrian peasantry, but now organized into a systematic method. His idea was that diseases were caused by acrid humour of the blood, and the skin was the proper organ for its removal. With such a powerful remedy, and such a sensitive medium of application as the skin, it is not surprising that, empiric though he was, he achieved a wonderful success. Priessnitz was practically the founder of the modern hydropathic, which for so long a time remained in the hands of empirics and charlatans, consequent upon the general indifference shown by medical men to the study.

But the advent of competent physicians to hydropathic establishments has led to rational and scientific treatment. The development of modern spas has been gradual; some, like Buxton and Bath, date back their origin from the Roman period. The facilities of present-day locomotion have speedily increased the number of health resorts, and enable crowds of visitors annually to receive benefit and change. The modern spas provide not only their special mineral waters and climatic treatment, but all forms of baths and exercises, natural and mechanical.

The authorities, whilst rightly specializing on the peculiar and particular properties of their mineral waters, are fully alive to the advantage of accessory means of treatment. Not only do they provide

hese supplementary aids, but also those secondary and advantageous means which make a health resort a desirable place to recuperate in.

Spa physicians are now frequently called upon to advise their fellow-citizens on balneological questions, and as some of these involve the expenditure of large sums of money, their advice should not be given before a collective discussion with their colleagues, and a full consideration of all the circumstances.

In the past there was no systematized relation between balneology and general medicine, and spa treatment has been regarded as quackery. Even in the columns of a leading medical journal its claims were not long ago entirely repudiated, and change alone was stated to be the sole agent in the cures. It is a significant fact that whilst electro-therapy, the most recent therapeutic agent, is represented by a special section at the British Medical Association, the oldest of all these, balneo-therapy, finds no place in its proceedings.

Recent researches have thrown fresh light on the hitherto obscure action of mineral waters. The study of the passage of electric currents through rarefied gases had for some time attracted the attention of investigators. But the discovery of radium, and that it gave off emanations analogous to the rays produced by the passage of those currents, was so important and far-reaching as to lead to a revision of ideas in physics and chemistry. This event gave a great impetus to investigators in all directions, and active emanations were found in mineral waters that before had been classified as indifferent. A new conception was given as to the cause and physiological action of this class of mineral springs, and they will probably be reclassified.

Many surmises were made as to the mode of action of radio-emanations, but the results obtained by Professor His (of Berlin), published recently,[1] have given us some data as to their action in one direction—i.e., the lungs, but it is probable other effects will be disclosed later. The clinical evidence in many of Professor His's cases was similar to that obtained by treatment with certain mineral waters, and we may now look forward with hope that in the near future some solid and scientific basis will underlie the treatment of disease by mineral waters of this kind.

That there is a steady experience of relief and cure obtained at these spas is certain, and the discovery of a real therapeutic agent gives great satisfaction to all connected with them. In a recent examination as

[1] "Studium über Radiumemanation," *Veröffentl. d. balneol. Gesellsch. in Berl.*, Berl. u. Wien, 1910, p. 92.

to the average result of treatment in the Devonshire Hospital, Buxton (containing 300 beds), since its foundation in 1858, over 87 per cent. of the cases treated were returned as improved or cured. This is a striking fact, and the results are obtained by the patients reporting their condition to the Hospital six weeks after their discharge.

With the encouragement of a new and powerful element in their waters, no doubt spa physicians and authorities will not relax their efforts for further investigations, and outlay will be required. Quantitative analyses must be obtained so that accurate doses can be given; space must be allotted for emanation rooms, and great care and ingenuity are required throughout. It is well known that spa waters, if taken in excess, produce nausea and headache. On the other hand, stronger doses of certain ingredients than are to be found in ordinary doses of these waters may be required. This should present no difficulty. With large quantities flowing to waste, the authorities can collect and reinforce the waters to any extent when necessary. It is obvious that the natural springs, enriched as they may be with so many powerful elements, such as radium, argon, helium, &c., must if so reinforced, be infinitely more beneficial than any artificial radio-emanation waters that can be placed on the market. By such a course spas will be able successfully to compete with outside competition, and thus the interests of the ratepayers, who are so largely called upon, to provide the necessary expenses for the pump-rooms and bathing establishments, besides the upkeep of the numerous grounds and other responsibilities incidental to entertaining visitors, could be protected.

It will be seen that there are many problems connected with spas, both medical and civic, which call for constant care and watchfulness. The wide and complicated nature of arthritic diseases alone forms a field for research almost interminable, but the patient gleaning and gathering of facts must in time clear away most of the difficulties. Where clinical evidence is available it should be made the most of. At the Devonshire Hospital, where 3,000 to 4,000 patients are treated annually for these complaints, a clinical pathologist has been appointed, so that the large amount of material from so great a number of cases may be fully examined and classified.

I cannot close to-night without recognizing the valuable help given by this Section to balneology, for it has not only succeeded in bringing together and uniting the various interests that pertain to it, but forms a central authority to which the whole of the country can look. The work done is all the more valuable when one remembers that, unlike

France and Germany, balneology receives no support or recognition from the Government, neither do the Universities interest themselves in the teaching of balneo-therapy.

After his address the President invited Dr. Gustave Monod, of Vichy, to explain the purport of his mission to this country.

Dr. MONOD, in responding, asked if it were not time for the whole spa question to be placed on an international basis. The more he saw of our health resorts the more astonished he was that they should be entirely unknown to the profession in France. It was, no doubt, their fault ; but was it their fault only ? In a few weeks' time his report would be issued, and he trusted that this might be of service in forwarding knowledge of the climatic resources of Great Britain. He did not think it good policy to build a wall round their spas ; for such a wall, though perhaps hindering the exit of a few compatriots, might well prevent the entrance of a greater number of foreign invalids. There were many of his countrymen for whom a complete change of surroundings would prove of the utmost advantage, and who would find in our country resources not available at home. For the most part they were people in the higher social strata—people who were familiar with our sports and language.

Balneological and Climatological Section.

December 13, 1911.

Mr. G. H. THOMPSON, President of the Section, in the Chair.

The Treatment of Gastro-hepatic Dyspepsia at Vichy, Carlsbad, and Cheltenham.

By GUSTAVE MONOD, M.D.

MANY a practitioner interested in hydrology must occasionally have felt hesitation when called upon to decide whether a course of spa treatment is appropriate for his patient, and to determine which of many competing stations is the most suitable for that purpose. The problem is, indeed, one of considerable complexity, demanding for its solution an insight, sometimes intuitive, into human temperament, a considerable knowledge of thermal methods, and a sufficient familiarity with climate and topography. In these circumstances it is not surprising that the physician sometimes hesitates, and the patient, even though a typically suitable subject for spa treatment, may leave the consulting-room still in doubt as to its necessity for him, and with the name of the watering-place not definitely settled. So, at any rate, may be interpreted the experience of, I suppose, every spa practitioner. He finds that a notable proportion of his patients arrive without definite medical prescription, and possessed with notions, hazy or erroneous, in matters which *we* deem to be of capital importance. Have I not myself encountered at Vichy a Scotch lady sent there by her doctor to enjoy the "mountain scenery"; and did I not in England meet two medical men who placed that famous resort in Germany? An unconscious tribute to Vichy, perhaps; for in the bad old times now passing away, and under influences now losing force, no good thing thermal could come out of France.

Every important spa has inherited traditions, some of which remain fixed while others are modified under the influence of scientific experiment, though there is ever an inclination towards change at the bidding

of mere conjecture. Nevertheless, a solid substratum remains, and evolution is based in the main on clinical observation. It may in fact be said that each spa is a separate school of specialized medicine. But in the literature emanating from these resorts it is difficult to winnow the wheat from the chaff—to distinguish sober clinical fact from fictitious *réclame*. More difficult still is it to form accurate deductions even from honest analyses; for mineral waters must be regarded as dynamic entities, and the only trustworthy guide to their use is clinical experience, which cannot be learnt from any book. If, therefore, I bring together in this paper three spas in many ways so dissimilar as Cheltenham, Vichy, and Carlsbad, it is solely because patients of the same category may find at each of them the treatment which their condition demands.

Take the list of indications displayed by every station, we shall assuredly find, in bold relief, the word—dyspepsia. And justly so; for should not dyspepsia really be written in the plural, and cannot hydrology in one way or another adapt itself to almost every conceivable requirement? Nevertheless, to alkaline and alkaline sulphated waters preference would commonly be accorded on theoretical grounds alone, and it is sufficiently well known that clinical results have fully justified such selection.

If we were asked for the formula of what is called a "digestive" water, it is more than probable that we should, in the first instance, think of a solution of sodium bicarbonate of medium strength, say five parts per mille; while influenced by recent research we should render the solution isotonic. As for the temperature, it would doubtless be that of the body—perhaps a little higher. The outcome of these efforts would be the manufacture of the water which gushes forth near the banks of the River Allier; or to be more precise, we should have a close imitation, lacking, however, as we hold, that impalpable quality (the *nescio quid divinum* of hydrologists) which gives to natural mineral waters their life and thus their unique merit. To offer a more prosaic conjecture, these qualities may be due to minimal quantities of the other salts in solution, to continuous synthesis, as described by Gautier, to radio-activity—or to some kindred property to be discovered by the Curies of the future. However this may turn out to be, for proof that mineral waters created by Nature herself differ from products of the chemist's laboratory, I appeal to the testimony of thousands of patients whose united experience dates back for centuries: for to quote President Lincoln's quaint and pithy phrase, "though you may fool some of the people part of the time, you can't fool all of the people all of the time."

I must now ask you to betake yourselves in imagination to the centre

of France, to a gently undulating country at the foot of the last spurs of the volcanic mountains of Auvergne. Under the soil there will be found a sheet of bicarbonated water, which has come from far, and doubtless from great depths, emerging here and there in springs, hotter or cooler, and more or less charged with divers salts according to the speed of the current or the strata traversed in its underground wandering. In the area of this immense basin you have only to put down a drill to tap a fresh spring, while the composition of the waters will display but minor differences. The existing springs, most of them in private hands, number about eighty. But, of those which have brought world-wide fame to Vichy only five or six need be mentioned—the *Sources de l'Etat*—and that because this group includes the most important of the thermal springs, and that every guarantee is given of honest, hygienic, and scientific management. Closely akin to each other, they present some differences, chemical and thermal, capable of definition, but also as we believe differences eluding the ken of the chemist and physicist.

These springs may be classified according to their temperature thus:—

Cold	*Celestins*, 57·2° F.
Tepid	... *Lucas*, 84·2° F.
Warm, less than body heat	... *Hôpital*, 93·2° F.
Hot, greater than body heat	*Grande Grille*, 107·6° F.
,, ,, ,, ...	... *Chomel*, 111·2° F.
Very hot	... *Dôme*, 141·8° F.

Although, as already mentioned, these waters differ but slightly in chemical composition, their effects are often widely dissimilar, and it is a matter of daily experience to find instances in which while one is well borne its neighbour cannot be tolerated.

The following indications are determined by empiricism: Celestins, affections of the urinary tract; Hôpital, diseases of the stomach; Grande Grille, biliary lithiasis, congestion of the abdominal viscera, diabetes, and gout; Chomel occupies a position intermediate between the two last mentioned; Lucas, the gouty dermatoses.

Such an ascription as the foregoing possesses a merely relative value. In practice we are guided solely by clinical findings. We regard all these springs, not as table waters, but as active medicaments, to be taken usually in minimal quantities, and always in measured doses; three glasses (1 pint) being the average amount ingested daily, and that quantity is reached by fractional increases. Baths, massage, and the numerous appliances of our well-known establishment for physiotherapy are simply adjuvants to the treatment.

Without burdening this paper with a complete list of the sources, with their analyses, you may now have an inkling of our conception of the springs, arranged as it were as a therapeutic step-ladder or scale—the famous *gamme* at which our British confrères are tempted to smile.

Special Vichy literature is, alas, so overgrown with trivialities that it may well give the impression of poverty. I laid stress just now on clinical aspects, to which my remarks will be restricted, and in doing so I desire to disclaim all attempt at originality. The foundations of Vichy practice were well and truly laid fifty years ago. They are immovable. Additions may be suggested, fresh explanations offered, but the main structure, consolidated by clinical observation, must ever remain unshaken. It is not too much to say that upon the work of the great Vichy masters—Claude Fouët (1686), J. F. Chomel (1734), Charles Petit (1842), and Max Durand-Fardel (1851)—all subsequent writers have based their observations; though in their pamphlets there is evidence of undue haste in the omission of tables of references. In this connexion even the authors of that excellent and original work "Clinique hydrologique" cannot escape from reproach. But they, at any rate, understand clearly that the reactions taking place in the living stomach differ radically from those of the test-tube, and that neither the classification nor the treatment of dyspepsia can be placed upon a purely chemical basis. Following the classification of Mathieu, they divide primary dyspepsias into three principal types. For each of these types Vichy offers resources, which while meriting the attention of the profession as a whole, should be studied with the closest care by physicians whose opinions are sought when thermal and other methods are to be weighed against each other in the balance. For the physician should be able to reply to these two questions: What will be the nature of the treatment adopted; what the ultimate result to the patient?

Let us then take three patients affected by gastric disorders of common type:—

(1) The first one states that ingestion of food is always followed by painful sensations. After a few mouthfuls his stomach is distended, his face flushes, and he becomes sleepy, but in an hour or two all is well again. If you examine him at the height of the attack you may be surprised to find how scanty are the physical signs. Analysis of the gastric juice discloses hypochlorhydria—less commonly hyperchlorhydria. Such is the clinical picture described by Mathieu as *sensory motor*. Patients of this category are suffering from overwork of the stomach, from excessive feeding, or, often, from abuse of digestive drugs. One of our Vichy springs, Hôpital, taken in small doses, meets both these

indications of the case—stimulation of the motor power of the organ, and sedation for the over-sensitive mucosa. Add to that a few baths of indifferent temperature, and you have a sufficient idea of the treatment to which your patient will be subjected. He will return to your care if not completely cured at any rate greatly benefited—or, to say the least, satisfied that something substantial has been done for him.

(2) Patients belonging to the second category, those suffering from pain some time after meals, come within the province of two other springs, Chomel and Grande Grille. Such patients are classed as "hypersthenic dyspeptics," and to Soupault we owe a masterly description of their sufferings—"the pyloric syndrome." The pains, retrosternal in position, are acute, and often followed by vomiting which affords instant relief. They are sometimes purely spasmodic and of nervous origin, or they may be reflex and due to actual gastritis. The sodium bicarbonate water relieves as if by magic, an effect attributed by some writers simply to chemical saturation, though Binet's analyses tend to prove that it is to be referred to the antispasmodic action of hot gaseous water on the pre-pyloric mucosa. To those who believe, with Hayem, that these disturbances imply a functional defect of the cells of the mucosa, Linossier offers the explanation that Vichy treatment, while stimulating the glands remaining healthy, improves the condition of those which have become impaired, and may even arrest degenerative processes.

(3) The third category of patients includes those whose suffering may be said to be wholly indefinite; appearing at any moment, it is apt to be of considerable duration—characteristics serving, according to Mathieu, to establish the essentially nervous origin of the affection. The invalid, often a woman, insists that pains assail her day and night, and that with an added sense of distension and loss of appetite she has completely lost heart. Sometimes she imposes upon herself a regimen as fanciful as it is irrational; at others she passes into a state of inanition. Arterial tension falls. There is stomach splashing, and the organ is uniformly tender on pressure. Examination of the contents shows sometimes hypo- sometimes hyper-chlorhydria, the lack of precision in all these signs establishing the diagnosis of *nervous dyspepsia*. What resources does Vichy offer in the presence of this symptom-complex? Apart from the advantages of change of environment and of habits, the patient will receive the utmost benefit from the sedative influence of a course of tepid baths, with the times of immersion prolonged. You may well leave to the spa physician the choice of the different waters and the manner of their application, both of which he

will arrive at by tentative trial. He may call to his aid the excito-secretory action of a cold spring, or the soothing and eupeptic effects of the Hôpital. Though the treatment is essentially opportunist, such cases furnish some of the most striking triumphs of thermal treatment.

For dyspepsias associated with organic lesions, such as cancer or spreading ulceration, thermal methods are absolutely contra-indicated. Vichy, like Carlsbad, possesses every appliance for rapid diagnosis. Do not send us patients whose treatment will perforce undergo sudden interruption.

Of the secondary dyspepsias—a vast pathological domain still involved in confusion—I must be content with a bare enumeration of those for which spa treatment is efficacious. These are the dyspepsias secondary to ptosis (Glénard's disease), alcoholic or malarial poisoning, obesity, diabetes or gout, and finally, but above all, *biliary lithiasis.*

In the whole chapter of mineral water therapeutics this last affection holds one of the most important positions. It is without doubt one of the chief specialities of the two leading French and Austrian inland watering-places. In both of them, perhaps chiefly at Vichy, clinicians have laid stress on the successes obtained. In calculosis of the gall-bladder the combination of a diathetic and an infective element demands the selection of a spa whose waters both stimulate nutrition and at the same time exercise a specific influence on biliary secretion. Full explanation of the success of Vichy treatment cannot as yet be given, but the results, open to the investigation of all, are decisive.

Let me quote the opinion of Huchard: "Admitting that sodium bicarbonate inclines the bile to alkalinity, a condition necessary for the dissolution of cholesterin, which forms the chief bulk of many of these calculi, there is some other factor at work. We know that biliary lithiasis is mainly of infective origin, and we believe that the influence of Vichy treatment is more potent than that of Carlsbad in combating infection."

Let us now consider the migration of calculi, and first under conditions of asepsis. The primary aim of the treatment is to secure what Gilbert calls "gall-bladder tolerance," and to favour circulation and biliary secretion. We must by no means wait until chronicity has brought in its train sclerosis and finally atrophy of the receptacle. The physician must preserve a light touch and refrain from provoking even the semblance of a crisis, for the object in view is less to favour the passage of a calculus (if one is present) than to soothe the irritation of the extra-hepatic ducts. For such cases also the milder Vichy waters are evidently superior to the more strenuous springs of Carlsbad. If

the migration takes place under septic conditions the prudent surgeon, after operative interference, will refer his patient to the spa physician, that the risk of recurrence may be lessened.

I have no intention of referring to hepatic affections in which the indications for spa treatment are less peremptory, but I must say a few words about simple *congestion of the liver*—a condition, according to my observation, commoner among English patients than among any other. Whether it is a clinical entity I know not; but certainly there is no better description of the syndrome than that to be found in the letters on Vichy by the late Max Durand-Fardel, one of the most masterly of French clinicians. Simple engorgement is not of necessity a grave condition, but it is notably chronic and resistant to treatment, and, moreover, it may be precursory to changes of more serious import. Of the effects of spa treatment I do not wish to say more than that in the majority of cases benefit is tangible. Even if there is not always considerable reduction in the size of the organ, it is seldom that the constitutional condition is not greatly improved. Can more be claimed for other therapeutic measures?

Let us pass on to Carlsbad. The two great rival spas are closely linked in the mind of the profession, though a glance at the analysis of the waters would scarcely lead one to suppose them competitors. The Vichy springs belong to the simple bicarbonated group, while Carlsbad water, though containing the bicarbonate, is more complex, chloride and sulphate of sodium being notable constituents. The one water we speak of as *gastro-hepatic;* the other, *gastro-intestinal.* The one is gentle in action, the other energetic. Our treatment is largely opportunist, that of our competitors perhaps more rigidly fixed. Gautrelet has endeavoured to show that in addition to the chemical difference there is one that is physiological. He maintains that Carlsbad water influences defects of nutrition only in the assimilative phase, and fails to deal with their fundamental cause—the "diathesis of hyperacidity." On the other hand, Vichy waters influence not only errors of nutrition in both of their phases, but, in addition, their joint underlying cause—hyperacidity. Nevertheless, if results were compared, as Renan says, "from the standpoint of Sirius," they would doubtless be found to be closely approximate.

Internal treatment at the two resorts differs within narrow limits, the average amount of mineral water ingested being nearly the same. At Carlsbad it is taken in the morning and consumed more rapidly, while at Vichy the doses are divided, and spread over the course of the day. They are drunk at Vichy at their natural temperature, whereas

at Carlsbad they are cooled down. There is some difference in the balneotherapeutic resources of the two stations. Both possess mineral water baths, as a matter of course, but Carlsbad has in addition peat baths, which are not as yet known at Vichy. We, on the other hand, employ a form of under-water massage which, I believe, in England is called the Vichy douche, and in addition, a true under-water douche known at Harrogate as "sub-massive," while at Bath it is even more quaintly termed the "wet douche."

The whole question has been thus stated by Dr. Leonard Williams: "In a general way it may be said that a course at Vichy is less severe than at Carlsbad, so that of the two, the former is the better suited to women, and to men of nervous organization. A chemical difference, which in view of Widal's work would seem to be of some considerable importance, is the fact that Carlsbad water contains chloride of sodium to the amount of 2·5 grm. per mille, whereas at Vichy this salt is present in small quantities only—viz., 0·05 per mille. So far as the accessories to the drinking of the waters are concerned, such as baths, massage, electrical and mechanical appliances, Vichy certainly bears the palm, the establishment at this spa being the finest and most luxuriously equipped in the world."

To this may be added the observations of Dr. Neville Wood: "Testing in turn the official springs at Vichy (except Mesdames) they seemed to possess one action in common, that of improving the appetite and increasing digestive competence. This sequence would seem to be a common experience there, and one at once explaining and justifying the fact that at Vichy greater dietetic freedom is accorded to patients than is considered prudent at most of the spas where kindred disorders are treated. True, the ingestion of mineral water at Carlsbad seemed to have no irritative influence, but certainly it failed to whet the appetite and so rendered less irksome the restrictions of the trial regime that had been suggested to me. The experience of invalids coincided with my own. Still, it would be imprudent to attribute the entire difference to the action of the waters, for at the French spa dishes which would be labelled 'rich' by Britons are so admirably served that the 'appetite juice' of even a moderately competent stomach might fairly be trusted to deal with them; while at Carlsbad the simpler varieties of fare were often the more naturally appetizing." According to the same witness "patients at our spa are protected from morbid introspection—they are reminded more often of health than of disease."

And again: "Carlsbad is the type of a strenuous spa, its very atmosphere seeming charged with intensity. No invalid should go there

unless he intends to devote himself consistently to the treatment, for otherwise he might feel, and be felt to be, rather in the way."

Thus, in advising your patients you will often find the deciding factor in accessory circumstances. There is an element not to be weighed in the chemical balance, a psychical element evading clinical observation—the temperament of your patient, and his international sympathies. There is also truth in the famous French line which I venture to render—"The manner of giving counts more than the gifts." Dr. Huggard, whose recent death we so much deplore, has pointed out that mineral waters do not exhaust the therapeutic efficacy of a spa. Add to this that at most Continental spas the period of election for British invalids is the early or the late weeks of summer.

But when armed with all these data, you are on the point of issuing a verdict for Vichy, or for Carlsbad—well—perhaps you will send the patient to Cheltenham after all!

You must allow me, however, to adhere to the Continental point of view—call it bias if you will. You have at Cheltenham waters unique after their kind in England. If they were hot they might, as we conjecture, unite the merits of Vichy with those of Carlsbad.

What account do clinicians give us of Cheltenham? Dr. Archibald Garrod ("Climates and Baths of Great Britain") writes: "In order to give any information as to the therapeutic effects of the Cheltenham waters, it would be necessary to quote the writings of older physicians who practised in the palmy days of the spa, and this I do not propose to do." Sir Hermann Weber is little more explicit. He says that Cheltenham has been "looked on" (note the past tense) as a special resort for persons suffering from prolonged residence in hot climates and also for many with chronic gouty complaints. More hopefully, Dr. Neville Wood remarks: "For many Anglo-Indians requiring eliminatory treatment Cheltenham is superior to Carlsbad in all but prestige and hotel accommodation. . . . . Cheltenham has, in being available at all seasons, an important advantage over its Continental competitors, which are at their best only in the early part of the summer." It is evident, then, that this writer also is not prepared to lay down precise indications for the use of the waters in gastro-hepatic disease, for from the context it appears that he has in mind persons suffering from minor disturbances incidental to protracted sojourn in tropical climates.

But what do the practitioners of Cheltenham (a town originally made by its mineral waters) claim for these promising springs? Examining a record of their writings, I notice that they never refer to Cheltenham waters. Here, then, is a resort, beautifully situated, attractively laid out,

enjoying a genial climate, possessing interesting traditions, and endowed with varied mineral waters of the highest promise; yet failing in that modern essential for a serious spa—precise clinical indications for the employment of its hydrological resources. Cheltenham awaits her prophet!

Note.—Since writing this paper my attention has been called to a pamphlet on Cheltenham waters, which, though undated and unsigned, has, I am informed, been issued with the approval of resident medical practitioners. Though leaving something to be desired on the score of precision it is only fair to quote verbatim the principal paragraph relating to indications, which runs as follows:—

In what diseases then are the Cheltenham Waters beneficial? Primarily in that ever-increasing class of auto-intoxications in which poisons are manufactured in the alimentary tract as the result of faulty digestion, fermentation and putrefaction. In such cases the waters of the Lansdown Terrace or Chadnor Villa Wells effect lavage of the entire gastro-intestinal tract, washing away bacteria, toxins, mucus, and fermenting food. Thus by their eliminative action they favourably affect diseases caused by faulty or imperfect metabolism such as gout, and diabetes, and also help the liver in its onerous duties of arresting and converting into harmless products the poisons introduced into the alimentary canal or manufactured there as the result of bacterial activity. Clinical evidence proves these waters to be beneficial to those who suffer from the deleterious effects of residence in hot climates, and in the usually tedious convalescence following attacks of dysentery, Malta fever, or sprue. They are also very serviceable in eczema and skin affections of a gouty origin. In some kidney affections they have been found useful as well as in cases of chronic rheumatism.

BIBLIOGRAPHY.

Baraduc (and others). "Clinique Hydrologique," Par., 1909.

Durand-Fardel, Max. "Lettres médicales sur Vichy," Vichy, 1887.

Durand-Fardel, Max et R. "Carlsbad et Vichy," Par., 1895.

Garrod, Archibald. "Climates and Baths of Great Britain," Lond., 1895, i, pp. 589-92.

Gautrelet. "De la prétendue identité entre Carlsbad et Vichy," Par., 1898.

Gilbert et Carnot. (Thérapeutique.) "Crénothérapie," Par., 1910.

Glénard. "De la cure de Vichy," Par., 1900.

Hayem. "Leçons de thérapeutique," Par., 1901.

Huchard. "La thérapeutique en 20 médicaments," Par., 1910.

Huggard, William. "A Handbook of Climatic Treatment," Lond., 1906.

Linossier. "Hygiène du dyspeptique," Par., 1908.

Linossier et Lemoine. "Action du bicarbonate de soude sur la sécrétion gastrique," *Bull. Acad. de Méd.*, Par., 1908, 3me sér., lix, pp. 461-74.

Parturier. "Les sources de Carlsbad et de Vichy," Par., 1909.

Weber, Sir Hermann. "Balneotherapy and Climatotherapy," London, 1907.

Williams, Leonard. "Minor Maladies," London, 1909.

Wood, Neville. "Spa Treatment," London, 1910; "British Health Resorts for Foreign Invalids," London, 1911.

Balneological and Climatological Section.

January 25, 1912.

Mr. G. H. Thompson, President of the Section, in the Chair.

Bubonic Plague.

By F. M. Sandwith, M.D.

It would, I think, be unbecoming to discuss bubonic plague before members of this Section without making some reference to our change of views in regard to climatic and seasonal influence on the disease.

Until a century ago it was universally accepted that the potential territory of plague never extended to the Tropics; this opinion was based upon the experience of several centuries that plague often occurred in Egypt but had never been known to spread to the adjacent Sudan or to Abyssinia. It is still mainly a disease of temperate and sub-tropical countries, flourishing best when the temperature of the air is neither very hot nor very cold. The dependence of epidemics of plague apparently upon season and weather is also founded upon definite experience in Egypt.

More than three hundred years ago Prosper Alpinus, the Italian, laid down the law that epidemic plague appeared in Egypt in the autumn and died out in June, and this has, with some limitations, been confirmed up to the present time. Alpinus, during three years' residence in Egypt, also noticed that when the plague died down in June, the property in infected houses suddenly lost all powers of communicating the disease, so that health and well-being were at once restored to the city. Many more modern travellers reported the interesting fact that the natives of the better class, in spite of their strong belief in the contagious power of plague, suddenly relaxed their rules of house quarantine after June 24

(St. John's Day), when they exposed in the market-place the bedding and clothes of the thousands who had recently died from plague, and though the purchasers at once wore the garments, no spread of the disease occurred. Similar occurrences have been noted in London, Marseilles, Naples, Hong Kong and elsewhere, for at the end of an epidemic the inhabitants have often, with impunity, flocked back to the deserted houses, slept in infected beds, and worn the clothes of the plague victims. Yet it was noted that in times of endemic plague the respite was only temporary, for when the plague reappeared it was apt to do so in the houses formerly affected.

Exactly twelve years ago Mr. Baldwin Latham, the sanitary engineer, tried to solve this mystery by contending that the incidence of plague was dependent upon exhalations from the ground, and that the decline of the disease corresponded with the time when the ground exhalations were least. But, as Professor Simpson wrote in 1905, "If the vapours have any influence, it is probably in the direction of favouring a condition productive of susceptibility of the organism in man or the lower animals, or in both."[1]

Light was not thrown upon the problem until the researches of Dr. F. Tidswell, of Sydney, and Major W. G. Liston, I.M.S., eventually proved that the seasonal influence of epidemic bubonic plague was due primarily not to changes of climate, but to the coincidence of the epidemic season with the period of greatest prevalence of the flea carrier of the *Bacillus pestis*.

It is only during the last few years that attention has been paid by medical men and entomologists to the habits and customs of fleas, but it has long been known that there is in some warm countries a very definite flea season. In Cairo that season of irritation to human beings is from February to June, which helps to explain the temporary decline of bubonic plague shortly after June 24.

The rat-flea is met with in the Sudan, but the human flea has not yet been reported from there. The human flea (*Pulex irritans*) has practically become cosmopolitan, but the tropical countries of the Eastern hemisphere are only troubled by it if they have intercourse with Europeans.[2] The rat-flea (*Læmopsylla cheopis*) occurs in all warm climates, and is the chief transmitter there of bubonic plague from rat to rat and from rat to man. Its true home appears to be the Nile

[1] "A Treatise on Plague," Camb.

[2] Jordan and Rothschild, "Parasitology," 1908.

valley, where it occurs commonly on various hosts. The factors considered to-day necessary for the spread of bubonic plague are thus summed up by the Indian Plague Commission:—

The rise of the rat epizootic, and, in consequence, of the human epidemic, depends upon:—

(1) A suitable mean temperature somewhat below 85° F., and in general over 50° F.

(2) A sufficient number of susceptible rats.

(3) A sufficient number of rat-fleas.

The fall of the rat epizootic, and, in consequence, of the human epidemic, is determined by some or all of the following factors:—

(1) A high mean temperature, 85° F., and above.

(2) A diminution of the total number of rats and an increase in the proportion of immune to susceptible animals.

(3) A diminution in the number of rat-fleas.

Dr. Sandwith then described, by means of the epidiascope, the natural history of the bubonic plague in India, remarking that since the black, or chief plague-bearing, rats have been driven from this country by the common or brown rats, the danger of an extensive epidemic was to that extent diminished. He also pointed out that our usually cool temperature and our relative shortage of fleas were in favour of our not having again a widespread outbreak of bubonic plague in England.

DISCUSSION.

The PRESIDENT (Mr. G. H. Thompson) remarked how much pleasure Dr. Sandwith's instructive and interesting paper had given to all present. The epidiascope had revealed the salient features of the disease and its environment. In connexion with the burial pits in London, he mentioned the historical outbreak in the village of Eyam in Derbyshire in 1666, which was introduced through a box of clothing from London. The vicar of the parish drew a cordon round the village so that the disease was strictly confined to it, although the population was decimated by its ravages. Tradition asserted that an accidental exhumation of the clothes ten years later caused the death of the people engaged in it.

Dr. ABRAHAM thanked Dr. Sandwith for his extremely lucid and interesting exposition of the subject. Dr. Sandwith's remark that, in one case, the fleas on a man's garment were so numerous that they had to be brushed off, reminded him of what he once saw in Jamaica; on going over an empty house with a

friend who was in white flannels, he suddenly observed that his friend's trousers were of a dark grey colour, soon found to be due to enormous swarms of fleas. Regarding the climbing power of the brown rat, he had seen them climbing trees in Ireland, and running up the woodwork of a barn. He remarked that the important rôle that the lower animals played in the spread of disease was one of the most notable advances in knowledge in recent times, and he believed that the number of diseases so spread would be recognized to be greater in the near future. He instanced leprosy, the bacillus of which would probably be found to be identical with the acid-fast bacillus discovered by Dr. Dean, some few years ago, in rats. Dr. Bayon was now working upon this subject, and his observations so far tended to prove the truth of this view.

Dr. CLIPPINGDALE expressed his grateful appreciation of Dr. Sandwith's instructive and interesting paper. Referring to the matter mentioned by the President—the outbreak of plague at Eyam in 1666—he had visited the place and was interested to learn that the vicar had not only placed a cordon round the affected area, but outside this cordon had placed a tank of water into which money was placed to pay the people who brought food for the cloistered, plague-stricken inhabitants, thus showing on the part of the reverend gentleman a commendable knowledge of the requirements of sanitation.

Mr. W. J. MIDELTON said he thought he had heard Dr. Sandwith say that pus organisms killed off plague bacilli in the buboes; if so, did not this raise the question whether pus organisms might be beneficent under certain circumstances?

Dr. LIEVEN (Aix-la-Chapelle) mentioned how important was the study of zoology for employment in practical bacteriology, giving as an example the *Bacillus typhomurium* (mouse typhoid) discovered by Loeffler, which attacked only certain species of mice; it was luckily the species most commonly found for which it had a predilection.

Balneological and Climatological Section.

March 6, 1912.

Mr. G. H. THOMPSON, President of the Section, in the Chair.

The Spa Treatment of Neuritis.

By WM. ARMSTRONG (Buxton).

NEURITIS is an ailment very interesting to the spa practitioner: its frequency, its refractory behaviour under treatment, and the peculiarly wearing nature of its pain, marking it out for special study and attention. That it is more frequent than formerly is certain; for allowing on the one hand for the fact that we are sometimes apt to call cases of simple neuralgia by the more high-sounding title of neuritis, and on the other for the modern opinion that in some cases neuritis is rather a fibrositis, there are still among the patients of all spa practitioners a considerable number who have undoubted neuritis, as I will try to describe it. It seems probable that the acknowledged shortage in the number of cases of acute articular gout (a plus formation of uric acid being in evidence) is made up for by the presence of many suffering from irregular gout (with a minus uric acid excretion), in whom neuritis makes its appearance; and that, while gout itself is not decreasing, the conditions of modern life, with its ever increasing nerve-strain, have diverted the gouty stream to some considerable extent into the channel of the nervous system.

I will try to record briefly certain differences between neuralgia and neuritis, as, from the point of view of spa treatment, the differentiation is most important. The term neuralgia, in my opinion, should be reserved for those cases in which the pain is usually paroxysmal in character, and in which there is no organic lesion of any part of the

nervous system. In neuralgia there are almost always intervals in the attacks when no pain is felt, and the trouble is usually confined to the outer sheath of the nerve. Neuritis, on the other hand, is an inflammation of the actual nerve itself. Often commencing as a peri-neuritis, it involves the interstitial connective tissue of the nerve, and in a certain number of cases (and these are the most intractable) the actual nerve-fibres become affected with parenchymatous or degenerative neuritis. In peri-neuritis the connective tissue round the nerve is red and swollen and the small blood-vessels of the sheaths are dilated, with or without hæmorrhages. This condition is followed by serous exudation between the sheath and the bundles of nerve-fibres, which, making its way between those fibres, often causes interstitial neuritis, and is sometimes followed by degeneration. In parenchymatous neuritis the nerve-fibres break down and degenerate. This very serious condition may be either primary or secondary to an interstitial neuritis.

The pain of neuritis is characteristic; it is boring or burning, being often likened to "a large toothache"; it is, as a rule, constant; is markedly increased by movement of any kind, especially by jerks or jars; and acute exacerbations are frequent in the early morning hours from 1 to 5 a.m. There is marked tenderness where the nerve leaves the spine and also along its whole course. At first there are paræsthesia and hyperæsthesia, later anæsthesia, and atrophy of the muscles supplied by the affected nerves. Electrical excitability is at first increased, being afterwards diminished. As the disease progresses changes take place in the nutrition of muscles, skin, nails and joints.

Even from this very fragmentary and incomplete résumé it will be evident that we are dealing with an ailment of a serious, far-reaching, and important character. The onset of this disease is frequently preceded by a lowered condition of bodily or nerve health, and worry and anxiety are often predisposing causes.

Neuritis may be divided into two groups: (1) multiple peripheral neuritis, and (2) mono-neuritis.

Multiple peripheral neuritis is usually caused by the action of toxic products or micro-organisms on the general nervous system. The following, either alone or in combination, have been described as causatives: alcohol, lead, arsenic, carbon disulphide, carbon monoxide, the poisons of diphtheria and certain other infectious diseases, tropical infections—such as beri-beri—diabetes, syphilis, gonorrhœa, pregnancy, gout, and chronic rheumatism. Where no cause is obvious the term idiopathic has been used.

Mono-neuritis may be divided into (*a*) acute, and (*b*) subacute and chronic. The chief causes are: (1) gout; (2) rheumatism; (3) diabetes; (4) arthritis; (5) auto-intoxication—(i) gastro-duodenal, (ii) intestinal, (iii) from defective elimination of lungs, skin and kidneys; (6) chill (often to neck), especially through motoring (open landaulettes being the most dangerous); (7) injury to nerves; (8) extension of inflammation of other structures to nerves; (9) compression of nerves by local induration, most frequent at point of issue from spine, or from local pressure as in metatarsalgia; (10) pressure of growths, malignant or otherwise.

With reference to the spa treatment of neuritis, I would like to emphasize the following points:—

(1) The need in all cases for rest, general, local, or physiological, as may be indicated.

(2) The avoidance of measures which jar, jerk, press upon, or irritate the tender structures, such as violent massage or movements, or over-strong currents of electricity.

(3) The necessity for using strengthening rather than lowering treatment.

(4) *And more than all*, a determined attempt to formulate the actual condition of the affected nerve or nerves, especially the amount and depth of the inflammation present, for on this both the prescription of the spa treatment and its success depends. As a rule spa practitioners are inclined to favour definite treatment. Many excellent methods of treatment are available of which patients are naturally anxious to make immediate use. The visit to the spa is only of three or four weeks' duration, and during this time some amusement, sight-seeing, and holiday are expected. It is therefore very difficult to have to suggest to a patient rest in bed for one or two weeks (which could be taken at home at a nominal cost), or the wearing of a splint up to the axilla for a neuritis of the sciatic, or of an appliance to give absolute rest in neuritis of the cervico-brachial nerves. Few of us can have failed to notice how in acute and subacute cases, where no relief is being obtained from treatment, a fractured fibula or an attack of pneumonia (or other illness necessitating confinement to bed) has given speedy relief to the nerve suffering.

The treatment of multiple peripheral neuritis embraces:—

(1) The discovery, stoppage, and avoidance of the exciting cause or causes.

(2) Rest—(*a*) general or (*b*) local. Many of these patients require to be kept in bed, with avoidance of noise, worry and anxiety, often being separated from friends and relations.

(3) The elimination of toxins or any other poisons present.

(4) The building up of the nervous system generally.

(5) The drinking of the various natural waters on account of their eliminating properties.

(6) The stimulation of skin action. This is most effectively carried out by the use of superheated air. The Greville method, so much used in Harrogate, Bath and Buxton, possesses great advantages, as it can be given in bed without moving the patient at all; it is not nearly so exhausting as many other methods, and is practically free from the danger of scorching the skin.

(7) Application to the whole spine of hot packs of fango or radio-active earth. The latter has given much the better results, as it contains much actinium and does not lose (as is so often the case with fango) its specific radio-activity through carriage. Later, as the severity of the pain decreases, baths of warm mineral water may be used (with or without oxygen), with a very careful use of the hot *under-water* douche to the spine and to the affected nerve; strong douching being contra-indicated as likely to increase pain.

Most valuable is ionization with solution (3 per cent.) of sodium salicylate or of cocaine (5 to 10 per cent.) with a constant current of low strength (3 to 15 ma.), followed in the latter case by light applications of the electric cautery over any specially tender spinal points, the cocaine ionization acting as a local anæsthetic.

The constant current water or Schnee bath, high frequency, wave and static applications are useful in the more chronic stages, but should always be low in strength. The Plombières douche, where indicated, is valuable. Massage should be used with the greatest care and gentleness, and only when the acute symptoms have disappeared. The injection of strychnine nitras, $\frac{1}{30}$ gr. to $\frac{1}{10}$ gr. once daily, is valuable in many cases.

Mono-neuritis is much the more common form met with in spa practice. It is of the utmost importance as regards treatment to attempt to decide the exact condition of the affected nerves as regards irritability.

Acute Mono-neuritis.—Absolute rest of the affected part is indicated. Elimination of toxic products may be promoted by drinking the various mineral waters—through the bowels and liver, by waters such as Harrogate, Llandrindod and Strathpeffer; through the kidneys, lungs and

skin, by the radio-active waters of Bath and Buxton. In acute mono-neuritis strong douching and massage are distinctly harmful. Super-heated air by the Greville method and packs of radio-active earth and fango are useful. Applications of the electric cautery over the exit of the inflamed nerves; the tenderness being found in the cervical region when the brachial plexus is involved; in the upper dorsal when the circumflex and ulnar nerves are implicated; and the lower dorsal, lumbar and sacral, when the nerve supply to the lower extremity is affected.

Ionization of cocaine with the constant current produces more or less skin anæsthesia and diminishes the pain of the electric cautery, the application of which should be made very lightly, the skin not being broken and the parts being covered at once with an antiseptic dusting powder. The application should only be made over the points where tenderness is found on pressure, and may be repeated every two or three days. Blisters over the same points are useful, but they are more painful, cannot be so frequently repeated, and are not so effectual as the electric cautery.

In subacute or chronic cases warm mineral baths with hot under-water douches are helpful, and also water massage by the Aix, Vichy, or Buxton methods, given with greatest care. These may be followed by very gentle passive movements of any stiffening joint, especially the shoulder. Moor or peat baths are often useful.

The Schnee bath with the constant current in most cases, with or without ionization with sodium salicylate or iodine preparations, and in the more chronic the sinusoidal current, static, wave, or high frequency electricity, are all useful as the cases progress toward recovery.

If the urine shows an excess of aromatic sulphates or a high per-centage of indican, skatol, cresol and phenol, the Plombières douche is very valuable; or if there is much gastric atony and splashing, gastric lavage is indicated.

For the general state of the nervous system (which is almost always disturbed and irritable) no remedy has proved so valuable as the oxygen bath. The oxygen is much more effective if gradually produced in the bath at the time of administration than when the bath is charged by oxygen from cylinder. By mixing in the bath sodium perborate and manganese borate, from 35 to 40 pints of oxygen are steadily evolved during fifteen to twenty minutes. It can be added to any mineral water, but is most effective when given with radio-active water, the oxygen and radium emanations mutually increasing each

other's activity. The effect is soothing and tonic, and aids the elimination of waste products.

The question of the radio-activity of many natural waters and of the therapeutic value of this property is exciting much interest and some controversy both here and on the Continent. The work of Lazarus, Saubermann, Deutelmoser, Bergel, Bickel, His, Gudzent and many others, show that by the use of radium emanations dissolved in water, of the radio-oxygen bath, and inhalations of radio-oxygen emanations, the activity of excretion is much increased, especially of uric acid by the kidneys, of urea and butyric acid by the skin, and of carbon dioxide by the lungs.

This should be welcome news to spa practitioners, as it puts on a firm, scientific basis the claims made for, and the results shown by, the various mineral waters which have sometimes been the subject of scepticism, more especially those waters which are wanting in taste or odour, and which have no definite action on the bowels.

DISCUSSION.

The PRESIDENT (Mr. G. H. Thompson) said the subject was very important to every spa physician. It was necessary to differentiate neuritis from cases of painful but less serious forms of neuralgia, otherwise treatment suitable to the latter would produce results profoundly disappointing in cases of neuritis. The great essential in severe and early cases was rest, and it was often difficult to get the patient to concur, as he looked upon his visit to a spa as a sure and certain escape from such restraint. Dr. Armstrong had stated the most approved and successful forms of treatment. Similar treatment of like conditions sometimes gave dissimilar results. In severe cases Dr. Thompson had had good results with a 2 per cent. salicylate ionization. In cases where blistering was used, he had found a dilute solution of formalin very effective in keeping open the part.

Mr. W. J. MIDELTON (Bournemouth) said he had employed the blister followed by savin ointment, and also a form of acupuncture followed by the application of a mixture of croton oil, cantharides, acetic acid and almond oil, in the treatment of neuritis, and quoted cases in which he had obtained marked success by one or other of these methods. He doubted if other methods were superior in any way to the above, and as the result of long experience he had succeeded in devising details by which practically all severe pain and discomfort were eliminated.

Dr. E. SOLLY (Harrogate) said that in his experience many cases of so-called neuritis were primarily due to the condition now usually spoken of as "fibrositis," in which the symptoms were more manifest in the perineural tissue than in the fibrous tissues generally. In these cases treatment should be directed to the injured nerve tissue, and should consist of soothing treatment by rest and local application of heat—whereas in fibrositis affecting the coarser forms of fibrous tissue, forcible massage and stimulating forms of electric treatment were necessary.

Dr. LÉON BLANC (Aix-les-Bains) referred to the distinction between neurasthenic and neuritic pain. He said that as long ago as 1846 Baron D'Espin had pointed out that massage must not be used to the affected part. He advocated the use of the under-water douche after vapour baths, and considered that the radio-active mineral baths and packs were of the greatest value.

Dr. C. W. BUCKLEY (Buxton) said that it was essential to have a clear idea of the pathology of neuritis before determining the line of treatment to be adopted. The majority of cases which came to spas were really cases of fibrositis at the outset, that is to say, they were limited to the sheath of the nerve and did not affect the parenchyma. Parenchymatous neuritis, such as arose from the action of toxins on the nerve-fibres, called for an entirely different line of treatment. American observers had pointed out that in many cases of so-called brachial neuritis the nerves themselves were unaffected, the actual pathological condition being an inflammation of the subacromial bursa, which explained the characteristic restriction of movement in such cases. These cases were often traumatic in origin and accompanied by gross lesions in the region of the shoulder-joint, while the fibrositis which was thus set up might extend to the nerve-sheaths and cause a secondary interstitial neuritis. The treatment varied with the stage of the disease. In the first stage, rest was essential, and hyperæmia, whether by Bier's methods or the old-fashioned and valuable poultice. In the second stage, hyperæmia, whether by radiant heat, mud packs, or any other method, was the most useful procedure, and might, with advantage, be associated with ionization of salicylate of soda and hot douching. In the third stage, massage, ionization, iodine or radio-active earth, hot radio-active mineral water baths, and similar measures, would complete the cure.

Dr. WILLIAM EWART inquired whether the radio-active oxygen baths had led to any favourable results in the clinical behaviour of patients as regards their temperature and general metabolism. He also wished to know whether any good effect had been obtained from a mild systematic eliminant treatment in cases of a plethoric tendency.

Dr. ARMSTRONG, in reply, said that he entirely agreed with the President as to the necessity for differentiation, on the importance of rest, and also on the value of ionization with solutions of salicylate of soda. He was much

interested in the application of formalin solution for keeping blister surfaces aseptic, and at the same time preventing them from healing too quickly. He said he had seen many cases in which Mr. Midelton's treatment by blistering and by acupuncture, followed by the rubbing in of a solution containing croton oil and cantharides, had been followed by excellent results, and he strongly supported him in his views as to the undesirability of massage in acute and subacute cases. He felt, with Dr. Solly, that when fibrositis was present strong massage did good, but held that the more nerve tenderness there was the less should massage be given. Dr. Blanc's experience with regard to radio-active mud and the value of the radio-activity of mineral waters confirmed his own observations. As Dr. Buckley said, a certain number of these cases of neuritis about the shoulder came from traumatic causes; he thought, however, that most of the cases depended on a general condition of the system, and many were set up by chills of the neck. He strongly supported Dr. Ewart's contention that elimination, carried out so as not to reduce the patient's strength, was of great importance, and it was in this respect that the use of radio-active waters and radio-oxygen inhalations were of such value, as they got rid of the various toxins by means of lungs, skin, kidneys, and bowels, while at the same time improving the general condition of the patients and strengthening their nerve force.

Balneological and Climatological Section.

April 17, 1912.

Mr. G. H. THOMPSON, President of the Section, in the Chair.

Radium Emanation in Mineral Waters.

An Address Introductory to a Discussion on the Subject.

By T. PAGAN LOWE (Bath).

THE year 1903 was an important one in the history of balneology, for then it was that Ramsay and Soddy demonstrated that helium was one of the products of the disintegration of radium. In the same year Professor R. J. Strutt discovered radium in the deposit of the Bath waters. In the same year again the late Lord Blythswood published in *Nature* the fact of the radio-activity of the gases from the springs of Bath.

In his presidential address to this Section in October, 1909,[1] my friend Dr. Leonard Williams attempted to redress the balance between science and empiricism, which in matters balneological and climatological had, in his judgment, too long shown a bias in favour of the latter. The burden of his argument, as it seems to me on reading that address, was that the methods which we employ exercise an influence upon the internal secretory glands, and he brought forward a considerable mass of evidence in support of his theory. He did not, however, offer any explanation as to how and by what means these internal secretory glands were influenced by such methods. I think the time has come when we can afford to venture a step further by suggesting that the means by which our results are produced are in some cases, at any rate, brought about by radio-activity.

[1] *Lancet*, 1909, ii, p. 1263, and *Proceedings*, 1910, iii, pp. 1-13.

In his recent report to the Bath Corporation on the subject of the radio-activity of the waters, Sir William Ramsay prefaced his remarks on the purely local aspect of the matter by a little explanation of the present position of our knowledge of radium. I feel sure that a good many members of this Section must be in a similar state of ignorance to that in which I unfortunately found myself, so I make no excuse for prefacing what I have to say by quoting briefly from Professor Ramsay's report. Radium is a very unstable element, continually changing into another body, also elementary. During this change it parts with helium, each atom of radium furnishing one atom of helium. Having shot out its particle of helium, the atom radium, Sir William Ramsay tells us, is no longer radium, but a gas to which he has given the name of niton, but which was first known by the cumbrous name of radium emanation. The potency of radium depends on its niton. There is not time to go into the life-history of niton, but I would refer those interested in the subject to the very clear account given by Sir William Ramsay in his "Report on the Mineral Water of Bath," copies of which I am able, by the courtesy of the Corporation of Bath, to distribute to-night. Suffice it to say that what is called the "half-period" of decay for niton is about four days, so that practically in about a month it is all changed. As its metamorphosis proceeds niton gives off certain solids named radium A, B, C, D, &c., and during the change of radium into niton B rays are evolved, and it is these which are used for therapeutical purposes.

While alpha rays—that is, atoms of helium—like other gases, can be confined in glass or metal vessels, beta rays pass through; the amount passing naturally depends on the thickness of the walls of the vessel. The alpha rays, for example, confined in a glass tube do not pass through the glass; the beta rays do pass through, and by interposing metal screens of foil of different thicknesses a greater or smaller quantity may be allowed to impinge on the skin. A third class of rays is also evolved during the emission of the beta rays which have been termed gamma rays. It is not decided as yet what is their true nature. They have great penetrative power; indeed, 3 in. thickness of metallic lead is not sufficient entirely to stop them. They, too, are sometimes employed for curative purposes. It is suggested that they more resemble rays of light; that they are not "matter," but waves in the ether which surrounds and interpenetrates all matter. The continuous dissolution of the element radium into other elementary states (for those of us whose chemical education dates back more years than we care

to acknowledge) is not very easy to accept, but it suggests to me a comparison. It is a *disintegratio ad integrum.* Remembering the transformation of insects, we may say that the chrysalis radium, casting aside its inert shell helium, becomes the imago niton.

Another product found in mineral water is neon. In the gas given off by the Bath water it is present to the extent of 188 times as much as is contained in atmospheric air, an amount which Ramsay describes as astonishing. Neon is also supposed to be one of the products of radium, but possibly further investigations may show it to be one of the results of the disintregation of some other radium-like element which has not yet been discovered.

The radio-activity of all mineral waters exists only at the source, and such waters rapidly lose this quality. Nevertheless, it is possible by artificial means to render them permanently radio-active. Does the therapeutic potency of natural mineral water depend upon niton alone, or is the credit to be divided between it and the other, the chemical constituents—that is, with those constituents which we have till now endowed with the entire beneficence? We are told that radio-active waters can be prepared by adding 1 mgrm. of radium sulphate to 1 litre of distilled water, which can then be used as a subcutaneous injection without harm. Is this artificial water as potent as natural radio-active water, or does it more quickly lose its acquired qualities? Can the radio-active water, as we find it in our springs, be used as a subcutaneous injection with advantage? These, as well as other questions, present themselves for our consideration. Such, for instance, as those connected with the statement that niton, although it has the power of rendering all substances radio-active, cannot itself, according to some authorities, be absorbed by the skin. If this were true it would seem that bathing, as far as any special benefit from the radio-activity was to be obtained, was at a great disadvantage. But a little more consideration modifies this view. In addition to the immediate relief which experience tells us occurs from mineral water baths, such, for instance, as increased mobility and lessened pain, the patient is in the best position for taking the full advantage from niton. The orifices of the body are soaked in it, and the surrounding air is more or less saturated; and thus when the patient is in the bath, with his nose and mouth just above the surface of the water, he must be most favourably situated to reap the benefits of the induced radio-activity. Moreover, contrary to the belief of those who say that niton cannot be absorbed by the skin, Professor Lazarus is of opinion that it can, and in this he is supported by Ramsay and others.

In the administration of baths much longer periods of immersion seem to be called for, and I have no doubt that the custom of spending many hours in the bath, as is the custom at Leukerbad and other resorts, has a good deal to recommend it. The large public baths, as used there, however, seem to be inadvisable; and I cannot but think that the small bathroom, due regard being, of course, given to ventilation, is preferable. The probability is that niton is none too strong, and we want to concentrate—perhaps to augment—it as much as possible.

Some authorities are of opinion that natural waters are not sufficiently radio-active to be of much service therapeutically, but on this question of their potency there are very conflicting views, and Professor Farr, of New Zealand, in an interesting discussion on the effect of radio-active spring water on trout, refers to radium emanation as a gas which physically is a hundred thousand times more active than radium.

Hitherto, details of the effects of niton given off at the fountain head are not very numerous, notwithstanding the immense amount of research which has been made with regard to radium and artificially prepared radio-activity. It is urgent that those of us who have access to natural radio-active waters should fill up the deficiencies. Fortunately at Bath the waters are sufficiently charged with niton for all purposes, and may be considered saturated, and we have 500,000 gallons coming up daily at a temperature of 118° F. There is also available more than 5,000 litres of gas.

Has the presence of niton any subtle action on the salts of magnesium, sodium, lime, and so on, which are found in indifferent waters, rendering them more absorbable or potent when brought into contact with mucous membrane? The free absorption of radio-activity by mucous membranes and by the lungs will probably alter our treatment by those routes, and we shall in the future rely upon them much more. Inhalations will be more frequent, and drinking more resorted to, for longer time and in greater quantity. The same, too, will apply to vaginal and rectal douching. This latter, the so-called Plombières method, is very popular at the present time at certain spas. Certainly, its effect in suitable cases is very striking. It is also almost certain that the future will see a great increase in the combined use of electricity and radio-active waters.

Sir William Ramsay, in his most lucid and able report, from which I have already quoted, referring to the ingenious spraying machine invented by Mr. Jones, the engineer to the bathing establishment of

Bath, for use in throat and eye affections, suggests that the natural gas should be used instead of the mineral water. If this were adopted—and I have no doubt it will be—Ramsay is of opinion that "an agent nearly twenty times as potent would be available." He also thinks that in a similar way niton might be applied directly in the treatment of local rheumatism, especially if the patient were insulated and connected with the negative pole of a battery. In this report he further outlines a method by which the intake of niton could be very much increased by giving electric baths with a continuous current. "If," he says, "the patient in the electric bath were connected with the negative pole of a battery giving, say, 100 volts potential, or even more, and the other electrode were placed in the water, of course not in contact with the bather, the niton would rapidly reach the skin." It is, indeed, not unlikely that it would enter the system by so-called ionization, and in this way a considerable dose might be given.

In the estimation of the value of natural water it is necessary to estimate the degree of its radio-activity, the contained volume of gas, and the amount of helium. Although helium is therapeutically useless, nevertheless its presence is an indication that we are dealing with a radio-active water. At the King's Well at Bath Lord Rayleigh found that there were 12 volumes of helium in 10,000 of the water, and some of the foreign mineral waters have been examined by Ramsay and others. Ramsay has found it impossible to compare these amounts with statements of the radio-activity of foreign waters, for the latter are stated in uncertain units and could only be determined in the special apparatus employed by the foreign scientists.

No introduction to a discussion on the subject we are joining in to-night would be complete without some reference to the valuable work of Professor His. He administers niton by means of a portable apparatus, and supplements its effects in certain cases of rheumatism by an injection of an insoluble, or repeated injections of a soluble, salt of radium in the immediate neighbourhood of the affected joints. His's article on the treatment of gout and rheumatism in the *British Medical Journal*[1] unfortunately lacks full details of the exact method of the administration of niton. He tells us that under its influence the blood loses its uric acid within a few weeks, but the way, the time, and the dose are somewhat vague. What we require as practical balneologists is to learn how we can best utilize the niton for the benefit of our patients. It seems probable that radium emanations have a specific action on gout,

[1] *Brit. Med. Journ.*, 1909, ii, Epitome, p. 73.

and it is in cases of this disease that we may expect some of our greatest successes.

In an article by W. H. B. Aikins and F. C. Harrison, published in the *Canadian Practitioner and Review*[1] last August, on the "Present Status of Radium-therapy," the results in general are summarized as follows: (1) Greatly increased diuresis and excretion of uric acid; (2) largely increased carbonic acid exhalation, from 20 to 60 per cent.; (3) lowered blood-pressure, especially in arterio-sclerosis; (4) decreased blood viscosity; (5) great improvement of gastric and duodenal digestion; (6) marked solvent action on gouty deposits; (7) the dislocation of uric acid and its salts into carbon dioxide and ammonia; (8) inhibition of inflammation and relief of pains in rheumatism; (9) increase of sexual vitality; (10) considerable influence over sympathetic nerve affections; and (11) marked results in diabetes, albuminuria, and glycosuria. All of these results I am able to confirm from personal observation in Bath, and I would add the rapid disappearance of indican from the urine.

A good deal of further work on the subject comes from Germany. It seems that the Kreuznach waters are highly radio-active and the local physicians have been studying the results of their applications in various conditions. Substantially their conclusions coincide with those just quoted, but some further information in the matter of contra-indications and best form of administering the emanations is now available.

So far as contra-indications are concerned, they do not at present appear to be very definite, but there are observations enough to warn us that they certainly exist, and that, to say the least, some cases demand very great caution in the application of the remedy. Thus Meternitzky, of Kreuznach, says that he has found application of radium emanation to cause not only subjective symptoms, such as dizziness, fullness of the head, faintness, pains in the joints, but also objective phenomena, such as emaciation, albuminuria, and even hæmaturia. This, as coming from a physician practising at Kreuznach, would be a very valuable testimony even if it stood alone. But it does not. Several others who have studied the question, notably Davidsohn, working with an artificial radium-producer, and not with mineral waters, found that rheumatic and gouty patients after radium treatment experience violent pains in the joints and aggravation of other symptoms. This is particularly interesting to balneologists as affording an explanation of what we are

[1] *Canadian Practitioner and Review*, 1911, xxxvi, pp. 465-79.

all so familiar with—namely, an acute exacerbation of gout soon after treatment has commenced. Other observers have described considerable rises of temperature, hæmorrhages from the mucous surfaces, and the like. It would be interesting to me personally to find any confirmation from other radio-active spas of the experience of everyone who has ever practised at Bath, to the effect that both baths and waters are definitely contra-indicated in a case where there is any suspicion of tuberculosis. Löwenthal considers that chronic nephritis, particularly of the contracting kind, affords a definite contra-indication for treatment. It is probable that at least some of the accidents and inconveniences which have been described as resulting from treatment by radium have been due to the ignorance under which we still labour on the subject of doses. I may say here that a serious disadvantage to radium research and the application of radium is the absence of a general uniform standard of measurement. Neither the counting by Volt unit nor that by Maché unit is uniform in itself, as these units differ according to the different apparatus used. At the suggestion of Löwenthal the Radiological Congress at the Brussels meeting last year appointed a committee which accepted as unit the action of 1 grm. of radium. It is hoped that this proposed Curie unit will bring order into the present confusion.

Concerning the method of administering radium, the German authorities do not agree with the views of those who attach a subsidiary importance to the balneary treatment. Thus Kemen says: "In the bathing method a certain part of the emanation passes through the skin into the blood, as was conclusively proved by Engelmann. It is also well known that during the bath the emanations readily find their way into the pulmonic circulation. Yet the bath appears to have a third mode of action in addition to these two; for, on the one hand, the fact of a very marked improvement, subjective and objective, is well known, yet on the other the quantities of radium available by the pulmonary and cutaneous routes are much too small to account for the results." What this third mode of action may be it is impossible to say at present; but we should do well to bear this view in mind lest in our enthusiasm for drinking and inhalation, the action of which we happen to understand, we be led into neglect of the time-honoured method of bathing, the detail of the action of which is still insusceptible of explanation. We should not reject it merely because we are unable to explain it. For we must remember that there is still a great deal about radio-activity and its therapeutic action which we are unable to explain. We cannot explain, for example, by what processes it produces its therapeutic effect. If we

may rely upon the work of German authorities its action is in no sense bactericidal. We know further that it stimulates metabolism, but by what process it achieves this is not clear. It may be, it is indeed probable, that the suggestion of Dr. Leonard Williams may yet prove to be correct and that the stimulation is effected by way of the internal secretory glands. The statement that radio-activity is not bactericidal is not universally accepted. Later investigations by Lazarus Barlow seem to show definitely that it is decidedly bactericidal. But whether this action is sufficiently intense in mineral waters, or even in the gas from such waters, to be of service has, as yet, not been proved. After all, this would seem to be a minor point, for as far as present knowledge goes it is improbable that bactericidal qualities, as we understand them, are the explanation of the benefit that is derived from mineral water treatment in such diseases as gout and fibrositis.

From my own personal experience I have little to add that is of value. It seems that we balneologists are like the man in Molière's play who was delighted to find that he had been talking prose all his life. We in Bath and elsewhere have been using radium all our medical lives, and we are proportionately uplifted. But we cannot say which of our results have been due to the radio-activity of the waters and which may be accounted for by the douching, massage, and other measures upon which we have so long relied. Nevertheless, I should like to confirm from my own observation some of the clinical results that have been reported by certain observers.

The experiments by Wickham and others show that radium is destructive to the gonococcus, whilst the literature of gonorrhœal arthritis shows that the cure of this disease is usually tedious and tiresome by medical means. Nevertheless, some of our greatest successes in Bath have been with patients suffering from it. I have always, in addition to the mineral water, given them iodide of potassium, and it is a significant fact that the salts of potassium are radio-active, giving off beta rays, that is the valuable ones in the medical uses of radium. It may be that the action of potassium in gout and fibrositis is due to radio-activity, and that metabolism under its influence is stimulated.

In connexion with gonorrhœal arthritis it is interesting to note how remarkably well cases of iritis do with mineral water treatment. We have patients sent to us with recurrent iritis who rapidly improve and, what is still more important, do not usually relapse. Mr. W. M. Beaumont, who has seen a considerable number of these cases, doubts the very existence of rheumatic iritis at all, and believes the cases we see

in Bath are almost entirely gonorrhœal, and that those which are not are due to some other form of toxæmia.

Experiments by Wickham and Degrais show that radium is not, strictly speaking, bactericidal, but that it so modifies the culture medium as to make it inimical to the growth of the gonococcus. They are of opinion that one-millionth part of pure radium in water will act on the cultures. If these views are corroborated it is probable that in the near future the whole treatment of gonorrhœa and its sequelæ will be revolutionized. Gonorrhœal ophthalmia at once strikes one as a disease in which some confirmation of these hopes should be sought for.

During the last few weeks I have been treating an old lady, aged 75, suffering from a long-standing atrophic scirrhus of the left breast by exposing it to the gas from the Bath springs. The ulcerated surfaces were exposed for ten minutes daily, and after twelve applications all the ulcers had healed and some of the surrounding induration had disappeared. This is the first case, I believe, of malignant disease in which the gas has been used in this way. A single case is, of course, of no very great value, nevertheless it encourages us to repeat the experiment, let us hope with equal success.

I have here a radiograph taken at the Bath spring which may be of interest. I also show you two glasses, one that has been in use for several months and the other for five weeks, which demonstrate the staining effect of radium. You will notice in the fractured specimen that the staining has gone quite through the glass, which would not have been the case if it had been caused by iron. Furthermore, I have Sir William Ramsay's authority for stating that this discoloration could only have been produced by radium.

I must not take up any more time with vague thoughts and random speculations regarding a subject the importance of which we probably have not yet fully grasped. The discovery of radium with its far-reaching effects on every part of the human economy has altered our views fundamentally with respect to balneological treatment. A more scientific era has dawned. Time was when a suspicion of a man's professional integrity was raised if he called himself a balneologist; but now, instead of groping in the shades of night, our landscape is illumined by the sunshine of science, and we march along with firmer footsteps and with head erect. And as old ideas go the way of all things human, our grouping of springs into saline and chalybeate, into purgative and alkaline, and so on, must now be amended to include those which are radio-active. Let us hope that any new

classification will rid us for ever of the somewhat sinister name of "indifferent waters," for they are no longer indifferent.

One final word more with regard to radium. Many of us believe that in this element we have the explanation of the therapeutical effects of mineral water, but beyond a declaration of our faith we dare not at present go. It is a postulation that would explain the accumulated evidences of the ages. Every bio-chemical fact concerning it dovetails with the clinical effects of its application. Is it, then, any wonder that we balneologists regard radio-activity as the unknown god, and the blind worship of ages and the empirical faith of centuries as justified? To us the unknown god and his shrine are alike worthy of our homage.

DISCUSSION.

The PRESIDENT (Mr. G. H. Thompson) said that he had listened to Mr. Pagan Lowe's paper with very great interest, coming as he did from a place like Buxton, where it was claimed that the waters were likewise radio-active, although hitherto they had been classified as indifferent. The subject was an all-absorbing one. This particular department of medicine had by many been relegated to that nebulous atmosphere which borders on the realms of quackery. Now, however, balneology could claim a scientific basis for its practice. We must remember that the profession was at present merely on the threshold of inquiry with regard to radio-activity, and under that term he included all its various forms and manifestations. It must not be forgotten that waters like those of Bath and Buxton had definite medicinal properties of a curative nature when properly administered. For instance, in the Buxton Devonshire Hospital, which was practically a national hospital, through which there passed three to four thousand cases every year, coming from all parts of Great Britain, this curative power had reached the high average (extending over many years) of 88 per cent. It was therefore very important to have some exactitude of dose. In terms of radio-activity this would be a matter of time. In cases of gout the clinical manifestations on using the waters were similar to those described by Professor His on administration of the emanation, which proved a powerful solvent of uric acid in the blood, and it was possible by an estimation of the blood before and after the administration of the waters, in cases of gout, that similar results would be obtained. With

regard to the action on the skin, some years ago a little handbook on the "Baths and Climate of Buxton" was drawn up by the medical men in the town, in which it was suggested that the probable action of the radio-activity in the waters was through the skin, but Professor His seemed to suggest that the action was chiefly through the lungs. No doubt the skin also was a potential recipient of the emanation. When one considered the physico-chemical constitution of these bodies, and remembered the great powers of katalysis, both organic and inorganic, the action of the small quantity of emanation which would affect the internal secretions or the tissues presented no difficulty. It was well-known how powerfully some of the ferments acted internally, just as an infinitesimal amount of substance would change an insoluble substance into a rapidly soluble one. He agreed as to the effects of the radium emanation which Mr. Pagan Lowe had enumerated. He had also observed them in cases treated by the Buxton waters. The specific gravity of this water was only 1010, and it contained no solid of any great potency; one was therefore driven to the conclusion that there was some power in it which was not represented by any solid chemical constituent in the ordinary sense.

Dr. ARMSTRONG (Buxton) said he would like to intervene early in the discussion because for the last three years he had worked much in connexion with radium water therapy, and had had the privilege of co-operating with some of the most eminent Continental observers. His object now was to lay before the meeting certain definite facts which had come to his notice in the hope that they might be discussed, and that further information on the points might be elicited. It had been definitely proved that radium emanations could enter the body: (1) by the stomach; (2) by the lungs; (3) by the skin, in both a general and local manner; and (4) by hypodermic injection. It left the body in order of quantity: (1) by the lungs; (2) by the intestines; (3) by the kidneys; and (4) by the skin. The speed of passage through the organism was: (1) in one hour by inhalation; (2) by injection, at a somewhat longer interval; and (3) by the stomach, which occupied three or four hours. The action via the stomach had been followed out as follows: (1) direct penetration of the stomach and intestines, with a rapid contact effect upon the ferment-secreting apparatus; (2) by the capillaries of the lymph and portal systems circulating through the liver, and reaching the cava ascendens and descendens through the thoracic duct; thence to the heart and lungs. A considerable quantity

of radium emanation reaching the lungs was re-absorbed. Lazarus held that this " retro-spiration " carried the radium emanation into the arterial blood, to which action he attached great importance. With regard to the action by the lungs, the radium emanation penetrated into the alveolar air; then into the blood; with the blood into the organs and tissue-cells; and was discharged through lungs, kidneys, skin and bowels. The effect of radium emanation in baths was increased by being combined with oxygen. Radium emanation was absorbed through the intact epidermis. That had been denied, but he had seen experiments in Germany which showed its truth. The skin was permeable in both directions. Lazarus had found in the perspiration over 1 per cent. of the radium emanation taken internally. Radium emanation initiated or intensified chemical bodily processes, especially those of oxidation. It activated the proteolytic and autolytic ferments, making more efficient the processes by which the nucleo-protein was oxidized into the purin bases; a most important fact, as disturbed metabolism was largely due to disturbed fermentation. The emanation decreased the blood viscosity; it increased the activity of the digestive ferments, and, by activating any deficient ferment, caused changes in the various inter-related ones. It assisted dissociation of uric acid and its salts into carbon dioxide and ammonia, and increased the output of the former. It exerted a solvent action on gouty and rheumatic deposits. It markedly decreased arterial tension in arterio-sclerosis both by inhalation and by drinking the water, the effect being much enhanced when oxygen was added to the bath or inhalation. Diuresis was increased, as well as the excretion of uric acid, and the toxic properties in the sweat were likewise greater. He worked on the question of the sweat with Lazarus, who proved conclusively that the toxic products under the influence of niton went up from 0·05 to 0·09, when the water had been taken for a month. It prevented the transformation of easily soluble lactim urate into the less soluble lactam urate (mono-natrium urate). It increased nerve power, especially of the sympathetic system. *In large doses*, given continuously, it destroyed or markedly hindered the development of staphylococci, streptococci, fermenting bacteria, and sporiferous fungi. That could only be brought about by very large doses. This effect could be best obtained by the frequent inhalation of niton and oxygen to saturation of the system.

He wished also to mention the efficacy of the application of radium emanation in radio-active earth-packs; especially when reinforced by ionization, passing the constant current through the compress. It was

held by a German professor that, in diabetes, one of the ferments was wanting or much lessened—namely, the ferment which caused the oxidation of glycogen derived from the carbohydrates. Under a dietary with the carbohydrates cut out, the bodily energy must come from oxidation of fats and albumens; the lipolytic and proteolytic ferments were overtaxed, and acetone, acetic acid, and oxy-butyric acid appeared in the urine; generally disappearing when carbohydrates were returned to. The same result could be obtained from radium-emanation because it stimulated oxidation. He had passed through radium therapy many hundreds of cases by means of artificial niton waters, as well as natural ones, with excellent results in the case of arthritis, neuritis, neurasthenia, and especially of auto-intoxication. With regard to bathing, he felt convinced that the benefit from that was largely a question of inhalation, and he thought the great success of one of the baths in Buxton—namely, the swimming bath—was due to the large body of niton water always running through the bath and being constantly agitated by the bathers. He wished to add his testimony to the truth of Mr. Pagan Lowe's contention that the power of the radio-active waters was greatly increased by the use of electricity. Electricity provided a means of carrying the radium emanation to the depths of the system. With regard to measurement, the Maché unit was the thousandth part of the electro-static unit. With regard to any ill-effects from radium emanation, he had worked with very strong radium water, and found that if the water or baths were too strong one could get a marked reaction and acute attacks of pain. Albumin often disappeared during niton treatment, and he had never seen hæmaturia occur. Some of his most successful cases had been the subjects of chronic nephritis, but it had been necessary to start with very small doses, and increase the strength gradually. That radium emanations were present in the blood after bathing was pointed out to him by Professor Englemann, and experiments made showed that observation to be correct. With regard to gonorrhœal rheumatism, in two bad cases he had used strong solutions of radium emanation, and in both cases with success. He was sure the best way of giving radio-active waters was by frequently repeated doses, say at intervals of three or four hours.

Dr. Buckley (Buxton) desired to express his appreciation of the very able and temperate method which Mr. Pagan Lowe had adopted in dealing with this important question. There were so many points of interest that they would suffice to carry the Section over not one, but

two or three meetings, and not only for members who came from places where the waters were radio-active, but also those coming from other towns. He agreed as to the importance of dosage. On the one hand there were radio-active waters at English spas and on the Continent, and on the other waters artificially prepared of much greater nominal strength. He could scarcely conceive of the results of the two being identical unless it could be explained by the fact that the body could only absorb a certain quantity of the emanation, and that a large or a small quantity was excreted in a given time. The results obtained by His were certainly results with which those who were accustomed to use radio-active waters in Buxton were familiar. The effect of a weak dose was prominently borne out by the results at the Devonshire Hospital, Buxton, where 88 per cent. of the cases were shown to have been relieved or cured. The water used at that hospital had been recently discovered to be drawn from the weakest spring which Buxton possessed. That matter was now receiving the attention of the authorities, and he did not doubt that the spring would be improved by better conduction. It had been said that there was an exacerbation of symptoms after artificial radium treatment, also after radio-active baths, but it must be remembered that it occurred also in the use of waters from totally different springs, as well as in the case of patients who took hot baths of plain water. If one gave a very gouty patient two or three really hot baths he would develop acute gout. Therefore, when a patient had treatment by a radio-active spring and got gout, he did not think it followed that that was due to the radio-activity, and it was necessary to be cautious about attributing the results obtained from mineral waters to anything which appealed much to the imagination. For instance, if radium emanation were given, using oxygen as the vehicle, how much could one put down to the emanation, and how much to the oxygen? The stimulating powers of oxygen on metabolism were very great, and so were those of radium emanation. If one adopted ionization of radio-active earths one was dealing with a very potent method. If the negative pole were used, the negative ions of everything in the pack would be driven through the skin, and it would be fallacious to assume that the results were those purely of radium. If the electric bath were used, as suggested by Sir William Ramsay, connecting the negative pole with the patient and the positive pole in the bath, the constant current was being driven through the water, and would be the means of splitting it up into its constituents hydrogen and oxygen, and, therefore, how could one know whether part of the result was not due

to these liberated ions? He thought this question should be approached in a very critical spirit. It was dangerous, because a method appealed to our imagination, to attribute to it all the virtues of the philosopher's stone. It had been said by many that the result of bathing in radio-active water was to reduce the blood-pressure. But the immediate result of bathing in the swimming bath at Buxton was to increase the blood-pressure: he was quite sure of that from a number of observations. But that increase was only temporary, for a course of baths resulted in a reduction. Anyone who bathed in water of the temperature of those baths, namely, 82° F., got a preliminary rise, followed by a fall; but if one bathed in a bath which was of a higher temperature, there would probably be different results in the matter of blood-pressure. The inference was that it was not only due to the radium emanation, but to bathing in water of that temperature. With regard to the use of radio-active water by the Plombières method, the Plombières Spring was the most radio-active one in France, and the result of the introduction of one or two litres of highly radio-active water into the colon could not but produce a marked effect. And without disparaging other mineral waters, his opinion was that it pointed to the fact that the Plombières method did not produce a purely mechanical effect, but its action must also depend on the water used. One water would produce one effect and another water another effect. Therefore, one must consider the quality of the water as well as the method. He agreed with Dr. Armstrong that the only satisfactory method of using these mineral waters was to give small frequently repeated doses.

Dr. PRESTON KING (Bath) considered that Dr. Buckley had sounded the right note—namely, that of caution in approaching the subject of the benefit derived from radium. They should endeavour to get something definitely scientific on which to go. Clinical experience of many years had been in favour of there being something in the Bath waters which could not be explained from chemical analysis, and it was fair to assume that this unknown "something" was the radio-activity which had been demonstrated in so large a quantity. One must remember in considering this subject that a new remedy was always the best. There was the tale of the professor who said to his students, "Make haste, gentlemen, to use a new remedy while it does good." At the Royal Mineral Water Hospital, Dr. King, with the aid of Dr. Lindsay, had begun some experiments (which were stopped by the coal strike) upon gouty patients who were kept upon a purin-free diet to determine the

excretion of uric acid by the use of radium waters. The Radium Company of Birmingham had kindly lent him an apparatus for making radio-active waters, but since Sir William Ramsay's discovery he would continue these experiments with the Bath waters. The artificial radio-active water did not seem to have any advantage over the Bath waters in the treatment of rheumatic cases, as far as his experience went.

Dr. King congratulated Mr. Pagan Lowe on the able way in which he had opened the discussion. He had spoken of Bath waters having been hitherto called indifferent, but Dr. King agreed with him that in the future they would be known as radium waters.

Dr. ACKERLEY (Llandrindod Wells) desired to put a few questions to those in the meeting who had probably had more experience in the administration of radium than he himself had had. But he first wished to add his testimony as to the excellent way in which Mr. Pagan Lowe introduced the question. In the case of his own town, they had known that the waters were radio-active for some years. His first question was, what actual proof was there that radium had anything to do with the benefit achieved? He had not heard of any control experiments. He believed there was a vast difference between a spring of running water and dead or distilled water—a difference in the effect on the body. Without controls it was useless to speak of results as being due to one property of the water. At a previous meeting, when the benefits of lavage were set forth, and when it seemed to be regarded as a universal remedy, and almost as much was claimed for it as was now claimed for radium, he also asked for proof, especially as other means of treatment were being used concurrently. Mr. Pagan Lowe spoke about giving iodide of potassium in cases of gonorrhœa, and suggested that any good results he got might be due to the radio-activity of the drug. Why should it be assumed that this was so? Were the poisonous effects of potassium iodide also due to its radio-activity? He believed as good results were obtained by other means as were now claimed for radium emanations. Two years ago everything seemed to be cured by drinking lactic acid milk, and nothing now said about radium exceeded that which was then said about lactic acid milk, except that the latter was said to be a prolonger of life. In regard to arthritic cases, he was not inclined to attribute benefit to any one thing which he tried. Seeing how much was at stake it was only fair, in investigating the claims of a new remedy, to have plenty of controls to test the results. The only point in which Dr. Lowe's paper seemed to him

to have scientific value, was in regard to the application of the radium gas to an ulcerating carcinoma. But even there he would like to know whether the other conditions of the patient had been altered; whether it was a question of better nutrition under treatment, of freedom from worry, and a greater share of fresh air and sunshine, which were factors of great value. If radium had a therapeutic value could it not be best obtained by exposing patients to sunlight, as the presence of helium in the sun was suggestive that radium also was active there?

Dr. JONES LLEWELLYN expressed the great pleasure with which he had listened to Mr. Pagan Lowe's opening paper, which, he considered, put the matter in a very lucid, scientific and temperate manner, and he did not agree with Dr. Ackerley's somewhat acrid criticisms. If the latter would read the literature of the subject he would find records of many control experiments. He would like to draw attention to some experimental studies of Mandel's in this connexion. This observer endeavoured to ascertain whether in the subjects of gout an increased excretion of uric acid took place under emanation, and if so, how far any clinical or subjective amelioration of symptoms could be attributed thereto. He found that in seven gouty patients after treatment in an emanatorium, an increased excretion of uric acid ensued in two subjects. Of the remaining five, in two no alteration in average excretion took place, in two a slight diminution, while in one a marked decrease to the extent of 50 per cent. was demonstrated. Now in four out of the seven cases an undoubted clinical and subjective improvement was observed, while the uric acid curve showed diminution rather than increase of excretion. Gudzent also had noted a case in which, without any increase, a diminution of gouty nodules occurred. Consideration of His's group of cases also showed that clinical improvement did not invariably run parallel to the uric acid content in the blood. Thus a striking amelioration occurred in a patient under radium emanation, notwithstanding that the blood contained uric acid, while in another, the subject of diffused gouty nodules, no uric acid was found in the blood throughout the treatment, and yet the patient had repeated attacks of gout throughout this period. We might take it, therefore, that His's series of cases showed that though in the majority of instances the amelioration of symptoms was attended by the disappearance of uric acid from the blood, yet in some few cases improvement took place while the uric acid content of the blood was unaltered. The same discrepancy obtained in regard to the excretion of uric acid in the urine

as shown by Mandel's observations. It seemed obvious therefore that the beneficial effect of radio-active water involved something more than the dissipation of uric acid in the blood and its excretion in the urine. Beyond, therefore, its solvent action on gouty deposits, it would seem possible that through its suggested capability of activizing the body ferments, it might in some obscure way correct the particular warp in metabolism responsible for gout. Thus through its stimulating action on the digestive enzymes, it might prevent a formation of the abnormal substances responsible for gout, or through its quickening of the autolytic enzymes might hasten the disruption and excretion of such when formed.

Dr. MANTLE said the question of radio-activity in mineral waters seemed almost ancient history to them in Harrogate, since as early as in 1905 they learned from Professor Ramsay that their sulphur waters were radio-active. But no capital had been made of that fact, possibly through ignorance concerning the therapeutic effects of radium, or possibly through their modesty. A very elaborate paper was read before the Section by Dr. David Brown[1] last year, which proved conclusively that the sulphur element was largely responsible for the changes in metabolism which took place during the experimental imbibition of the old sulphur spring in both physiological and diseased subjects. Therefore, perhaps, Dr. Ackerley had some support when he said too much credit must not be given to radium in explanation of the good effects produced at the various spas. More would be known about radium in ten years' time, but for the present one should not encourage its being regarded as a panacea for most of the ills to which flesh was heir.

Dr. FORTESCUE FOX remarked that Fellows might be interested to hear what was done with regard to radium at Kreuznach, where he had the pleasure of examining last year, with Dr. Engelmann, the method of manufacturing it from the waters. For three or four years radium had been derived from the deposit at the bottom of the tanks. The water was first poured six or seven times over the "graduators," on the thorny sides of which it slowly evaporated, with the result that a strong sedimentary liquid was obtained. From this a reddish powder, *sinter*, was extracted. The *sinter* after various treatment for several weeks

[1] *Proceedings*, 1911, iv, pp. 45-62.

yielded a white powder (barium radium bromidi). From this white powder fluorescent radium bromide was finally obtained. The Kreuznach physicians had long attached importance to the local application of this *sinter* in arthritic affections; and it was interesting that for probably 200 years the "graduators" had been used as an inhalatorium. This treatment, hitherto empirical, had been valued by Dr. Engelmann's father and grandfather, who were in their day distinguished practitioners at Kreuznach. Besides inhalation and local applications, the grandson attached importance to the use of the "radium emanation water" by the mouth. The Section had had both aspects of the subject very fairly placed before them at that meeting; the dangers of an over-active imagination had been set out, as well as those of having no imagination at all. It would be foolish and unscientific to shut one's eyes to the fact that the discovery of radium and of the radio-active derivatives had exhibited to us a new power, and opened up new possibilities of treatment. The chemical and physical properties of medicinal waters were but little appreciated. The knowledge of this unsuspected natural force would affect the science of medical hydrology. To any investigation that tended to elucidate the various physical and chemical properties of natural springs it behoved the Section to give a careful and patient and cautious consideration.

Dr. ARMSTRONG wished to add, with regard to Dr. Ackerley's remarks concerning control experiments, that Dr. Harburn and he took 10,000 observations on the waters of Buxton, and compared the results with those of distilled water. The paper was published in the *Lancet*, and showed that any disturbance of contained emanation, either by heating or cooling the water, at once diminished efficacy.

Dr. ACKERLEY added that his purpose in speaking was to ask questions and suggest doubts. He was not without imagination, nor very sceptical; he simply required proof. The fact that Buxton waters produced certain results was neither here nor there, as every spa could show the same results. The point was, why was one agent selected as the cause of improvement, and that only?

Mr. PAGAN LOWE, in reply, reminded Dr. Ackerley that there was a great deal of literature detailing careful experiments with radium and controls, and there could be no doubt about the results. The results obtained at spas were confirmed over and over again by artificially

produced radio-active waters, and, therefore, one was justified in presuming that the results obtained when radio-active waters were used might in a measure be due to the radio-activity; and in Bath it was a matter of common observation that the waters were far more potent in cases of gout and rheumatism when drunk at the fountain head than when kept for a few hours, and that the bottled Bath water, although an excellent table drink, was quite inert as far as gout and rheumatism were concerned. He was much obliged to Dr. Armstrong for what he said, and he owed him an apology for not having quoted from his paper, instead of from the reproduction of it by a Canadian journal. But he was not aware that Dr. Armstrong had brought the matter forward in that way; and in searching the literature of the subject he was anxious to obtain evidence from independent places, and get the result of very careful experiments in Canada. He agreed as to the importance of dosage, and that there was yet much to learn on the subject generally, so that it was necessary to proceed carefully and to experiment. He regretted that time did not permit of his elaborating his reply any further.

PROCEEDINGS

OF THE

ROYAL SOCIETY OF MEDICINE

VOLUME THE FIFTH

COMPRISING THE REPORT OF THE PROCEEDINGS FOR THE SESSION 1911-12

SECTION FOR THE STUDY OF DISEASE IN CHILDREN



LONDON
LONGMANS, GREEN & CO., PATERNOSTER ROW
1912

Section for the Study of Disease in Children.

OFFICERS FOR THE SESSION 1911-12.

President—

G. A. SUTHERLAND, M.D.

Vice-Presidents—

Sir W. WATSON CHEYNE, Bt., C.B., F.R.S.
H. D. ROLLESTON, M.D. H. J. STILES, C.M.

Hon. Secretaries—

T. R. WHIPHAM, M.D. H. S. CLOGG, M.S.
PERCY G. LEWIS, M.D. (*Provincial*).

Other Members of Council—

HAROLD BURROWS, F.R.C.S. (Southsea).
H. J. CAMPBELL, M.D.
J. W. CARR, M.D.
EDMUND CAUTLEY, M.D.
E. J. CAVE, M.D. (Bath).
DUNCAN L. FITZWILLIAMS, F.R.C.S.
A. E. GARROD, M.D., F.R.S.
S. GILFORD, M.B. (Reading).
G. S. HAYNES, M.D. (Cambridge).
F. W. HIGGS, M.B.
GUY B. HOLLINGS, M.D.
F. LANGMEAD, M.D.
J. T. LEON, M.D. (Southsea).
R. CLEMENT LUCAS, F.R.C.S.
W. J. MIDELTON (Bournemouth).
J. P. LOCKHART MUMMERY, F.R.C.S.
P. N. BLAKE ODGERS, F.R.C.S. (Northampton).
GEORGE PERNET, M.D.
J. D. ROLLESTON, M.D.
ERIC BELLINGHAM SMITH, M.D.
E. I. SPRIGGS, M.D.
SYDNEY STEPHENSON, F.R.C.S.Ed.
A. H. TUBBY, M.S.
G. DE BEC TURTLE, M.D.

Representative on Library Committee—

J. PORTER PARKINSON, M.D.

Representative on Editorial Committee—

H. S. CLOGG, M.S.

SECTION FOR THE STUDY OF DISEASE IN CHILDREN.

CONTENTS.

October 27, 1911.

PAGE

Case resembling General Paralysis of the Insane. By J. Porter Parkinson, M.D. 1

By G. A. Sutherland, M.D.
(1) Infantilism 3
(2) Extreme Rickets and Infantilism 5

By F. Langmead, M.D.
(1) Sclerodermia with Myositis Fibrosa 6
(2) Sclerema 7

Two Cases of Partial Subluxation of Knee-joints, with Voluntary Production of Noise during Flexion and Extension. By O. L. Addison, F.R.C.S. ... 9

By P. Maynard Heath, M.S.
(1) Intra-uterine Fracture of Tibia and Fibula, with Absorption of Bone 10
(2) Gigantism of Forepart of Foot 12

Double Third Nerve Palsy due to Acute Poliomyelitis. By A. H. Payan Dawnay, F.R.C.S. 13

Recurrent Unilateral Œdema. By T. R. Whipham, M.D. 14

Diphtheria of Œsophagus. By J. D. Rolleston, M.D. 16

By R. Salusbury Trevor, M.B.
(1) Aneurysm of the Descending Branch of the Right Coronary Artery, situated in the Wall of the Right Ventricle, and opening into the Cavity of the Ventricle, associated with great Dilatation of the Right Coronary Artery, and Non-valvular Infective Endocarditis ... 20
(2) Congenital Morbus Cordis (Cor Biatriatum Triloculare) 26

Night-blindness with peculiar Conjunctival Changes in Children. By Sydney Stephenson, C.M. 28

November 24, 1911.

PAGE

Infantilism. By VINCENT DICKINSON, M.D. 37

By REGINALD MILLER, M.D.
(1) Chronic Interstitial Nephritis with Infantilism 38
(2) Intracranial Tumour 42

Psoriasis and Flexion of the Terminal Phalanx of the Thumb. By F. J. POYNTON, M.D. 44

Congenital Absence of Patellæ and Deformity of the Nails in a Mother and Three Children. By A. C. D. FIRTH, M.B. 44

Hereditary Syphilitic Infant treated by Intravenous Injection of "606." By J. L. BUNCH, M.D. 46

Congenital Word Deafness and other Defects. By E. BELLINGHAM SMITH, M.D. 47

Granuloma Annulare. By HALDIN DAVIS, F.R.C.S. 48

Cerebral Aplasia with Hydrocephalus (Pathological Specimen). By R. SALUSBURY TREVOR, M.B., and H. D. ROLLESTON, M.D. 49

Purpura in Infective Diarrhœa. By H. D. ROLLESTON, M.D., and J. B. MALONY, M.B. 54

Congenital Flexion of the Proximal Interphalangeal Joints of the Fingers. By DUNCAN C. L. FITZWILLIAMS, F.R.C.S.... 59

December 15, 1911.

A Discussion on the Treatment of Tuberculous Joint Disease in Children. Opening addresses:—

(I) Indications for Surgical Interference, with remarks as to the After-results. By A. H. TUBBY, M.S. 65

(II) The Results obtainable without Operation. By Sir ANTHONY BOWLBY, C.M.G., F.R.C.S. 76

(III) The Rôle of Vaccine Therapy. By A. BUTLER HARRIS, M.B. ... 84

Mr. ROBERT JONES (Liverpool) (p. 107)—Sir W. Watson CHEYNE, C.B., F.R.S. (p. 109)—Mr. H. J. GAUVAIN (p. 111)—Mr. J. JACKSON CLARKE (p. 113) Mr. W. G. SUTCLIFFE (p. 114) Mr. E. M. CORNER (p. 114)—Mr. LOCKHART MUMMERY (p. 117).

January 26, 1912.

Cases illustrating the Late Results of Muscle Transplantation for the Relief of Talipes Valgus (Paralytic). By DOUGLAS DREW, F.R.C.S. 121

Cerebellar Tumour. By ERIC PRITCHARD, M.D., and SYDNEY STEPHENSON, C.M. 122

Two Brothers with Hæmophilia. By THEODORE THOMPSON, M.D. 123

PAGE

By R. C. Jewesbury, M.B.
(1) Deformity of the Chest ... 124
(2) Hemiplegia—Right Side ... 126
(3) Hemiplegia—Left Side ... 127
(4) Hemiplegia—Right Side ... 127

Congenital Pulmonary Stenosis without Cyanosis. By F. Parkes Weber, M.D. 128

By Edmund Cautley, M.D.
(1) Lymphatism in a Boy ... 130
(2) Specimen of Tuberculous Tumour of the Dura Mater in a Child, aged 14 Months ... 130

Shortening of the Left Femur. By P. Lockhart Mummery, F.R.C.S. ... 132

The Radical Cure of Inguinal Hernia in Children. By Philip Turner, M.S. ... 133

February 23, 1912.

Athetoid Movements. By James Taylor, M.D. ... 141

By R. Hutchison, M.D.
(1) Paralysis of the Muscles of the Neck (? Poliomyelitis) ... 142
(2) Hysterical Vomiting and Achylia ... 144

Green Teeth, subsequent to a Prolonged Jaundice in the First Weeks of Life. By H. Thursfield, M.D. ... 147

Anomalous Jaundice, with Enlargement of Liver and Spleen, and Bile-stained Teeth. By Frederick Langmead, M.D. ... 148

Transposition of Viscera in a Girl, aged 12 Years. By Leonard Guthrie, M.D. 150

Unusual Cardiac Bruit. By J. A. Torrens, M.B. ... 153

By T. R. Whipham, M.D.
(1) Arthritis of the Shoulder and Hip (? Tuberculous) ... 154
(2) Proliferative Osteo-arthritis of the Hip in a Youth ... 156

March 22, 1912.

Leucodermia and Premature Canities. By J. L. Bunch, M.D. ... 159

Purulent Pericarditis. By J. Porter Parkinson, M.D., and Douglas Drew, F.R.C.S. ... 161

Chronic Jaundice and Splenomegaly. By Leonard Guthrie, M.D. ... 164

By G. A. Sutherland, M.D.
(1) Bilateral Deltoid Paralysis ... 165
(2) Cerebral Palsy ... 166

Subacute Arthritis of Shoulder-joint (due to an Organism of the *Bacillus enteritidis* Type). By Lionel E. C. Norbury, F.R.C.S. ... 166

Congenital Deformities in the Lower Limb. By Duncan C. L. Fitzwilliams, F.R.C.S. ... 168

Congenital Dyschezia. By Arthur F. Hertz, M.D. ... 169

April 26, 1912.

	PAGE
By O. L. Addison, F.R.C.S.	
(1) Bony Growth on the Skull	175
(2) Exostosis of the Inner End of the Clavicle	175
(3) Pathological Specimen of Tumour on the Back	177
Destruction of the Uvula in Vincent's Angina. By J. D. Rolleston, M.D. ...	178
By F. J. Poynton, M.D.	
(1) Morbus Cordis	182
(2) Rachitic Dwarf	184
Gumma of the Lung. By D. Forsyth, M.D.	186
Mucous Gastritis in Infancy. By Edmund Cautley, M.D.	188

Corrigendum.—On page 149, third line from the bottom, for "structures devoid of blood-vessels" read "structures rich in blood-vessels."

The Council think it right to state that the Society does not hold itself in any way responsible for the statements made or the views put forward in the various papers.

John Bale, Sons & Danielsson, Ltd., 83-91, Great Titchfield Street, London, W.

Section for the Study of Disease in Children.

October 27, 1911.

Dr. G. A. SUTHERLAND, President of the Section, in the Chair.

Case resembling General Paralysis of the Insane.

By J. PORTER PARKINSON, M.D.

A GIRL, aged 9 years, with a healthy personal history. The mother has four living children out of nine pregnancies. The first pregnancy terminated in a dead infant at the seventh month, the next lasted eight months, and the infant died when three days old. The third and fourth children are living. Patient is the fifth child. The sixth pregnancy miscarried. The seventh confinement produced a child who died, aged 10 weeks. The eighth was normal, and the child is living. The ninth was a miscarriage. The patient never had any illness, except slight snuffles in infancy; till four or five months ago she was noticed to be walking badly, especially on the right leg, and occasionally she turned giddy and faint. From being a good-tempered child she became irritable and spiteful, if she could not get her own way. She used to bite and kick people, and was generally uncontrollable. She used to steal things and then give them away. She did not spend money on herself, but if she saw anything she fancied she would not rest till she got it, and so she was caught stealing other children's possessions at school.

At the present time the child is fat and of a good colour. Her speech is indistinct and her mental processes slow. There are none of the usual stigmata of hereditary syphilis except the remains of an iritis from which she suffered three months ago. The pupils are excentric, and the left is larger than the right; they react sluggishly to light and accommodation; there is some congestion round each iris; there are no

fundus changes. There are no scars on the lips, and the teeth, though much decayed, are not characteristic. The movements of the limbs are rather unsteady, but there is no tremor of the hands, lips, or tongue. Grip is weak on the right side. The gait is uncertain, and there is a tendency to scrape the right toes on the ground. Romberg's sign has been present, but now she can stand well with closed eyes. The right knee-jerk is active, but difficult to obtain on the left; no ankle-clonus. The plantar reflex is extensor on both sides. The handwriting is good for her age. There are no sensory changes.

The Wassermann reaction was positive in the blood, but negative in the cerebrospinal fluid. Dr. Woodforde's careful report on the latter is as follows: "Fluid clear, no coagulum; albumin present, 0·07 per cent. (Aufrecht); globulin increased (Noguchi); glucose decreased, 0·125 per cent. Microscopically, lymphocytes only, 40 per cubic millimetre. No other cells and no organisms seen. Cultures sterile."

Patient was treated by mercurial inunctions in the Queen's Hospital for Children from July 3 to August 10, and there is a distinct general improvement.

Though this case resembles in most respects general paralysis of the insane, yet it is uncommon in that disease to find a negative Wassermann in the cerebrospinal fluid, no Argyll-Robertson pupil, no tremor, while the plantar reflex is extensor in type, a feature which is rare in general paralysis.

DISCUSSION.

Dr. F. PARKES WEBER said he believed that cases of general paralysis of the insane in children, and the cases of true syphilitic disease in children which resembled general paralysis of the insane, but were characterized by the presence of decided spasticity and of strongly marked Babinski sign, actually (from the clinical point of view) merged into each other, and for practical purposes they could not be clinically separated. For that reason he was in favour of every child showing symptoms resembling general paralysis being given the chance afforded by anti-syphilitic treatment.

Dr. LEONARD GUTHRIE said he did not think it was possible to say whether it was general paralysis of the insane or diffuse syphilitic cerebritis. There could be little doubt about the syphilitic element, although the child seemed to have none of the stigmata of the congenital form of that disease. On the whole he was inclined to regard the case as one of diffuse syphilitic cerebritis, of mild character, rather than as an ordinary case of general paralysis of the insane.

Dr. JAMES TAYLOR, while hesitating to call the case one of general paralysis of the insane, did not think anyone could say definitely that it was not. There were no signs of syphilis about the child, but the family history, and the recent occurrence of syphilitic iritis in the child, left little doubt as to the underlying cause of the condition.

Dr. PARKINSON, in reply, said he proposed to have another Wassermann test done on the blood, to see if it was still positive. If it should be so he would give a further trial to mercurial treatment.

Case of Infantilism.

By G. A. SUTHERLAND, M.D.

M. P., FEMALE, aged 16 years. History that patient seemed normal in every way until the age of 6 years, when growth ceased, rickets appeared, and constipation became very marked. Teething was normal as regards time but the teeth were very soft and crumbled away. Suffered from measles at the age of 2 years. The rickets led to a waddling gait and much bending of the bones, for which she has been under treatment at times at an orthopædic hospital.

(*a*) Infantilism: Appearance, talk, and intelligence, that of a child of 6 years. Only two teeth of second dentition.

(*b*) Persistent rickets: Bones of extremities much distorted, epiphyses thickened, chest rachitic.

(*c*) Abdomen very large and superficial veins prominent. At times peristaltic waves visible in the region of the colon. Bowels do not act without enemata or aperients. Great fæcal accumulation takes place, palpable over whole of abdomen when bowels not carefully regulated.

(*d*) Urine: Specific gravity 1000 to 1005; no albumin or casts; occasionally marked retention of urine without discomfort; polyuria; polydipsia; radial and brachial arteries thickened; arterial pressure, 120 mm.; no retinal changes; skin pigmented; heart somewhat dilated, but no marked accentuation of sounds. Wassermann reaction negative.

DISCUSSION.

Dr. SUTHERLAND said the question of infantilism was a complicated one, and sometimes one found one condition present and at other times another. His personal opinion was that if any organ of the body was not performing its

function properly it was possible that delayed development might be the result; for instance, in chronic disease of the lung, congenital disease of the heart, chronic bowel trouble, as well as in cases of kidney disease. The patient he now showed had several conditions present, any one of which might be associated with infantilism. She had had rickets, which he considered to be still persistent, although the active signs of that disease had gone. During the last fortnight she had been in the hospital for the purpose of getting rid of fæcal accumulation: there was an enormous accumulation, so that at one time the abdomen was twice its present size. The distension appeared to be more marked in the colon region, and there was sometimes visible peristalsis there. There was, he thought, though it was not perhaps fully marked, a condition of congenital dilatation of the colon. With regard to the motions, there was nothing special to note, and certainly there was not the condition which Dr. Langmead had described in connexion with a number of cases of dilatation of the colon. The motions were well coloured, and although they were hard and lumpy sometimes, there was nothing abnormal about their appearance. The other condition she had was that of the kidneys, in which he believed changes had taken place, and judging by the associated condition of the arteries, he suggested that contracted kidney might be present.

Dr. F. PARKES WEBER said he hoped the exhibitor would not mind his questioning the existence of organic renal disease in this case. He admitted there was polyuria, but a kind of diabetes insipidus was an occasional accompaniment of defective general or both general and mental development. He considered that the polyuria in the present case of infantilism was very likely merely a condition of diabetes insipidus. Probably further testing would either confirm or refute that idea. If, on the other hand, Dr. Sutherland's idea were correct, that there was organic renal disease—i.e., interstitial nephritis, contracted kidneys—present, he thought anyone might suggest that both the interstitial nephritis and the infantilism were due to inherited syphilis. The fact that the Wassermann reaction was negative did not negative the possibility of congenital syphilis. In spite of the accounts by Dr. Leonard Parsons[1] and Dr. H. Morley Fletcher,[2] he doubted whether chronic renal disease could ever by itself give rise to infantilism.

Dr. LANGMEAD suggested that the cases resembled most closely those described by Herter, of Chicago. Such patients showed a good deal of general abdominal fullness, with distension of the bowel, and infantilism. In several cases there was very marked rickets. There was one decided difference, however, between Herter's cases and that shown by the President, for in the former there was a leakage of proteid, carbohydrate, and fat in the stools, and especially a leakage of the salts of the alkaline earths. Further investigation of the stools might show a greater deviation from the normal than appeared.

[1] L. Parsons, *Brit. Med. Journ.*, 1911, ii, p. 481.
[2] H. Morley Fletcher, *Proc. Roy. Soc. Med.*, 1911, iv (Child. Sect.), p. 95.

The PRESIDENT (Dr. Sutherland), in reply, said that it appeared to him there were some definite changes in the arteries which would not be expected from the condition of diabetes insipidus. He did not wish to dogmatize about it being a case of contracted kidney. The condition of the bladder was very curious, because it dilated at times without discomfort to the patient. It could retain comfortably 1½ pints of urine, so possibly there was a cystic condition of the kidneys, as Dr. Parkes Weber had suggested. With regard to the suggestion that the kidney trouble might be due to congenital syphilis, he had thought of that, but the patient showed no stigmata and the Wassermann reaction was negative. With regard to intestinal infantilism of the Herter type, there was no evidence of disorder in that region, except the constipation. He believed the intestinal symptoms in this case were those usually associated with the dilatation of the colon. He could not find that there was any other evidence of intestinal disease or disorder, such as Herter had described in his cases.

Case of Extreme Rickets and Infantilism.

By G. A. SUTHERLAND, M.D.

R. F., FEMALE, aged 6 years. Child was born healthy, and fed on the breast for eight months; after that cow's milk and water and bread. Had a fright at the age of 2½ years and since then never strong. Has had measles and attacks of diarrhœa. Five other children in the family said to be healthy.

Child was in an extremely neglected condition when admitted (October 5), very exhausted, lethargic, and suffering from constant cough. Physical development was much retarded, her appearance being that of a child of 3 years. Marked rachitic changes in the bones of the upper extremities and of the thighs. Greenstick fracture of both bones of right forearm. Both scapulæ are small; show thickening of bone in lower part, and bending at inferior angle. Some of the upper ribs are subluxated from their costal cartilages and displaced backwards. Marked spinal curvature. Heart displaced, apex felt in upper part of left axilla. Abdomen contains some free fluid. Deficient entrance of air and impaired resonance, right lung, and some scattered bronchi over both lungs. Von Pirquet's reaction negative. Fingers clubbed. Great sweating, especially about the head.

Dr. SUTHERLAND said he thought that in the present day fewer cases of extreme rickets were seen than formerly. In the present case the marked delay in development was probably directly associated with severe and persistent rickets.

Sclerodermia with Myositis Fibrosa.

By F. Langmead, M.D.

The patient, now aged 1 year 8 months, was shown before this Section just twelve months ago, and is reported in the *Proceedings* for November of last year.[1] Since birth, or shortly afterwards, the skin has been noticed to be tense and hard, and the movements of the body, especially of the limbs, to be greatly restricted. She cannot completely close her eyes at times, whilst the mouth is usually open. The usual expressionless aspect of sclerodermia is present. Sclerodactyly is marked. The muscles are rigid also, and feel like leather bands, accounting for much of the fixity of the limbs. During the last twelve months she has grown considerably, and has seemed well in general health. The condition seemed to be improving until August, when she developed pneumonia, the result of which has been to accentuate it, as far as can be determined by examination.

Remarks.—The case is shown again because it serves as an example of, and an introduction to, a condition which is now beginning to be recognized, but is still very imperfectly understood. In it sclerodermia is associated with changes in the muscles. This association was first pointed out by Thibierge[2] in 1890, who described five cases. More recently Dr. Nixon,[3] of Bristol, has worked at the subject and has collected another fifteen cases from the literature and added four of his own, making twenty-four in all. These remarks are based, for the most part, on Dr. Nixon's paper. In such pathological investigations as have been made the muscle change would appear to originate as a myositis diffusa, going on later in some cases to a definite fibrous overgrowth, justifying the name myositis fibrosa. Dr. Nixon has shown that the serous membranes are occasionally affected. It seems that, in these cases, there is an infection, sometimes acute, sometimes subacute, at other times chronic, which affects subcutaneous tissue, muscles and serous membranes. In the skin it produces sclerodermia; in the muscles myositis, culminating in fibrosis and atrophy; and in the serous membranes polyorrhomenitis. It is questionable whether ordinary sclerodermia, unassociated with muscle changes, is the same disease.

[1] *Proceedings*, 1911, iv, p. 5.
[2] Thibierge, *Rev. de méd.*, Par., 1890, x, p. 291.
[3] Nixon, J. A., *Lancet*, 1907, i, p. 79.

DISCUSSION.

Dr. HUGH THURSFIELD said the case now shown reminded him of one which was under the care of Dr. F. E. Batten and himself last summer. This was in a much older child, one aged 9 years. She had what he thought might be described as "dermato-myositis," a class of case which had been a good deal studied in America. It commenced with erythema, which led to much desquamation and a certain amount of fever. This was succeeded by a slowly developed stiffening of many of the muscles. While the patient was under observation it was noticed that as each muscle became involved the skin over it assumed a sclerodermatous character—i.e., it could not be separated from the muscle and it lost its elasticity. It reminded one of very œdematous skin without admitting of pitting on pressure. Gradually the child wasted, and the parts of the skin which had been most affected by the erythema and the sclerodermatous condition became atrophied, and the girl finally died of an intercurrent affection of the kidneys. Probably there was sepsis of the kidney and the urinary tract generally. During the girl's lifetime a small portion of one of the affected muscles was removed for the purpose of making sections. It was found to be œdematous, and little else could be made out either microscopically or with the naked eye; all that one could say was that it seemed to be rather more cellular than normal muscle. Dr. Batten had removed a number of portions of muscle after death, but he had not yet heard if he had found any change. He did not know whether, as the muscles improved or altered, the condition of the skin also altered.

The PRESIDENT asked if he might take it that the fixation of the joints was regarded by Dr. Langmead as being due to myositis, and not simply to infiltration.

Dr. LANGMEAD, in reply, said he had seen the case which Dr. Thursfield referred to, and he thought the resemblance was very close. Possibly the cases described by American authors might be classed in the same group. If so, it meant that one more tissue, the epidermis, had been added to the many which were known to be involved in this particular form of disease.

Sclerema.

By F. LANGMEAD, M.D.

G. S., AGED 10 months, shows the more common type of hardening of the skin and subcutaneous tissues found in infants. She is the only child. The father is blind, and aged 37 years; the mother, aged 40 years. She was first brought to the hospital when aged 7 weeks, for

persistence of the posterior fontanelle, which was widely patent, and a patent occipital suture. These have now closed. In June last a brawny condition of the cheeks and legs was first noticed which has persisted since; it followed an attack of severe diarrhœa.

The case is shown to afford a contrast to that just described.

DISCUSSION.

Dr. REGINALD MILLER said he had seen two cases in older children of a peculiar scleremato us condition of the skin which came on after scarlet fever. He had asked one of the members of the Section, who was in the habit of seeing many fever cases, whether he had ever seen it follow scarlet fever, and he had replied in the negative. He wondered whether it was simply a coincidence that the scarlet fever had preceded the sclerema which appeared in the cheeks, buttocks, and thighs.

Dr. HUGH THURSFIELD said he had seen two or three of the cases which Dr. Langmead had referred to in which there had been evidence of a chronic staphylococcus infection. In one of them the baby had a sclerematous affection of the whole of the buttocks and back, and it continued for about six weeks or two months before it was discovered that the child had chronic periostitis. When that was treated and the staphylococcic infection cleared up the sclerema disappeared. He had seen the same condition in two other children, both of whom had chronic staphylococcic infection in different portions of the body. He had no opportunity of examining Dr. Langmead's child from that point of view, but probably there was no such thing to be found in that case. Still, it was possible to overlook it if one was not fully alive to the fact that chronic staphylococcic infections did occur, causing such curiously localized swellings in the skin.

Dr. LANGMEAD, in reply, said he could not claim to have gone thoroughly into the literature on the subject, but he had read some of it. It was necessary at the outset to distinguish between sclerodermia and sclerema. There were two types of the latter—sclero-œdema and simple sclerema. These appeared to be the same disease, differing a little clinically, in that the former was accompanied by œdema whilst the latter was not. He thought it was usually conceded that sclerodermia was a different disease, and not connected with sclerema. In true sclerema there was no cellular change, but there was fibrous tissue overgrowth in sclerodermia. Sclerodermia persisted; sclerema often disappeared. He did not think that because a baby recovered it indicated that it was the subject of "pseudo" sclerema. He knew nothing about its relation to scarlet fever.

Two Cases of Partial Subluxation of Knee-joints, with Voluntary Production of Noise during Flexion and Extension.

By O. L. ADDISON, F.R.C.S.

Case I.—C. R., aged 9 years, an undersized child. Marked genu valgum, exostoses at upper end of both tibiæ, and hyperextension of both knees. Flattening of chest, beaded ribs, and enlarged epiphyses at wrists. Breast-fed child. Quite well till four years ago, since then has had recurrent attacks of bronchitis. Measles, and pneumonia two years ago. The legs were only noticed to be crooked two years ago, and have been getting worse since.

Case II.—F. R., aged 6. Breast-fed. Healthy till 2 years of age; since then she has had frequent attacks of bronchitis. Measles, and pneumonia two years ago. Legs became bent after the measles, and have been much worse lately. Patient shows a condition like that of the brother, except that the genu valgum is not so great, no exostoses are present, and the noise produced in the knee-joints is not so loud.

DISCUSSION.

Mr. ADDISON asked for opinions as to the cause of the condition, and for suggestions as to treatment.

Mr. TUBBY said that the condition here described was often known as "snap-knee." The causation of it was doubtful, but he submitted for consideration a solution which might be correct. First, the condition of the knees was one of genu valgum. Secondly, there was great relaxation of the ligaments of the knee, so that there was considerable lateral movement. It was well known that in genu valgum the biceps and iliotibial band became contracted. On observing this knee-joint one heard a peculiar noise, and saw a peculiar movement at the end of hyperextension, and again at the end of flexion. It was his opinion that the causation of the phenomena was the contraction of the biceps and the iliotibial band at a certain stage in the movement of the joint. The biceps was normally a flexor of the joint, but if hyperextension of the knee-joint was present it became an extensor, once the joint had passed the absolute plane and the tibia had become hyperextended. What happened, he thought, in these cases was that at the extreme of extension, or on the occurrence of hyperextension, the biceps suddenly came into action, pulling the tibia outward and grating its surface against the condyles of the femur. At

the extreme of flexion the biceps had attained its greatest tension and again pulled the tibia outward on the femur, and gave rise to the curious click which was heard. He did not think the sound could be regarded as due to exostoses, or to a slipping patella. Although there was excessive lateral movement of the patella, yet this was not accompanied by the click.

Mr. ADDISON, in reply, said he thought Mr. Tubby's explanation was a very good one, and the most likely to fit the case. He had nothing to suggest further.

Intra-uterine Fracture of Tibia and Fibula, with Absorption of Bone.

By P. MAYNARD HEATH, M.S.

BABY, 4 weeks old. At birth the left leg was noticed to be shorter than the right below the knee. On examination the shortening is very obvious. There is a bowing forward of the leg at the junction of lower and middle thirds and a corresponding groove in the soft parts on the posterior aspect. The bones in the situation of the prominence can be felt to be irregular and there is free mobility between the upper and lower portions of the leg at this point. The circulation in the foot is good, and there are no other deformities. The child was born head first without any difficulty, and no structure was found constricting the leg. X-rays show a fracture of the shaft of the tibia, with a considerable area of rarefied bone around it (*see* figure). There are similar areas of rarefaction about the lower extremities of the tibia and fibula.

DISCUSSION.

Mr. HEATH added that the child showed none of the stigmata of congenital syphilis, but he had not yet had the opportunity of having a Wassermann reaction done. This child was the eighth in the family: two children had died of undetermined causes. As he had not been able to question the parents, the history was at present unsatisfactory. The condition was noticed immediately after birth, and the groove in the soft parts was said to have been more marked than at the present time. There was no evidence of an amniotic band having constricted the limb. He would be glad to hear opinions as to treatment. He was averse to operative interference, and had merely applied a splint.

Mr. TUBBY said the case was of great interest to him. He had seen about thirty specimens of the kind during the last twenty years, a sufficient number to enable him to classify them. He would do so as follows: They were cases

of either congenital deformity or congenital fracture of the tibia, and he would place in one class those in which both bones were present, and in another class those where one bone was absent—namely, the fibula. As subdivisions in each class he would put down, first, those in which there was no fracture, but only curvature; and secondly, those in which both curvature and fracture were present. The striking point about Mr. Heath's case was that it differed from the majority of the cases he had seen in the fact that there was no dimple and



Intra-uterine fracture of tibia and fibula.

no scar over the front of the tibia. He made that remark in order to draw attention to what had been considered to be the theory of production of those cases. The generally accepted theory was that the curvature of the tibia was due to the adhesion of an amniotic band, which drew the tibia forward, or bound it down to the uterine walls, and in almost all cases one would find a dimpling or scar, which was at the summit or most marked part of the curve and where the band had been adherent. With regard to treatment, no one who knew anything about the condition would attempt to deal with it surgically. He had done many osteotomies and he had only one case with failure of union afterwards. It was a case of congenital curvature of the tibia, and he divided it with the object of putting the bone straight. It failed to unite. He

remembered eighteen years ago seeing a case which had come under the notice of a colleague, and in which a surgeon had performed a similar operation. An attempt was made by his colleague to graft a rabbit's bone. Finally the limb was amputated, and there was found to be acute fatty degeneration of every tissue below the site of the fracture. He urged that no attempt should be made to operate on this case. His advice was to treat it only with orthopædic apparatus.

Gigantism of Forepart of Foot.

By P. Maynard Heath, M.S.

Girl, aged 6 years. At birth a deformity of the right foot was noticed. At the age of 3 months the enlarged second toe was amputated.



Gigantism of forepart of foot.

Since then the forepart of the foot has grown more than the rest of the body. The great toe is now deflected inwards, but not much enlarged. The third toe is very large and deflected outwards. Between these two toes is a rounded, elastic, firm swelling, more pronounced on the sole

than the dorsum, and in the former situation extending almost to the middle of the foot. At the summit of the swelling is the scar of the previous operation. There are no other deformities and no history of similar overgrowth in any members of the patient's family.

DISCUSSION.

Mr. HEATH said that he proposed to try to remove a wedge from the forepart of the foot, so that the child might be able to wear a boot.

Mr. TUBBY said he regarded this as an example of congenital diffuse lipoma of the foot. He was much interested in these cases because they presented peculiar characteristics. It would be noticed that in this case a toe had been amputated for hypertrophy, and a fatty swelling had appeared in its place. The point about those curious cases of congenital diffuse lipoma, which must be distinguished from acquired painful lipoma, was that they had the features, clinically, of malignant disease, but histologically they had the characteristics of healthy tissue, with one exception—namely, that the fat lobules were larger than usual, and between the fat lobules and the stroma a few round and spindle cells were in evidence, but the appearance was not at all like that of spindle-celled sarcoma. He had sections cut, which he submitted to Mr. Bland-Sutton, who gave it as his opinion that there was no microscopical evidence of malignant tissue in the fat. He had operated upon several of these cases. The fat was dissected out, but the swelling invariably returned. If one amputated the toe, the condition might either come back, or several masses might appear at a distance, and coalesce, with the result that there was a repetition of the growth. The only thing to be done was to amputate at a great distance. In the present case he would not hesitate to amputate the foot at the ankle; because otherwise the condition was almost certain to recur.

A Case of Double Third Nerve Palsy due to Acute Poliomyelitis.

By A. H. PAYAN DAWNAY, F.R.C.S.

F. L., MALE, aged 7 years.

Past history: The patient is said to have been very healthy until last November, when he had a bad attack of measles, with broncho-pneumonia; subsequently in March he was in hospital with scarlet fever for six weeks. There is no history of diphtheria. In July he was at Margate and his eyes are said to have been inflamed.

Present illness: About the third week in August he went to Holwell, near Hitchin, and was taken ill with vomiting; the temperature rose to

103° F., there were constipation and some delirium at night. The temperature remained high for three or four days, and he was in bed about a fortnight. He is said to have told a playmate just before he was ill that he saw double. He was seen by me at hospital on September 9, when his condition was as follows: Marked ptosis on both sides, elevation of the lids only by action of the frontalis, no power of elevation or depression of the globes, fair lateral movements, with complaint of occasional diplopia to the right. Pupils equal and active directly and indirectly, no paralysis of accommodation. Vision of each eye was $\frac{6}{12}$ one letter, and J1 at 12 in. There were no other signs of paralysis, hand-grip was good, and knee-jerks were easily obtained.

The patient was seen by Dr. L. Guthrie, who agreed that the probable cause of the condition was acute poliomyelitis. He has much improved in health since first seen, and there is now a very slight amount of upward movement of the globes.

DISCUSSION.

Mr. DAWNAY referred to a paper read on October 13, before the Clinical Section,[1] on an epidemic of poliomyelitis occurring in Cornwall. Some cases were recorded in which the only muscle affected was the external rectus.

Mr. SYDNEY STEPHENSON said the present case seemed an unusually typical instance of the condition which he described before this Section some months ago.[2] It was even more typical than any of the cases included in his own paper.

Recurrent Unilateral Œdema.

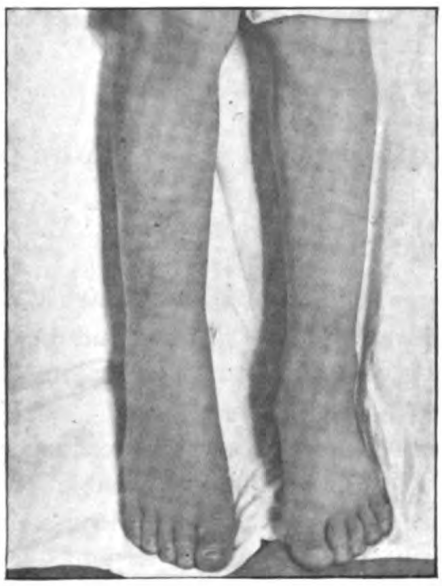
By T. R. WHIPHAM, M.D.

THE patient is a boy, aged 14 years, small and of a degenerate type. According to the history the right leg has swollen once a year, generally in the month of September, for the last three or four years, but during the present year the œdema has occurred on two or three occasions. There has twice been swelling of the right arm—in 1909 and 1910—but not at the same time that the leg was affected. The swelling of the leg starts with pain in the inner side of the thigh, which goes down to

[1] *Proceedings* (Clin. Sect.), p. 28.
[2] *Proceedings*, 1911, iv, p. 87.

the foot. Puffiness then appears over the dorsum of the foot and gradually extends upwards to the knee. At the same time patches of skin over the affected area become red and covered with "blisters." The œdema lasts about a fortnight, as a rule, and disappears gradually.

The last attack began on September 29, when the right foot and leg were noticed to be swollen and the right side of the face to be a little puffy. When seen on October 4 the leg presented considerable œdema, which was soft, and pitted on pressure, and was especially marked over the foot and ankle. On the inner side of the calf there was a purpuric patch on the skin, raised and irregular on the surface, giving the sensa-



Recurrent unilateral œdema.

tion to the fingers of obstructed lymphatic vessels. No vesication was present. The extremities were all cold and of a bluish colour, these features being especially marked in the right leg. The right hand was a little puffy, and the same was the case with the right cheek, more especially below the eye. The nose, cheeks, and ears were congested, and both lower eyelids were swollen. On the left ear was a small scar, apparently the result of some old ulceration, causing the helix at this spot to be adherent to the antihelix. The purpura and œdema gradually subsided in the course of ten days or a fortnight, but there is still some swelling of the right ankle and foot and of the right hand to be seen,

which the patient states to be permanently present. The condition when the patient was first seen is shown in the illustration. The extremities are cold and bluish and the face has a congested, puffy appearance, the swelling of the lower eyelids being specially marked. The viscera are normal, and the urine contains no albumin. The temperature has been normal throughout. The patient has five brothers and four sisters, but there is no history of any similar condition in the family.

In spite of the existence of what seemed to be a localized obstruction of the lymphatics in the calf of the leg, the condition appears to be due to a vasomotor spasm, in view of the fact that the œdema occurred in other parts than the leg. It is perhaps akin to angioneurotic œdema or an early stage of Raynaud's disease.

Diphtheria of Œsophagus.

By J. D. Rolleston, M.D.

Boy, aged 2 years 4 months, was admitted moribund to hospital on October 14, on eleventh day of disease, and died in seven hours. No antitoxin had been given, diphtheria not having been diagnosed until day of admission.

Condition on admission: Profound toxæmia, pulse imperceptible, temperature 97·6° F. Old membrane visible on tonsils, uvula, and epiglottis, pronounced oral fœtor, profuse blood-stained nasal discharge. Slight stridor, croupy cough, and recession. Upper part of right external ear swollen and excoriated. Sloughing wound just below left external malleolus. Cultures of throat, ear, and wound all showed abundant Klebs-Löffler bacilli.

Specimen A, Œsophagus: Upper third normal; middle third shows some injection of mucosa; in lower third are two longitudinal areas of necrosis measuring each 3·5 cm. in length, coalescing below, where they measure 2·2 cm. in width, and stopping just short of the lower end of the œsophagus. In the centre of one of the areas the muscular wall of the œsophagus is exposed. Direct smears and cultures from the necrotic areas showed numerous Klebs-Löffler bacilli.

Specimen B shows remains of membrane on and varying degrees of necrosis of tonsils, uvula, pharynx, epiglottis, valleculæ, ary-epiglottidean folds, and interior of larynx. Trachea normal.

Involvement of the œsophagus in diphtheria is a rare event. When it does occur it is usually associated with multiple lesions elsewhere, as in the present case, in which the fauces, nostrils, pharynx, larynx, and skin were also affected. The condition cannot be diagnosed during life except by expulsion of a cast of the œsophagus, or by subsequent development of œsophageal stricture in cases which survive. As a rule, as in this case, it is a necropsy surprise.



Diphtheria of œsophagus (× $\frac{3}{4}$).

The Museum of the Royal College of Surgeons contains only two specimens of the kind: the one presented by Dr. Goodhart in 1875 (No. 2,292 in Catalogue), and the other by Dr. E. W. Goodall in 1896 (No. 2292A).

The literature of the subject shows that the diagnosis of diphtheria of the œsophagus was much more frequently made in the pre-bacteriological era than at present, often indeed on purely clinical grounds—e.g., difficulty of swallowing and vomiting, as in cases reported by Greenhow [11], Burdon Sanderson [20], and Gull [12]. My own

patient had great difficulty in swallowing, but not more than is usually found in severe faucial and laryngeal diphtheria. Although post-mortem evidence was present in the cases reported by such well-known authorities as Bretonneau [2], Bristowe [3], Jacobi [13], Jenner [14], Sanné [21], and Morell Mackenzie [19], who may be accredited with a correct diagnosis in spite of the absence of bacteriological confirmation, it is not improbable that some of the early cases in which membrane was found in the œsophagus at the necropsy were due to the action of concentrated hydrochloric acid which was once much in vogue for the local treatment of diphtheria.

Fry [8], of Washington, in 1885, collected fourteen cases of diphtheria of the œsophagus, including one of his own. In seven, diphtheria was the primary disease, and in seven it was secondary to scarlet fever, pneumonia, tuberculosis, or other diseases. The membrane involved the mucous surface of the œsophagus either as a tubular lining or in bands prolonged to the cardia in seven cases. Only two of the fourteen expelled an œsophageal cast.

Since the publication of Fry's paper, which appeared prior to the general recognition of the diphtheria bacillus, I can find records of only eight cases. In five the diagnosis was established on bacteriological and post-mortem findings (Goodall [10], Cautley [4], Fawcett [6], Leathem [17], Field [7]), only one of which (Field's case) expelled an œsophageal cast before death, and in three, which recovered, the diagnosis was based on the presence of a stricture of the œsophagus which developed shortly after an attack of diphtheria (Korczynski [16], Jungnickel [15], Danielsen [5]). A striking feature in the present case is the marked destruction of tissue which is present, not only in the œsophagus, but also in the pharynx.

Although neither of the specimens already alluded to in the Royal College of Surgeons' Museum shows any such changes, necrotic lesions in the œsophagus have been met with in other cases, and are due to secondary infection, especially by cocci.

It is readily conceivable that, had my case recovered, cicatrization of the lesions would have led to stricture. Post-diphtheritic stricture of the œsophagus, though an extremely rare occurrence, has been recorded in the pre-bacteriological era by Gendron [9], Leube and Penzoldt [18], and Trendelenburg [22], and more recently by Korczynski, Jungnickel, and Danielsen. In Korczynski's case perforation of the œsophagus occurred, as shown by the sudden appearance of subcutaneous emphysema in the neck. In all the cases recovery followed gradual

dilatation of the stricture by bougies. In some of the cases recorded the obstruction to the passage of food may have been due merely to diphtheritic paralysis, but in others an undoubted cicatricial stenosis, sometimes multiple, was found, for which no other cause than diphtheria could be discovered.

In conclusion, it may be noted that in children already suffering from œsophageal stricture as the result of swallowing lye, an attack of diphtheria is apt to be very severe. Jacobi and Baginsky [1] have each recorded fatal cases. The œsophageal scars in both cases were invaded by diphtheritic membrane, the frequent use of bougies having predisposed the stricture to infection. There was no history of tube-feeding in my case.

REFERENCES.

[1] BAGINSKY, A. "Diphtherie," 1898, p. 202.
[2] BRETONNEAU, P. "Memoirs on Diphtheria," *New Sydenham Soc.*, 1859, ii, pp. 18, 77.
[3] BRISTOWE, J. S. *Trans. Path. Soc. Lond.*, 1859, x, p. 333.
[4] CAUTLEY, E. *Rep. Soc. Study of Dis. in Child.*, 1902-03, iii, p. 234.
[5] DANIELSEN, W. *Beitr. z. klin. Chir.*, Tüb., 1909, lxiii, p. 257.
[6] FAWCETT, J. *Trans. Path. Soc. Lond.*, 1906, lvii, p. 336.
[7] FIELD, F. E. *Lancet*, 1907, i, p. 161.
[8] FRY, H. D. *Amer. Journ. Med. Sci.*, Philad., 1885, n.s., xc, p. 329.
[9] GENDRON. *L'Union médicale*, Par., 1860, n.s., v, p. 569.
[10] GOODALL, E. W. *Trans. Path. Soc. Lond.*, 1896, xlvii, p. 39.
[11] GREENHOW, E. H. "Diphtheria," 1860, pp. 183-85.
[12] GULL, W. "Second Report Medical Officer Privy Council," 1860, p. 300.
[13] JACOBI, A. "Treatise on Diphtheria," 1880, p. 83.
[14] JENNER, W. "Diphtheria: its Symptoms and Treatment," 1861, p. 8.
[15] JUNGNICKEL, H. *Prag. med. Wochenschr.*, 1903, xxviii, p. 489.
[16] KORCZYNSKI. *Centralbl. f. klin. Med.*, Leipz., 1883, iv, p. 542.
[17] LEATHEM, A. *Rep. Soc. Study of Dis. in Child.*, 1905-06, vi, p. 239.
[18] LEUBE and PENZOLDT. Quoted by FRY, loc. cit.
[19] MACKENZIE, MORELL. "Manual of Diseases of Nose and Throat," 1884, ii, p. 69.
[20] SANDERSON, BURDON. "Second Report Medical Officer Privy Council," 1860, p. 267.
[21] SANNÉ, A. "Traité de la diphthérie," 1877, p. 64.
[22] TRENDELENBURG. Quoted by FRY, loc. cit.

Aneurysm of the Descending Branch of the Right Coronary Artery, situated in the Wall of the Right Ventricle, and opening into the Cavity of the Ventricle, associated with great Dilatation of the Right Coronary Artery and Non-valvular Infective Endocarditis.

By R. Salusbury Trevor, M.B.

The specimen was obtained from the body of a girl, aged 11 years, who was admitted into St. George's Hospital, on September 4, 1911, under the care of Dr. Latham.

The history of the case was as follows: Seven days prior to admission she caught a "chill" while bathing. This was followed by a rigor, sweating, &c., pain in the left knee. For three days prior to admission there was soreness of the throat, and the legs became swollen. The only previous illness was measles six years ago.

On admission the child was flushed and restless, with a temperature of 102° F., pulse 108, and respiration-rate 28. There was no pain in or swelling of the legs. The heart's apex beat was diffuse, in the sixth space, 1 in. external to the nipple line. The area of cardiac dullness extended 1½ in. to the left of the nipple line but not to the right of the sternum. A rough systolic mitral murmur and thrill were present. The murmur was best heard 1½ in. internal to the nipple line. The lungs and abdomen were natural.

On September 8, four days after admission, a red, tender swelling appeared at the right great toe joint. There was no reaction to salicylates. Temperature 104° F. to 105° F. The heart condition was as on admission.

On September 11 streptococci were found in the blood and a vaccine was prepared. Polyvalent serum was given in the meantime.

On September 14 a to-and-fro cyclical murmur, which was very rough and scratchy, became audible, and was best heard over the tricuspid area, but was conducted over the entire præcordium.

On September 16 there was evidence of rapid dilatation of the heart, temperature remaining about 104° F.

On September 19 the patient died.

The diagnosis made was infective endocarditis and pericarditis.

I examined the body thirteen hours after death. It was fairly well nourished. The legs were slightly œdematous. Thorax: There was bilateral sero-fibrinous pleurisy with a small quantity of turbid effusion in each pleural cavity. Both lungs overlapped the pericardium, to which the pleura was lightly adherent. Both lungs were œdematous and contained numerous septic infarcts. There was general bronchitis. The infratracheal and bronchial glands were free from tubercle. In many of the intrapulmonary branches of the pulmonary artery there were tough adherent ante-mortem clots. The pericardium was normal.

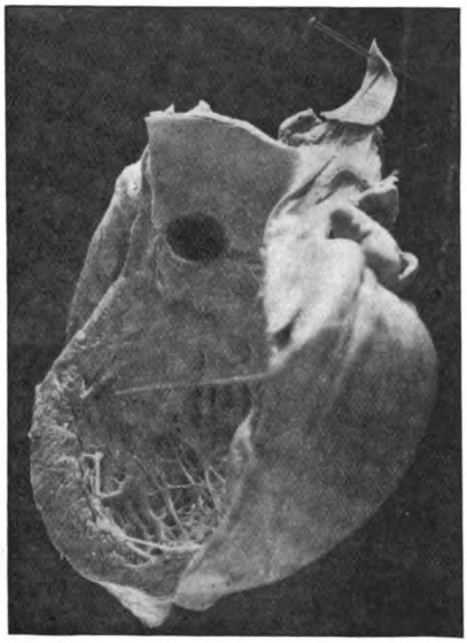


FIG. 1.

Cavity of the left ventricle, showing the large orifice of the right coronary artery.

The heart weighed 10 oz. and was rounded in shape. Both ventricles, as seen in the specimen, are dilated, the right one being especially so. The muscle in the fresh state was cloudy and pale. The heart-valves are free from vegetations, and they are all thin and flexible, with the exception of the posterior flap of the mitral, which shows slight thickening. In the cavity of the right ventricle, just to the inner side of the anterior papillary muscle, is an adherent mass of clot, guarding at its lower end a rounded or oval opening, with a maximum diameter of

¼ in., leading into the interior of a prominence on the postero-lateral wall of the ventricle. The edges of the opening are rough, and the surrounding endocardium appears ulcerated. The prominence is caused by a thin-walled fusiform aneurysm of the descending branch of the right coronary artery, situated within the muscular wall of the right ventricle. The sac is of about the size of a damson or a small plum, and the lining membrane bears some rough adherent clot at its upper part and is ulcerated below at the opening into the right ventricle. The right coronary artery is remarkably dilated. Its opening in the right sinus of Valsalva measures ½ in. across and admits the little finger easily, and the lumen continues wide to the point where the descending aneurysmal branch is given off. Beyond this point the lumen ends almost blindly, two fine holes indicating the continuation of the vessel along the auriculo-ventricular furrow. The left coronary artery is normal at its commencement and shows no obvious dilatation. The aorta above the valves shows a few patches of atheroma. Its branches were given off normally and no abnormalities in the rest of the vascular system were discovered. The abdominal organs showed cloudy swelling, and there was no ascites. The right knee-joint was healthy; the right great toe joint was unfortunately not examined.

Remarks.

Aneurysm of the coronary arteries of the heart is an uncommon lesion, and one which in consequence receives but scanty treatment in the text-books. This is the first case which has occurred during the last ten years in St. George's Hospital among a little over 3,000 post-mortem examinations.

In the *Transactions of the Pathological Society* there are records of three cases, two being cases of multiple aneurysms, and one of a single aneurysm of the left coronary artery in a man aged 50. In this case the artery showed extensive calcareous changes and the aneurysm was practically filled with clot. In the case described the physical signs suggest that the communication of the aneurysm with the right ventricle occurred five days before death, and coincided with the onset of the to-and-fro murmur which was thought to have been due to pericarditis. The date of the occurrence of the aneurysm itself can only be a matter of speculation. The impression which has been left on my mind is that it is of old date. It is on the whole smooth-walled, but the wall is remarkably thin. No communication between the artery and any of the cardiac veins can be made out.



FIG. 2.

Back of right ventricle, showing the dilated coronary artery and the aneurysm (laid open) on the descending branch.

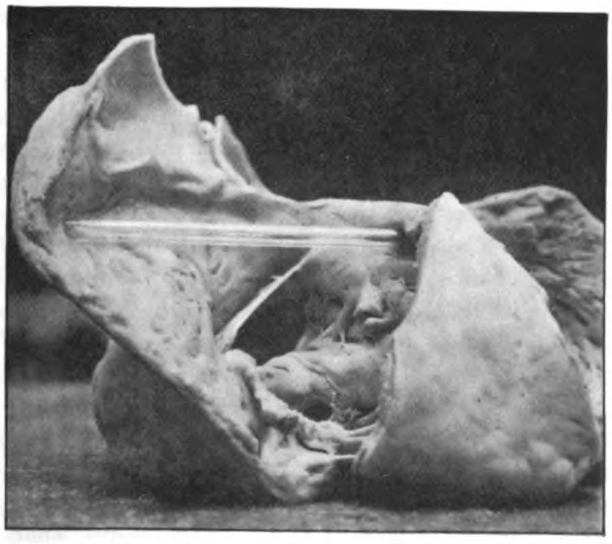


FIG. 3.

Interior of right ventricle. The round dark spot is the opening between the ventricle and the aneurysm. Above this is the tricuspid orifice.

With regard to the remarkable dilatation of the coronary artery itself, it does not seem probable that this can date from the time of rupture of the aneurysm into the ventricle. Yet it seems certain from the differences of pressure within the two ventricles that at this time the blood must have passed from the left ventricle via the coronary artery into the right ventricle, and this being so, it may be that there was a larger blood-flow through the artery. This may have led to the dilatation; but the length of time, viz., five days, does not seem sufficient to have allowed any dilatation to have assumed the size found post mortem.

The only other explanation I can offer is that the artery was anomalous at birth. If this were so, it would favour the entrance into it of septic emboli, which might perhaps account for the aneurysm found. The condition present produced clinically a murmur in many respects similar to that associated with persistent ductus arteriosus.

My thanks are due to Dr. Latham and Dr. Golla for permission to show the specimen.

BIBLIOGRAPHY.

Bristowe. *Trans. Path. Soc.*, vii, p. 98.
Jackson Clarke. *Trans. Path. Soc.*, xlvii, p. 24.
Peacock. *Trans. Path. Soc.*, i, p. 227.

DISCUSSION.

Dr. Trevor said he showed the specimen with the idea that members might be able to give him some help with regard to the causation of the condition. The dilatation of the coronary artery in this case was remarkable, and he had been unable to find among the records a case resembling it. The appearances in the artery in the specimen now were the same as he saw them at the autopsy. The vessel was white and smooth-walled, and there was no evidence to the naked eye of any acute inflammatory change in the wall. Histological examination of the vessel wall showed no evidence of acute endarteritis. The history of the case extended to only twenty-two days from the commencement of the illness, which was seven days before the child's admission to the hospital. Apparently, from the history, she had symptoms pointing to some septic infection, following what was said to be a chill. The opening which was present in the ventricle was of recent date, and corresponded with the to-and-fro cyclical murmur which had been mentioned. The alternative explanation he had put down in the notes, that the artery was anomalous at birth, seemed to be rather begging the question. He would be very glad of suggestions which might help to clear up the nature of the case.

Dr. EDMUND CAUTLEY said he did not think it was possible to make further suggestions than those which Dr. Trevor had made already. The explanation seemed a very probable one, that at some time there had been an infection which gave rise to aneurysm of the coronary artery. Whether that artery was abnormally large to start with was uncertain, because in the whole of his experience he had not heard, seen, or read of such a congenital dilatation. He thought it likely that at some time the child had had an infective embolus, carried by the coronary artery, which had caused the aneurysm. Possibly there might have been arteritis and some softening of the wall, and dilatation had occurred at the same time as the aneurysm. Subsequently, weeks or months later, the aneurysm burst and set up secondary infective endocarditis. But that was pure hypothesis.

Dr. PARKES WEBER said that although this case was doubtless absolutely unique, he thought one was justified in being almost certain that the dilatation of the left coronary artery was congenital. It was inconceivable that during the child's short illness, however much the walls of the arteries were softened, they could have dilated so much as to form an enormous artery of the size of a man's little finger. Moreover, if one supposed that this condition of dilated coronary artery was congenital, it offered an explanation of what happened when the child became infected with the sore throat, &c. As a *locus minoris resistentiæ* the congenitally diseased artery caught up the septic infection, as congenitally diseased valves frequently do, and a condition of malignant septic inflammation was started. A specially interesting point in the description of the case was the following: "On September 14 a to-and-fro cyclical murmur, which was very rough and scratchy, became audible, and was best heard over the tricuspid area, but was conducted over the entire præcordium." That was the kind of churning, rumbling murmur, extending throughout the whole cardiac cycle, which, with its intensity in another position (namely, the pulmonary area), had been described by Dr. G. A. Gibson,[1] of Edinburgh, and others, as typical of patent ductus arteriosus. In Dr. Trevor's case the position of greatest intensity was in the tricuspid area—an area, so far as he (Dr. Weber) knew, hitherto not supposed to supply that kind of murmur. It was of interest, too, that this was the second time in the present year, at this Section, that a pan-cyclic murmur had been brought forward which was not due to patent ductus arteriosus. He referred to a case shown by Dr. T. R. Whipham,[2] in which the murmur was in the same position as that described by Dr. Gibson. At the necropsy on Dr. Whipham's case it was discovered that the murmur must have been due to a vegetation from the pulmonary artery beyond the valves, hanging back and separating the valves. He had seen a case (an adult) in which a murmur probably similar to that in

[1] G. A. Gibson, *Edin. Med. Journ.*, 1900, n.s., viii, p. i.

[2] T. R. Whipham, *Proceedings*, 1911, iv, pp. 31 and 199.

Dr. Whipham's case was heard.[1] The disease was obviously of a malignant, septic type. At the post-mortem examination, however, there was no actual malignant endocarditis; the pulmonary valves were not themselves diseased, but there was a malignant endarteritis of the pulmonary artery outside the heart, and there was a rough vegetation a little beyond the valves. Perhaps the murmur in that case was caused in the same way as in Dr. Whipham's case, by a vegetation hanging back and separating the valves, though after death only the base of the vegetation was found in situ.

Dr. CHARLES W. CHAPMAN said the argument against the idea of dilatation was that there was no thinning of the vessel, which was very thick. The thickness before dilatation must have been very great.

Congenital Morbus Cordis (Cor Biatriatum Triloculare).

By R. SALUSBURY TREVOR, M.B.

THE specimen was obtained from the body of a male infant aged 4 days. The child was of good colour and showed no evidence of disease during its short life. It died suddenly in a "fit" after looking "very ghastly."

At the necropsy the body was generally dusky (cyanosed) in colour, and the finger- and toe-nails were purple. The lower lobes of both lungs were collapsed and congested, and there was much sticky mucus in the tubes. The thymus was small. The pericardium appeared normal. The heart lay in its natural position. The right ventricle formed practically the whole of the anterior aspect of the heart. The right auricle was prominent, and the left auricular appendix appeared purely rudimentary compared to the right one. Only one arterial trunk left the ventricular part of the heart.

The various anomalies present are as follows:—

(1) There is only one ventricular cavity, formed largely from what should be the right ventricle. There is a superficial interventricular furrow on the outside, but no septum within the cavity. At the apex is a small ridge of muscle tissue, which is produced by a fusion of papillary muscles, and is not a rudimentary septum.

(2) There is but one auriculo-ventricular valve, and that a valve with three flaps communicating with the right auricle.

[1] See K. Fürth and F. P. Weber, "A Case of Malignant Pulmonary Endarteritis after Gonorrhœa," *Edin. Med. Journ.*, 1905, n.s., xviii, p. 33 (especially p. 35).

(3) There is but one main arterial trunk which takes the course of the aorta, arching over the left bronchus and giving off the following branches in order: (*a*) The right and left pulmonary arteries. (*b*) The innominate artery which arises from the top of the arch just to the left of the trachea. From this point it ascends *in front* of the trachea to reach the right side of the trachea. As it does so it gives off the right subclavian. (*c*) The left common carotid. (*d*) The left subclavian.

(4) The right auricle is very large, and has its normal openings. The septum between it and the left auricle is imperfect, and at its upper and posterior part is a large defect, a patent foramen ovale. At the lower end of the septum in front is another small opening between the auricles.

(5) The left auricle is rudimentary, and its only outlet is via the deficient septum into the right auricle. There is no communication between it and the single ventricle. The left auricle receives two sets of veins from each lung, each set apparently fused into a single trunk.

(6) From the sinuses above the valves guarding the single arterial trunk there are no openings for the coronary arteries.

(7) A single branch is given off from the innominate artery just at its commencement, which runs downwards on the anterior aspect of the heart, just to the right of, and partly bound down to, the main arterial trunk. Before reaching the heart the artery divides into two branches. One of these runs downwards between the main arterial trunk and the right auricular appendix to reach the furrow between the right auricle and ventricle; the other passes downwards behind the main arterial trunk to reach the superficial furrow between the right and left sides of the ventricle. This is the coronary artery of the heart, arising as a single stem from the innominate and dividing into two branches.

There were no other abnormalities in the body, and, except for some altered blood in the stomach and general congestion, there was no pathological change.

The anomalies underlying this specimen are, failure of development of both the ventricular and aortic septa, and incomplete closure of the ostia of the auricular septum. As a consequence, the original common auriculo-ventricular orifice persists, and there is but one arterial trunk leaving the solitary ventricle. The defect in the auricular septum is due to persistence of the ostium secundum, and, to a lesser extent, of the ostium primum. The presence of this septum, however defective,

places the specimen in the class of three-chambered hearts, though for practical purposes it is a two-chambered one, as the only outlet of the rudimentary left auricle is via the defective septum into the right auricle. Two-chambered hearts, according to Dr. Maude Abbott,[1] are of extreme rarity. Of three-chambered hearts with two auricles and one ventricle, Dr. Abbott refers to 37 cases. In 31 of these cases collected by Arnold, there was only one arterial trunk in 6, and a defective auricular septum in 18. The origin of the coronary artery and its course are remarkable.

Night-blindness with peculiar Conjunctival Changes in Children.

By Sydney Stephenson, C.M.

At a meeting of the Section held on May 26 last I exhibited a little boy, aged 5 years, who was brought by his mother to the Queen's Hospital for Children, because he could not see at night, and had "white spots" on his eyes. Examination of the blood showed that hæmoglobin was 68 per cent.; red cells were 3,900,000, and white cells 8,000 per cubic centimetre. The colour index then was 0·87.

The interest excited by the foregoing case led me to believe that a brief description of this curious condition, which does not seem to be at all generally recognized outside the ranks of the ophthalmic surgeons, might not be wholly devoid of interest to members of the Section. Yet it is not altogether uncommon in the neighbourhood of London, provided one knows where to look for it, and looks for it at the right season. It prevails in children belonging to the lower strata of society—as, for example, such as are to be found as inmates of Poor-law schools, orphanages, and so forth. In order to give my hearers some idea of its prevalence in those places, I may say that I found it to be present in 1·87 per cent. of 6,209 presumably healthy children whose eyes I examined a few years ago.[2]

On the other hand, in my experience it is not at all common in children's hospital work, mainly, I suppose, because the affected children seldom make any complaint of their eyes, and also because the hospital

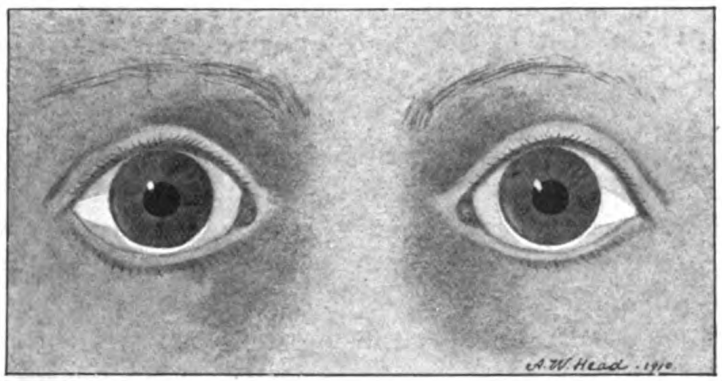
[1] Abbott, Osler and Macrae's "System of Medicine," 1908, iv, p. 360.

[2] *Trans. Ophthal. Soc. U.K.*, 1898, xviii, p. 55.

class, as a class, are notoriously unobservant. At the same time a number of cases have been reported from Sheffield Infirmary by Simeon Snell,[1] and a few cases from the London Hospital by Stephen Mayou[2] and by Malcolm L. Hepburn[3] respectively. Speaking for myself, I have seen occasional cases in the ophthalmic department of the Queen's and the Evelina Hospitals for Children; but I have yet to meet with a case among children of the better class—i.e., such as would be likely to consult us in private practice.

It prevails in summer and autumn, and is seldom seen during the winter months.

The symptom-complex, in its fully developed form, includes, first, changes in the ocular conjunctiva, and secondly, night-blindness.



Night-blindness with peculiar conjunctival changes.

This conjunction is sometimes spoken of as "Bitot's syndrome," despite the fact that it was described, though perhaps not very fully, by de Hubbenet, a Russian army surgeon, in 1860,[4] that is, three years before Bitot[5] wrote.

The conjunctival changes are seldom seen except in that part of the ocular conjunctiva which is exposed when the lids are open, the so-called "interpalpebral zone" (E. Fuchs). They usually affect both eyes, sometimes to an unequal extent. They occur as more or less triangular areas (often situated one on each side of the cornea), which are dry,

[1] *Trans. Ophthal. Soc. U.K.*, 1881, i, p. 207.
[2] *Ibid.*, 1904, xxiv, p. 9.
[3] *Ibid.*, 1910, xxx, p. 167.
[4] *Ann. d'Oculistique*, Brux., 1860, xliv, p. 293.
[5] *Gaz. hebdom.*, Par., 1863, x, p. 284.

and look as if they had been bespattered with tiny particles of white foam. If the foam-like material had been wiped away (a simple affair), it is reproduced within twenty-four or thirty-six hours after complete removal. The glistening, dry-looking plaques, if once seen, can scarcely be mistaken for anything else. To the condition the name " epithelial xerosis " (Saemisch) is commonly applied.

In my experience, and in that of every other observer who has investigated the point, the patches on the conjunctiva are crowded with that common saphrophytic organism, the xerosis bacillus.

A history of the second symptom, night-blindness, is often difficult to elicit from young children. Neither have we at our disposal any ready or accurate objective means of ascertaining its presence or absence in that class. In residential institutions, however, nurses and attendants not infrequently present a child for medical consultation because they have noticed him to stumble about in twilight or to knock into objects after the lights have been turned low in the dormitories.

The relationship between conjunctival changes, on the one hand, and night-blindness on the other is not invariable. The one symptom may occur without the other, and vice versa. Indeed, the proportion of those with conjunctival xerosis who suffer from night-blindness varies according to time and place, and for that matter, in the same place at different times. Thus, among eighteen cases of xerosis seen by me in 1895 at one of the largest London Poor-law schools, no fewer than 61 per cent. were more or less blind at night, whereas during 1896 and 1897, when nine cases of xerosis were respectively found, definite night-blindness appeared to be altogether absent.

At the same time, xerosis and night-blindness occur so often side by side that, as several authors have remarked, the association between them is likely to be something more than merely coincidental; indeed, I shall now endeavour to show that the connexion is much closer than is generally supposed.

In the course of investigations undertaken some years ago,[1] I found that in children with xerosis, but without ascertainable night-blindness, there existed changes in the visual fields. These were of two kinds—viz., constant and inconstant. The former consisted in a reduction for the red and green fields. But that was not all, for the field for red was more shrunken than that for green, so that the former lay inside

[1] *Trans. Ophthal. Soc. U.K.*, 1898, xviii, p. 55.

FIG. 1.

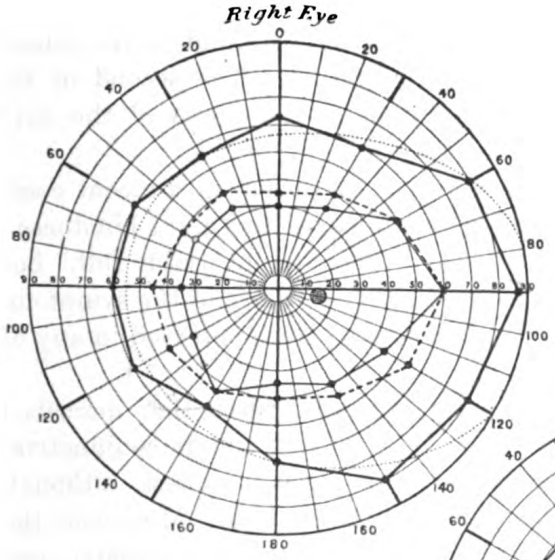


FIG. 2

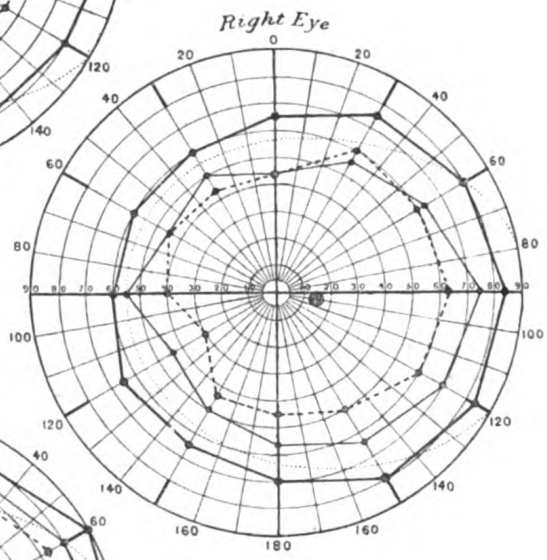
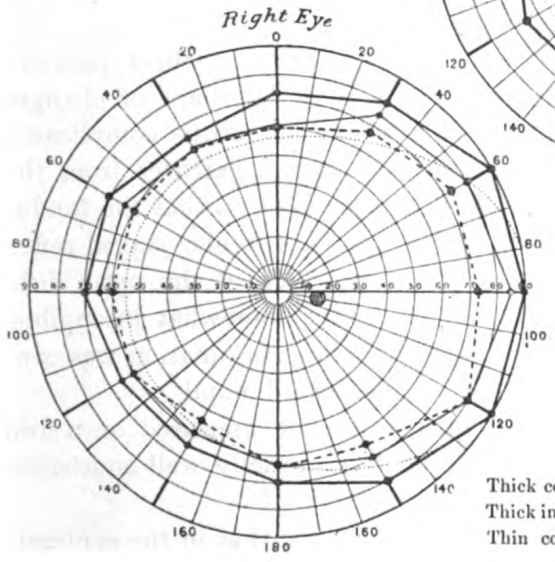


FIG. 3.



Thick continuous line = field for white
Thick interrupted line = field for green
Thin continuous line = field for red

Changes in field of vision in a case of epithelial xerosis not accompanied by night-blindness. —L. M., aged 10 years. Fig. 1, field on August 16, 1897: Light minimum, 2 sq. mm.; hæmoglobin, 70 per cent.; red cells, 80 per cent. Fig. 2, field on October 30, 1897: Hæmoglobin, 80 per cent.; red cells, 90 per cent. Fig. 3, field on recovery: Hæmoglobin, 80 per cent.; red cells, 116 per cent.

the latter, whereas under normal conditions the reverse should, of course, be the case. In three-fourths of the patients examined with reference to this point the transposition was complete, but in the others the two fields overlapped at one or more places. The second or inconstant change lay in a slight contraction of the limits of the field for white.

In contradistinction to this observation, Hepburn, in a recent communication dealing with xerosis of the conjunctiva and night-blindness,[1] states that in these cases "the fields of vision are invariably full," but Theodor Saemisch[2] mentions the occasional occurrence of the symptom.

Neither ring scotomata nor zonular defects were observed in any of my patients.

Now the changes in the visual fields, indicated rather than described above, may be found, as I have already said, in a simple conjunctival xerosis as well as in xerosis associated with night-blindness, although they are more pronounced under the latter conditions. Moreover, the so-called "light minimum," as estimated by Förster's photometer, was diminished in both the conditions named. The obvious conclusion is that in both there exists a state of torpor retinæ. In other words, every child with the conjunctival changes is in a condition of potential night-blindness, although obvious symptoms of that ailment cannot be discovered on a mere casual examination.

Furthermore, during my investigations I found a third point of connexion to exist between the two conditions in the shape of changes in the fundus oculi. Both in simple xerosis and in xerosis complicated with night-blindness, the fundus presented slight departures from the normal. Thus, the retinal reflexes were exaggerated, so that the fundus looked paler than usual, while, in addition, a semicircular jagged reflex was often to be observed close to the inner side of the optic disk. These points, although under any circumstances somewhat intangible, were easier to appreciate when ophthalmoscopic examination was conducted under weak illumination with an undilated pupil.

Most of the affected children I have examined appeared on a first view to enjoy good general health. They were usually well nourished, while not a few had bright and ruddy cheeks.

Bitot (loc. cit.) noted as a singular circumstance that in the epidemic described by him among the inmates of the Hospice des Enfants Assistés

[1] *Trans. Ophthal. Soc. U.K.*, 1910, xxx, p. 1697.

[2] "Handbuch der Gesamten Augenheilkunde," 1904, i, p. 443.

FIG 4.

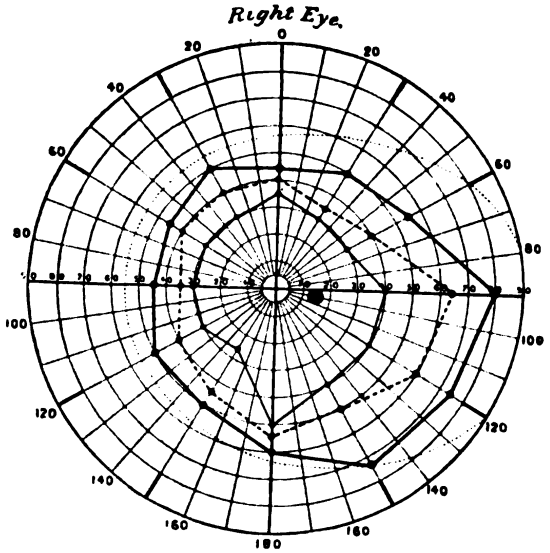
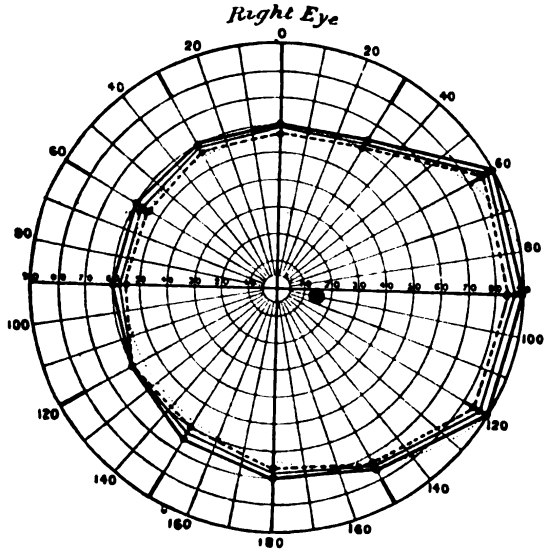


FIG. 5.



Thick continuous line – field for white ———
Thick interrupted line = field for green - - - - - -
Thin continuous line = field for red ———

Changes in field of vision in a case of epithelial xerosis accompanied by night-blindness.—H. H., aged 10 years. Fig. 4, taken on August 9, 1897, shows that the fields for white, red, and green are contracted, and that the red and green fields are transposed. Light minimum, 10 sq. mm.; hæmoglobin, 70 per cent.; red blood cells, 98 per cent. Fig. 5 shows the visual field after recovery. Hæmoglobin, 85 per cent.

de Bordeaux, the ailment affected those in rude health, and spared for the most part the many scrofulous and rachitic inmates sheltered by the asylum. A somehat similar conclusion was reached by Cohn,[1] and was repeated only the other day by Malcolm L. Hepburn (loc. cit.)

A more attentive examination of my own cases convinced me that to not a few the old name "strumous" might fitly be applied.

Such appearances as otorrhœa, large tonsils, opacities of the cornea, eruptions about face and ears, swollen upper lids, nasal catarrh, enlarged cervical glands, and synovitis of the larger joints were common among them.

Besides this, I found that the children with xerosis conjunctivæ, with or without night-blindness, showed a deficiency in the hæmoglobin content of the blood. For example, in fifteen cases the hæmoglobin averaged only 65 per cent. of the normal. When the conjunctival changes had disappeared, the proportion was found to have risen, but never to par. This observation led me to inquire as to the possibility of Gowers's hæmoglobinometer (the instrument I employed) being over-standardized. On this point I was unable to obtain any satisfactory information, and so, with the assistance of Mr. G. C. Burton, then resident medical officer at the Queen's Hospital, the point was investigated in 164 children, whose ages ranged from a few months to fourteen years. In every instance the percentage of hæmoglobin fell below 100. It averaged 76·62 per cent., was slightly greater in males than females, and bore no definite relation to age. The exact figures have been embodied in an appendix to the present communication. One fact stood out clearly, namely, that the percentage of hæmoglobin was lower in children with than without xerosis conjunctivæ. Among the former the average was 65 per cent., while among the latter it stood at 76·62 per cent. The red blood count ranged from 70 per cent. to 134 per cent., and averaged 80 per cent.

From all this it follows that in xerosis, with or without night-blindness, the relationship between red cells and hæmoglobin—the so-called "colour index" is generally reduced. To put the matter in another way, a condition akin to chlorosis, as determined by the blood, exists in the cases we are discussing.

Can we bring the several facts enumerated into line as accounting for the cause of this curious affection? I believe that we can. In the first place, it is significant that xerosis and night-blindness should occur

[1] "Ueber Xerosis Conjunctivæ," Breslau, 1868.

only in poor-class children, and should make their appearance exclusively in summer and autumn. Again, as regards any given institution it is a matter of familiar observation to those acquainted with the circumstances that the brighter the weather the greater is the number of cases. The dazzling of sunlight, indeed, appears to be the immediate cause, and this is doubtless intensified by the ingrained habit of institutional children, who persist in running about the airing courts of the Poor-law school, which are generally paved with York flags, without any protection whatever to the head in the shape of caps or hats. Anyone who has ever visited one of those places during summer weather will bear me out when saying that the flagged yards reflect an uncomfortable body of light into the eyes, apart altogether from the direct rays of the sun falling from above. The reflection from the white-washed walls so common in those institutions, also, is doubtless not without influence. That boys suffer more than girls, a point first brought out by Bitot (loc. cit.) is probably due to the relatively greater freedom enjoyed by the first-named class. For my own part, I do not believe in a true sex-incidence of the disease.

The white patches are to be found only on that part of the conjunctiva which is exposed to light when the eyes are open. This suggests that under the influence of light or of some of its elements the metabolism of the exposed part undergoes an alteration, and thus allows the xerosis bacillus—an almost constant inhabitant of the conjunctival sac—to lodge upon the parts and to multiply to an enormous extent. Particles of keratin and of kerato-hyalin are found pathologically in the altered epithelium, and this leads, as Stephen Mayou has pointed out (loc. cit.), to an alteration in the surface tension of the affected areas, in consequence of which the oily secretion of the Meibomian glands collects upon them in the form of a white foam. But it is important to remember that the bacilli themselves play no part in the causation of the symptom-complex, although the contrary view has, of course, been held by more than one writer.

Last, but not least, the remote cause is to be sought in some slight defect of nutrition, as indicated by an alteration in the colour index of the blood.

Finally, it should be said that recurrence is common in one and the same subject. Yet, although this may happen during several successive summers, the ailment does not appear to entail any serious or permanent mischief, either as regards the eye itself or the body generally.

APPENDIX.

Estimation of Hæmoglobin in 164 Healthy Children.

(Tested by means of Gowers's hæmoglobinometer.)

Age	Males	Females
1 year	83·5	75·0
2 years	73·3	—
3 ,,	70·0	73·0
4 ,,	73·3	75·0
5 ,,	73·8	76 3
6 ,,	74·0	76·4
7 ,,	74·0	73·7
8 ,,	79·0	75·0
9 ,,	77·7	75·2
10 ,,	78·3	82·4
11 ,,	76·3	72·0
12 ,,	91·0	80·4
13 ,,	81·8	78·4
14 ,,	77·0	73·0

Number of children examined, 164 (ages 2 months to 14 years) } Males, 88; females, 76.

Average hæmoglobin = 76·62 per cent.—i.e., males 77·35 per cent. and females 75·83 per cent.

Section for the Study of Disease in Children.

November 24, 1911.

Dr. G. A. SUTHERLAND, President of the Section, in the Chair.

Infantilism.

By VINCENT DICKINSON, M.D.

G. M., MALE, aged 7 years, is the youngest but one of eight children, born of apparently healthy Italian parents; four of the children are dead, the others healthy; no miscarriages. This child weighs 2 st. 4 lb., and his height is 2 ft. 9 in. Forehead rather prominent, eyes large, with (?) slight epicanthus; mouth not large, tongue small and pointed; hands large, with square-topped fingers, thumb and little finger long, no curvation. There is considerable deformity of bone structures—notably the clavicles, wrists, elbows, knees, and ankles; marked lordosis and genu valgum; chest rachitic; abdomen large, spleen and liver not felt; no excessive sweating. There are slight movements of the facial muscles and of the fingers, suggesting a mild degree of athetosis. The child is not deaf, and seems to understand what is said, but he does not speak; he will repeat a few words when told, such as "papa," "grazie," "buona sera," but in a whisper only. Von Pirquet's test is negative.

Dr. DICKINSON said that the child presented rickets in an extreme degree, and was very backward intellectually. He did not think rickets would explain the whole case, including the mental deficiency. He thought there was a Mongolian aspect about the boy.

Chronic Interstitial Nephritis with Infantilism.

By Reginald Miller, M.D.

Boy, aged 9½ years; height, 3 ft. 1½ in.; weight, 34 lb. Full-term child. From birth it was noticed that he took a lot of fluid, and passed much urine, and that his growth became retarded. Worse after an



a *b*

Infantilism with chronic interstitial nephritis.

a = Patient, aged 9 years.
b = Brother, aged 3½ years.

attack of diarrhœa at the age of 5 months. Certainly very small for his age at 3 years old. Measles at the age of 4 years. Six other children in the family, all healthy; no miscarriages. Has been under observation as an out-patient for two years. During this time he has grown 1½ in.

Perpetually suffers from thirst; will drink 1½ pints of water in the night. Marked polyuria. Marked phimosis. Child is thin and very pale; skin is dry and lined. He sweats only in the hottest weather. There are slight evidences of old rickets; knock-kneed. Abdomen prominent; liver, spleen, and kidneys not enlarged. The heart shows some slight hypertrophy of the left ventricle; the radial arteries are not thickened; the pulse tension does not appear abnormally high. Urine very copious; specific gravity 1002-4; thick trace of albumin, lessened, but not disappearing, after twelve hours' rest in bed. A granular cast found once.

Mentally is intelligent, but very backward; can write his own name, and begins to do sums now. Is in the infants' class at school. Wassermann test (Fleming's modification) negative.

No improvement was made under a six weeks' course of thyroid extract.

Photographs: (1) patient at age of 5½ years with same wrinkled skin as he has now; (2) patient at age of 9 years. He is slightly taller than his brother, aged 3½ years (*see* figure).

DISCUSSION.

Dr. MILLER added that the brachial arteries were easily palpable, and were perhaps slightly thickened, although the radials appeared normal. It seemed to be the third example of a condition which had been only recently recognized. Dr. Morley Fletcher showed a case of infantilism with chronic renal disease before the Section on March 24, and in that the symptoms dated from birth.[1] Dr. Leonard Parsons reported a case of infantilism with chronic interstitial nephritis at the meeting of the British Medical Association at Birmingham.[2] In these the symptoms were polyuria and thirst, the Wassermann reaction was negative, and there was very distinct infantilism. The present case seemed to fall into line with the other two cases. Contracted granular kidney in children had been described before, but in those accounts there had been no mention of infantilism in association. Dr. Sawyer had merely mentioned that about half the children were undersized, and Dr. Guthrie mentioned that his cases were undersized, stunted and wizened, but that mentally they were precocious. It would be interesting if Dr. Guthrie could say anything about the later history of his cases, especially in regard to the changes at puberty. The question arose as to whether the condition seen in this child was a newly described one, or whether it was merely a new title for a condition which had been long observed.

[1] *Proceedings*, 1911, iv, p. 95.
[2] *Brit. Med. Journ.*, 1911, ii, p. 481.

Dr. LANGMEAD said he had recently had the opportunity of seeing post-mortem examinations on two patients whose cases he thought might be of interest from the point of view of the present discussion. Both patients had been under the care of his colleague, Dr. Sainsbury, at the Royal Free Hospital, who had kindly permitted him to mention them. The first was the case of a man aged 23 years, who was undersized and poorly developed mentally. From childhood it had been noticed that he was very pale, and he was admitted for extreme anæmia. Albumin was present in the urine. His red corpuscles amounted to only 960,000, the hæmoglobin content being only 10 per cent. He ultimately died from that anæmia, and post mortem it was found that there was slight fibrosis of the kidneys, as well as fibrosis of the spleen and of the liver. The liver was very distinctly cirrhotic. The Wassermann reaction had been repeatedly done, and each time was negative. The other case was that of a girl aged 19 years, who was also undersized and mentally weak, and in like manner suffered from very severe anæmia, which dated from early life. In that case there was definite polyuria and polydipsia. There was albuminuria. The anæmia ended fatally, and post mortem very marked chronic interstitial nephritis was found. One kidney only weighed ½ oz., and the other 1 oz. There was fibrosis of the spleen. The Wassermann reaction was negative. The point of interest was whether one should take a standpoint which gave a larger conception of these cases, regarding them as unexplained visceral fibrosis beginning in early life. There had been many cases of chronic interstitial nephritis in childhood described, but several of them could not be ascribed to syphilis. Nor could many cases of hepatic cirrhosis in children be attributed to alcohol. In the two cases he had mentioned the organs were carefully examined, and apart from the fibrosis nothing suggestive of syphilis could be found. He supported the suggestion that one should take a broader view of such cases, and not call them simply fibrosis of the kidneys or interstitial nephritis and infantilism, but fibrosis of organs generally, of unexplained origin.

Dr. MORLEY FLETCHER referred to the similar case that he showed in March, and said that the resemblance was so striking that the two cases might almost be taken for twin brothers, except that this boy was more pallid. The points in the cases were, first, the polyuria of very high degree, and secondly, greatly retarded bodily growth amounting to infantilism, in children, who were probably suffering from renal disease of intra-uterine origin. He would add a third point, namely, that in all these cases he had seen—and he had several under observation now—there was some deformity of the legs; they had all genu valgum, as was the case here, and one had curved tibiæ. He thought there was no necessity to assume more than that there was some fibrotic condition of the kidney, and that these cases should be regarded as distinct from those described as diabetes insipidus, with retarded growth, seeing that the urine in Dr. Miller's case and in his own cases contained a very considerable quantity of albumin. The polyuria was so extreme in Dr. Fletcher's case, that he had mentioned the fact that the boy sometimes passed in twenty-four

hours a weight of water equal to one-fourth of his body-weight. The opinion he expressed was that there was probably some congenital condition of his kidney of unknown description produced by intra-uterine disease. It was not the ordinary congenital cystic disease of the kidney; one could not feel, even under an anæsthetic, kidneys of abnormal size or shape. Nor was he aware of any recorded cases of congenital cystic disease of the kidney similarly associated with infantilism. Therefore he thought these children had suffered in intra-uterine life from some toxic or infectious condition, involving the kidney, which had brought about an atrophic diffusely fibrotic state. In none of the cases he had seen was there evidence of tubal nephritis. There were no epithelial casts, and Dr. Miller's case only showed occasional granular casts. In his own case one or two hyaline and granular casts had been found. It was very important that the attention of the Section should be drawn to those cases, so that further light might be shed upon them. Also observations should be made as to the frequency of occurrence of curvature of the bones or other deformities.

Dr. NAISH said that during the present year he had seen autopsies on two cases which closely corresponded to the present one. Both were boys: one aged 16½ years who appeared about 10 years old, and the other 9½ years, appearing about 4 years old. They both had genu valgum, and in both that deformity came on late in life, at 14 and 8 years of age respectively. This late onset was verified in the case of the elder boy by a photograph the parents had, showing him at the age of 13 years with straight legs. In both cases the kidneys were very small and fibrotic, weighing about 1½ oz. each; the tubules and glomeruli were comparatively little affected. Both the children were very tiny at birth, and very much smaller than the other members of the families. There was no history of syphilis and all the other children were apparently quite normal. Both had, according to the parents' emphatic statement, polyuria and polydipsia for a considerable portion of their lives, and both had been taken to a doctor for "drinking diabetes." In the younger boy this had, as the parents said, much diminished during the last few months of life. The elder boy suffered from air hunger at the last, but had no acetonuria.

Dr. LEONARD GUTHRIE said he believed Dr. Miller's case and those described by Dr. Langmead belonged to a different category from those which were described as chronic interstitial nephritis of children. He could discover no cardio-vascular changes in Dr. Miller's case. But in true chronic interstitial nephritis in children the changes had been as marked as in adults, and the course of the cases had been exactly similar. Infantilism, as a rule, was not associated with chronic interstitial nephritis of children. Most patients were small, thin children, but they could not be regarded as cases of infantilism. He did not see that there was any evidence of nephritis here, except the polyuria and the slight amount of albumin. In chronic interstitial nephritis in children, sooner or later there were exacerbations in which there was a large quantity of albumin, and very often casts were passed, uræmia and sometimes cerebral

hæmorrhage occurred. In most cases which had been described as chronic interstitial nephritis the cause was congenital syphilis. He used to consider this the cause of all of them, but now he thought that any kind of septic absorption might produce a form of interstitial nephritis in childhood which was not necessarily syphilitic. His view, therefore, was that Dr. Miller's case should not be described as one of chronic interstitial nephritis.

Dr. C. W. CHAPMAN said he noticed the brachial arteries in this case were decidedly thickened, though this condition was not so apparent in the radials.

Dr. MILLER, in reply, apologized for the paucity of details given of the case. This was due to the fact that although the patient had been under his observation at long intervals over a considerable time, the mother would not allow the boy to come into the hospital; therefore he could not measure the quantity which he drank nor the amount of water passed. The child was stated to have been large at birth. The genu valgum, according to the mother, developed as soon as he began to walk. As to the question whether or not such cases should be called chronic interstitial nephritis, after the expressions from eminent authorities he did not feel inclined to debate the point. Of the three cases which he had mentioned, the important one, in his view, was that of Dr. Parsons, because there was found, post mortem, to be distinct chronic interstitial nephritis; there was also a large left ventricle, with the cardio-vascular changes ordinarily associated with chronic interstitial nephritis. Nevertheless, in Dr. Parsons's case there was very definite infantilism; although 6 years of age, the child was only the height of a child of 3 years, for which age it was usually taken. The symptoms, too, were congenital, and there was no evidence of syphilis. Thus it seemed that Dr. Parsons's case formed a connecting link between the group of cases exemplified by Dr. Morley Fletcher's and his own case on the one hand, and the group of cases of chronic interstitial nephritis without infantilism described by Dr. Guthrie and Dr. J. E. H. Sawyer, on the other.

Intracranial Tumour.

By REGINALD MILLER, M.D.

GIRL, aged 7 years 3 months.

History: Had pneumonia a year ago, and since then has gradually become unsteady in her gait, and suffered from occasional sudden attacks of vomiting, associated with transient headache and giddiness.

Present state: Child has sudden attacks of headache, usually frontal; with these are attacks of typical cerebral vomiting, sudden, explosive, and sometimes brought on by change of position. Mentally the patient is happy, quick, and intelligent. Her gait is ataxic; she

falls rather more to the right and backwards than in other directions. There is no ataxia of the hands; no Rombergism. Cranial nerves: Nystagmus in all directions, most marked and coarsest to right. Slight paresis of right external rectus. Vision poor; hypermetropia; no optic neuritis. Child shows "cerebellar tilt" of head, the right ear being lowered towards right shoulder and chin turned upwards and to left. Hearing good. Sensation: Sensation of movements of self are in the opposite direction from sensations of movements of external objects: Loss of sense of position in lower limbs; no areas of anæsthesia. Reflexes: Pupil, light reaction sluggish; abdominal reflexes absent (right disappearing first), arm-jerks increased, finger-flexion bilateral, ankle-clonus bilateral (right appeared first); Babinski's sign at first occasional on right, now occasional on both sides; knee-jerks increased (right increased first). Wassermann (Fleming) negative; von Pirquet negative.

Diagnosis: The character of the symptoms, particularly of the vomiting, points to intracranial tumour. Friedreich's ataxia and disseminated sclerosis may be excluded. The localization of the tumour appears to rest between a diffuse pontine glioma, with early cerebellar symptoms and a right lateral lobe cerebellar tumour.

DISCUSSION.

Dr. MILLER said that there were various points which indicated that it was a cerebellar tumour of the right lateral lobe, a view which several members had taken of the case. On the other hand, he thought it was possibly a diffuse pontine glioma ("hypertrophy of the pons") owing to the very insidious onset, the early involvement of the pyramidal tracts and the absence of optic neuritis after nearly a year's illness. In his experience the early symptoms of such cases were of a cerebellar type, and he had seen three cases running such a course as in the present child, in which a diagnosis of cerebellar tumour had been made, and in all of them the condition was found, post mortem, to be one of diffuse pontine glioma. He suggested the present case might perhaps be one of the same type.

Dr. LEONARD GUTHRIE said he believed it to be an almost typical case of tumour in the right lobe of the cerebellum. The only point against that diagnosis was the absence of optic neuritis. One usually expected this to occur early in such cases, but perhaps it would develop shortly. Dr. Miller suggested it might be extra-cerebellar. There would be the same symptoms in extra-cerebellar tumour; but against that diagnosis was the fact that extra-cerebellar tumours in children were very rare.

Psoriasis and Flexion of the Terminal Phalanx of the Thumb.

By F. J. Poynton, M.D.

G. M., female, aged 2 years, had suffered from rickets. Three months ago a discrete eruption began to appear, which has proved to be psoriasis. About the same time the right thumb began to contract. There has been no pain and no other joint affected. The terminal phalanx of the right thumb is obviously flexed on the proximal and cannot be fully extended. A skiagram, by Dr. Ironside Bruce, shows that there is no alteration in the structure or outlines of the bones to suggest a reason for the alteration in the function of the joint. The flexor tendons of the thumb are apparently shortened, and have undergone some fibrosis.

The interest in the case lies in the gradual painless development of a partial flexion of the right thumb, associated with a psoriasis, in a child of 2 years.

Mr. Fitzwilliams said it was difficult to know what the connexion could be between psoriasis and the flexion of the thumb, though the one seemed to have followed the other. The condition was due to an adhesion of the long flexor of the thumb to its sheath, opposite the base of the first phalanx of the thumb. On attempting to extend the thumb the tendon became tense as far as that point, and remained perfectly lax beyond it. He thought the condition must have been set up by some slight injury or some inflammatory condition which had caused an adhesion to the thecal sheath. He thought that if extension was made and the adhesion freed and kept free by movements, then the use of the thumb would be recovered.

Congenital Absence of Patellæ and Deformity of the Nails in a Mother and Three Children.

By A. C. D. Firth, M.B.

The members of the family showing the deformities are the mother, aged 32 years, and three children, all girls, aged respectively, 10, 4½ and 3 years. There is no history of any similar deformity on the paternal side, and although the mother said she thought some of her uncles had

"double-jointed" knees, I have communicated with one of them and he tells me that they are perfectly normal. Four children have died, one of whom had no patellæ.

The members of the family have been as follows:—

Mother, aged 32 years: Patellæ absent, nails deformed.

Mary, aged 10 years: Patellæ absent, nails deformed. Is unable to fully extend the arms at the elbow and has undergone an operation for talipes.

Twins (premature): Died soon after birth. Sex not stated. Said to have been normal.

Boy: Died, aged 2 years; normal.

Girl: Died, aged 2 months. Patellæ absent; cleft palate.

Violet, aged 4½ years: Patella present on right side, rudimentary on left side.

Kathleen, aged 3 years: Patellæ absent, nails deformed; unable to fully extend left arm at the elbow.

The deformity in no way interferes with the activity of the patients and was first discovered in the eldest child when brought to the hospital to be passed for admission to a convalescent home.

Dr. JOHN THOMSON (Edinburgh) said that within the last few weeks he had made the acquaintance of a similar family, consisting of father, mother, and nine children. The mother and five of the children showed complete absence of both patellæ and deformity of all the finger-nails and toe-nails. The father and the four other children were quite normal as to patellæ and nails. The mother showed some slight inability to straighten her right elbow completely, and she could not fully supinate her right forearm. That was a deformity which was present in two of Dr. Firth's cases. The sixth child had also congenital ptosis and paralysis of the internal rectus, which apparently was due to defective development of the third nucleus. This class of case was very interesting, especially in connexion with the obscure but apparently certain relationship between the development of the nails and that of the patellæ.

Hereditary Syphilitic Infant treated by Intravenous Injection of "606."

By J. L. Bunch, M.D.

The patient was a male, aged 8 weeks, who was admitted to the Queen's Hospital for Children with an eruption which had been present for the last four or five weeks. The child presented a thin, old appearance, the skin was of a brownish-yellow tint and was covered with a maculo-papular eruption, especially well marked in the genito-crural region and on the buttocks. The papules were flattish and were present also on the hands and soles, and the eruption had a brownish tint. There were fissures at the angles of the mouth, some moist papules on the trunk and near the anus, and the child had snuffles. Weight was 10 lb.

The Wassermann reaction was positive and the *Spirochæta pallida* was found in the skin lesions.

On June 21, 1911, 0·03 grm. arsenobenzol was injected by me into the median basilic vein. On June 24 the coppery eruption had greatly diminished in intensity and the ulcerated patches were much cleaner. These symptoms diminished gradually until June 30, when 0·04 grm. of the same drug was injected intramuscularly into the glutei muscles.

By July 7 all syphilitic lesions had disappeared and the child has remained free from syphilitic symptoms until the present.

DISCUSSION.

Dr. Bunch added that the notes hardly conveyed a true idea of the severity of the child's illness. There were numerous superficial ulcers, and the discharge was so offensive as to make the ward objectionable to other patients. He would not say that the case appeared hopeless, but the child was at least extremely ill. He gave an injection of salvarsan, 1 centigramme per kilo of body-weight, intravenously; his previous experience of intramuscular injections of salvarsan had not been very favourable, and some months ago he showed a case of much the same age in which after intramuscular injection, although the symptoms cleared up for a time, they recurred very soon. The Wassermann reaction was negative. Another alternative was, of course, to inject the mother with it, but he was not inclined to attach much value to such a method of treatment. In answer to the President, he said that no mercurial injection

was given; the intravenous injection of salvarsan was given within a few days of the child's admittance into the hospital.

Dr. BELLINGHAM SMITH said that the reports as to the use of salvarsan had not been very favourable in cases of congenital syphilis. Therefore he would like to know if all Dr. Bunch's cases had been as successful as the one now shown.

Dr. POYNTON said he had had one very severe case in which salvarsan was injected into the buttock. That was before the great importance of intravenous injection had been fully realized. As in Dr. Bunch's case, the symptoms cleared up well, and he continued with mercury afterwards. The buttock, however, got into a very bad state and most of it seemed to slough.

Dr. BUNCH, in reply, said he knew that many of the results, especially in Germany, of intravenous injection of salvarsan in infants had not been very successful, and he would not recommend it in every case of congenital syphilis. But he laid stress on the fact of the baby's serious illness. In answer to Dr. Morley Fletcher, the Wassermann reaction was still negative. With reference to Dr. Poynton's remarks, one of the reasons why he inclined to the use of intravenous injection of salvarsan in infants was that cases of sloughing had occurred after intramuscular injection.

Congenital Word Deafness and other Defects.

By E. BELLINGHAM SMITH, M.D.

J. T., A BOY, aged 10 years, of Jewish parentage, was brought to hospital for an inability to speak and general backwardness. His appearance at first sight suggests a marked degree of mental deficiency, which is, however, more apparent than real. A right congenital torticollis causes a considerable displacement of the head to the right and a diminution in size of the face on the affected side. His walk is sidelong, with left shoulder tilted up, and his gait is staggering and ataxic; all movements which he performs are associated with a coarse tremor. There is no spasticity and the reflexes are sluggish. Sensation is normal. As regards observation he exhibits a marked degree of interest in everything that goes on around him, describing them by means of gestures in a rapid and tremulous fashion. If not understood he becomes very excited and utters a number of low guttural sounds. Speech is limited to a very indistinct enunciation of "mumma," "papa," and "Bert," his brother's name; these he produces by carefully watching his mother's expression as she frames the words. On a casual examination he appears deaf, but is not really so, as he can hear loud sounds,

such as a bell or jingling keys, and if given a watch places it immediately to his ear and seems to appreciate its ticking. Spoken words have, however, no meaning to him whatever and he only interprets what is meant by the accompanying gestures. Sight is said to be good for distant objects but bad for near ones. If shown figures or letters on a piece of paper he holds it close to his left eye to see them. He can copy letters, although he is considerably hampered by his associated tremor, which accompanies every movement. When writing he uses the left hand and occasionally commences with the last letter of the word and writes backwards from right to left. He can count up to twenty on his fingers and recognizes the numerals when he sees them on paper.

He recognizes objects in pictures and describes them by gestures; thus a horse is an animal that is driven and a bird is something that lays eggs and flaps its wings, a policeman is a man who wears a helmet, and so on, all of which he describes by appropriate pantomime. His gestures are rapid and imperfect, like all his movements, and are frequently only intelligible to his parents. This method of communication has been entirely evolved by the patient himself and suggests some considerable degree of intelligence. He is cleanly in his habits, not spiteful or destructive, and assists in simple domestic work at home. He is nervous and sensitive, and amongst other things is possessed of an excellent memory.

Granuloma Annulare.

By Haldin Davis, F.R.C.S.

The patient was a little girl, aged 6 years, of delicate appearance. She suffered from a slight lateral curvature of the spine and nocturnal enuresis, but was otherwise healthy. The mother said that she was the third of six children, the remainder of whom were quite sound. The only illnesses which the patient had suffered from were chicken-pox and measles; there was no history of rheumatism, tubercle, or syphilis.

The eruption, which made the patient an object of interest, was situated on the dorsum of the right hand. There were two distinct lesions. One, the larger, extended from the metacarpo-phalangeal joints of the second, third and fourth fingers about 1 in. in a proximal

direction. It was roughly circular in shape, sharply margined, and the periphery was made up of a sort of necklace of nodules closely set together round a central area, in which were only a few isolated nodules. The skin was unbroken and unaltered in colour, but the nodules could be made out, and when touched were found to be of much tougher consistency than the normal skin. Separated by a short interval and nearer the wrist was a second lesion, smaller and made up of the same constituents, but the outline presented was like that formed by two intersecting circles. The centre of this patch was free. The patient was stated to have had a similar lesion last year, which disappeared.

The diagnosis made was that of granuloma annulare, or "ringed eruption of Colcott Fox," who originally described the appearances in the year 1892. Since then the eruption has been described many times under different names, for example, lichen annularis, eruption circinée chronique de la main, sarcoid tumours, &c. Dr. Graham Little collected forty-nine cases in 1908, which he considered all belonged to the same group, and read a paper on them.[1] Most of the cases have been in children, and this is the second which has come under the notice of the present writer, the former occurred in a baby of 6 months. The patches were more numerous, situated on the buttocks, and after a time they all disappeared without leaving any trace behind.

Mr. DAVIS desired to emphasize the fact that although this was an eruption (probably of toxic origin) which had been only recently described, it was not very uncommon, because, in addition to the cases described by Dr. Graham Little, many more had now been shown. The eruption, which occurred more in young people than in those of older years, was typically present on the hands, and, in very young children, on the buttock.

Cerebral Aplasia with Hydrocephalus (Pathological Specimen).

By R. SALUSBURY TREVOR, M.B., and H. D. ROLLESTON, M.D.

A FULL-TIME male child, born with instrumental assistance, appeared normal for the first ten days of life. Dyspnœa with inspiratory spasm then came on, and the infant had opisthotonos. On the twelfth day of life the infant had a fit with right-sided spasm, the head being turned

[1] *Proceedings*, 1908, i (Derm. Sect.), pp. 95-163.

to the right, with nystagmus and involuntary movements of the left side. The temperature rose to 103° F., and the respirations to 72. The condition suggested meningeal hæmorrhage or tetanus neonatorum, but the umbilical wound appeared normal. The child was noticed to yawn very frequently. On the twentieth day the child had another fit, the temperature going up to 105° F., the pulse to 180, and the respirations to 90. The infant steadily lost weight and went downhill. On the twenty-sixth day there was permanent convergent strabismus. Death occurred on the twenty-eighth day of life. There was no history or evidence of inherited syphilis.

The body was examined twenty-eight hours after death. It was wasted and covered with a fine down. The legs were flexed at the knees and the thighs on the abdomen, whilst the body, as a whole, was markedly rigid. The head did not appear to be out of proportion to the rest of the body, and there was no abnormal appearance of the eyes. The anterior and posterior fontanelles were widely open, the anterior one being unusually so. The frontal bones were separated to the extent of ¼ in. at a point two-thirds of the way down from the bregma. The head measured 13½ in. round the base. As soon as the skull was opened a large quantity of clear fluid, which was evidently under pressure, escaped. This fluid escaped from the interior of the brain which was torn in removal of the calvarium. The reaction of Rivalta showed that the fluid was not due to inflammation. It was evident that the brain was abnormal. It was represented by a thin layer of brain-tissue enclosing a cavity; this change was also present in the cerebellum.

The specimen shown is the collapsed sac, which represented the brain. It weighs 22·45 grm. The cerebral hemispheres are represented by a thin sheet of brain-tissue, fused in the middle line by union of the pia-arachnoid. The falx cerebri is absent. Over the posterior part of the left half of the sac is a subarachnoid collection of fluid forming a cyst, which, on further examination, is found to be directly continuous with the membranous sac forming the cerebellum.

The cerebellum is represented by a cystic cavity with thin membranous walls, in which are traces of brain-matter. The cyst projects on either side of the brain-stem in the shape of the cerebellar hemispheres, but all trace of the peduncles and the valve of Vieussens is absent. The cyst is unilocular, and is continuous with the cyst on the outside of the left half of the brain, so that the latter cyst appears to have arisen by an escape over the free margin of the tentorium of a part of the cerebellar cyst—much as a rubber bag containing fluid may

be made to pass through small apertures. Subsequent adhesion must have occurred between the cerebellar cyst and the pia-arachnoid of the left hemisphere.

The medulla is fairly well formed but the normal thickening constituting the pons is wanting. The pituitary body was well formed, as are the optic chiasma and nerves and the olfactory tracts. The Sylvian fissures are grooves only, and are not separable. The circle of Willis is very small, and the carotids appear rather smaller than usual for a baby. The basilar artery is well formed. All the cranial nerves were identified at the base of the brain.

Examination of the cerebrum proves it to consist of one large cyst. There are practically no convolutions on the surface. On the floor of the cyst is a central trough running antero-posteriorly, which represents the third ventricle. Above and on either side of this are two string-like bands, under which the handle of a scalpel can be passed, which represent, perhaps, the remnants of the anterior pillars of the fornix—the opening beneath them being the foramen of Monro. Across the trough runs a thread-like band representing the middle commissure. The aqueduct of Sylvius is closed. On either side of the third ventricle there is a slight projection posteriorly, representing the optic thalamus, and on each of these are small cystic swellings at the site of the choroid plexuses, and beyond these the crumpled plexuses themselves. The great transverse fissure is wanting and there is no trace of the velum interpositum, nor of the corpus callosum. The iter is absent. The corpora quadrigemina and geniculate bodies are represented, but the pineal gland is unrecognizable. The fourth ventricle forms the floor in the mid-line of the cerebellar cyst, and except for the upper cyst wall it is roofless.

On separating the tentorium a fair quantity of blood-stained fluid was present on either side outside the cerebellar cyst. The colour of the fluid was brownish and the hæmorrhage appeared to have been of old date.

The cord was bathed in a similar brownish fluid. It appears rather flattened and in the upper part shows vacuolations in the region of the grey matter. The cut section in the upper part is not quite even in contour and suggests a possible mal-development of the pyramidal tracts.

The lungs showed collapse posteriorly. The heart was cloudy and the foramen ovale widely patent. Otherwise the thoracic organs were healthy.

The abdominal organs were cloudy, and the suprarenals—1·75 grm. (right) and 2·5 grm. (left)—seemed somewhat small but otherwise healthy. The bladder was healthy. Throughout the alimentary canal, from the stomach onwards, there was definite acute catarrhal inflammation. In the small and large intestines the lymphoid tissue was prominent.

Remarks.

The condition present in the specimen differs from that seen in the common form of hydrocephalus in the small size of the brain, as a whole, as well as of the head. Another striking difference is the involvement of the cerebellum, which in cases of ordinary hydrocephalus is usually normal. The absence of any remains of the falx cerebri and of the structures normally forming the roof of the fourth ventricle leaves little doubt that the primary lesion is an error of development. Both the fore-brain and the hind-brain are mal-developed—no division into hemispheres having taken place. In the case of the former the cerebrum is a unilocular cyst, but, unlike the condition met with in cases of cyclencephaly, in which the cerebrum forms a single cyst (Adami [1]), in our case the development of the eyes is normal. The great transverse fissure, the corpus callosum, and the greater part of the fornix, are wanting, and there is no trace of the velum interpositum as such, nor of the vena Galeni. Similarly the cerebellum is a thin-walled unilocular cyst, which has been forced by the pressure of fluid outside it to find further room for itself above the tentorium. As far as can be made out, the cystic cerebrum and cerebellum do not communicate at all, in spite of the fact that the fourth ventricle is included in the cerebellar cyst.

The choroid plexuses present in the main cerebral cyst on either side may be presumed to be capable of function, but with closure of the transverse fissure fluid would tend to accumulate and form the hydrocephalus present. This view of continued secretion with diminished outflow has been put forward by Ballantyne [2] as a possible explanation of the cause of hydrocephalus.

References.

[1] Adami. "Principles of Pathology," 2nd ed., 1910, ii, p. 553.
[2] Ballantyne. "Ante-natal Pathology: The Fœtus," 1902, p. 389.

DISCUSSION.

The PRESIDENT (Dr. G. A. Sutherland) recalled a case of cerebral aplasia with hydrocephalus which he had had three or four years ago, in which the hydrocephalus was external, not of the ordinary type, and the condition of the brain was that which was sometimes known as "walnut brain." In the present case the clinical diagnosis was meningeal hæmorrhage or tetanus neonatorum. That was exactly the diagnosis made in the case of the infant he referred to, and he finally came to regard it as a good example of chronic tetanus neonatorum. The child came under his care when it was about 6 weeks old, and there was a history of spasm and convulsions on the twelfth day. A very marked feature was marked opisthotonos, and at other times pleurosthotonos. In the present case dyspnœa and inspiratory spasm were noted. In his own case there was not so much dyspnœa as tachypnœa, and the respirations became as frequent as 120. In this case there were also rises of temperature, and in his own case the temperature at times reached 103° F., 104° F., and even 105° F. The yawning referred to was also a marked feature of his own case; the child would occasionally throw its arms back and give a thorough and leisurely yawn. Although his case was one of cerebral aplasia, it was of different type from that now described, the occipital lobes being fairly well developed, while the cerebral hemispheres were almost entirely absent. The cerebellum, the pontine region, and the medulla seemed normal. There was no internal hydrocephalus. The cranial cavity was filled up with material of a fluid nature, the dura mater being swollen and hæmorrhagic, and the pia-arachnoid being much thickened and of a jelly-like character. It was marvellous that his patient should have lived so long, namely, four and a half months. He supposed one might conclude that in the present case the intelligence was not of a high order. In his own case there was no sign of any intelligence whatever, the child was living a purely automatic existence.

Dr. E. CAUTLEY said he had had a case very similar to that now described, and showed the specimen before the Pathological Society in 1900 as "Congenital hydrocephalus with non-development of the prosencephalon."[1] It presented practically the same pathological appearances as those which the President had described in his case, the cerebrum being converted into a sac, and there being some remains of the occipital lobe. But he did not think it should be called true hydrocephalus, because there was no enlargement of the head, and the brain as a whole was of the normal size for a child of that age. In his own case there was extreme craniotabes; the parietal bones and the upper part of the occipital bone were practically entirely membranous. Details of the case: Male, aged 4 months; born November 2, 1899; died April 9, 1900; second child. Never healthy, breast-fed for one month.

[1] *Brit. Med. Journ.*, 1900, i, p. 1341.

Taken ill on March 10 with vomiting, drowsiness, variable rigidity and occasional screaming. On March 19 collapsed; retracted head, circumference 16⅛ in. Large anterior fontanelle, open sutures, aud extreme craniotabes of frontal, parietal, and occipital bones. Much spastic rigidity of lower limbs and exaggerated knee-jerks. Some rigidity and flexion of upper limbs. Pupils equal and reacted to light. Fair general nutrition. Died from passive congestion of the lungs and asthenia. The medulla, pons, crura cerebri, optic thalami, and cerebellum were small and ill-developed. The rest of the brain consisted of two dilated lateral ventricles, the walls of which were about as thick as a normal adult dura mater, except for a thicker occipital portion containing some brain-tissue. Section of the aqueduct of Sylvius (Branson), where the dilatation ended, showed extreme stenosis of the central canal.

Purpura in Infective Diarrhœa.

By H. D. Rolleston, M.D., and J. B. Molony, M.B.

Symptomatic purpura in infective diarrhœa of infants has not attracted much attention. Diarrhœa is mentioned incidentally by John Thomson [4] among the cachectic conditions which may cause purpura. As two well-marked examples of extensive purpura in infants semi-comatose from the effects of infective diarrhœa happened to come under observation on the same day, we analysed the notes of 100 cases of acute infective diarrhœa with reference to this point. The cases were sufficiently severe to be admitted to the wards of the Victoria Hospital for Children, but otherwise were unselected; about two-thirds of them were seen during the past summer. This analysis was made before we had seen Voelcker's paper "On Purpura in Children" [5], in which he comes to much the same conclusions.

Of the 100 cases (fifty-six males with an average age of 8 months, and forty-four females with an average age of 7·36 months), sixty-seven (thirty-six males, average age 7 months, thirty-one females, average age 6·2 months) proved fatal, and thirty-three (twenty males, average age 9·4 months, thirteen females, average 10·2 months) recovered. Purpura occurred in eleven (six male, five female) cases, all of which were fatal. Of the sixty-seven cases 16·4 per cent. showed purpura. The average age of the eleven cases was 8⅓ months, the extremes being 1 month and 28 months. All but two cases (28 months and 12 months) were under 11 months of age; and, exclusive of the girl aged 28 months, the average age works out at 6·2 months. None of the

purpuric cases showed œdema. Among the 100 cases there was one case with œdema of the hands and feet which recovered; and two fatal cases (without purpura) showed "septic" rashes.

Site of the Purpura.—In one of the eleven cases the situation is not recorded. In eight the eruption occupied the skin of the abdomen, more especially of the lower part, and in four of these the thorax was also affected; in one of the latter there were hæmorrhages on the arms, legs, and head. In one instance the thorax alone was affected, and in another the head only was involved. It is an interesting question why the purpura occurs on the trunk and avoids the extremities, where ordinary purpura is more commonly seen. Possibly the absence of the extravasation on the extremities is connected with the exhausted condition of the circulation in these patients, and an extremely low blood-pressure in the peripheral vessels. It is conceivable that with a higher blood-pressure the purpura would be universal. In addition, from the horizontal position of the infants the force of gravity does not favour purpura of the legs as it does in patients who are up. Since this paper was read Dr. R. S. Trevor has suggested that the presence and frequent changing of napkins may play a part in determining the occurrence of purpura on the lower part of the abdomen.

Usually the hæmorrhages are small, but they may be so closely set as to make the skin of the abdomen almost uniformly purple when seen from a distance. In a case under the care of Dr. E. I. Spriggs, to whom we are much indebted for its use, there were large hæmorrhages 2 in. in length on the chest. From the heart's blood of this case Dr. H. R. Dean, of the Lister Institute, isolated *Bacillus enteritidis* (Gaertner) in pure culture. The average duration of the diarrhœal disease was forty-one days, the extremes being two days and eighty days, but in all except one case the duration was more than two weeks. The purpura was usually a late phenomenon and appeared on an average on the thirty-fourth day, that is, a week before death. It is therefore connected with cachexia rather than acute infection or toxæmia. Though usually seen shortly before death, in one instance, six hours before, the purpura is not always terminal: in one patient the rash disappeared and the child improved, but the diarrhœa returned and proved fatal two weeks after the eruption had vanished. In another case there were three crops of purpura, seventeen, eight, and two days before death respectively.

Special note was taken to see if transfusion or the administration of horse or other serum could have had any influence in causing the

purpura. But in most instances the appearance of the purpura preceded transfusion or the use of serums.

Our cases do not suggest any close relation between purpura and the œdema which sometimes occurs in children after gastro-enteritis (Fairbanks [2], Dewolf [1], Hume [3], and others). It would not be unreasonable to suppose that if intestinal toxæmia gives rise to œdema, a more severe toxæmia would induce hæmorrhages, and that a case might first show œdema and later purpura as the toxæmia became progressively more severe. The suggestion that purpura is due to a hæmic infection is attractive, but we have little proof to offer, as bacteria were only found in the blood in one case, that mentioned above. In one case only was there evidence of infantile scurvy. Our notes do not justify any expression of opinion on the question whether or not renal insufficiency plays any part in the production of purpura.

Prognosis.—As judged by our eleven cases of purpura, all of which were fatal, the prognosis is extremely grave. Voelcker, however, says that it is by no means necessarily a fatal sign, and the events already described in two of our cases suggest that recovery might occur.[1]

Conclusions.

(1) Symptomatic purpura in infective diarrhœa mainly occurs on the abdomen and chest of infants under the age of 1 year.

(2) It is usually a terminal phenomenon in prolonged cases.

(3) The prognosis in these cases is extremely grave.

REFERENCES.

[1] Dewolf. *Arch. Pediat.*, New York, 1902, xix, p. 895.

[2] Fairbanks. *Amer. Journ. Med. Sc.*, Phil., 1903, cxxvi, p. 443.

[3] Hume. *Brit. Med. Journ.*, 1911, ii, p. 478.

[4] Thomson. "Clinical Examination and Treatment of Sick Children," Edinb. 1908, p. 226.

[5] Voelcker. *Trans. Med. Soc. Lond.*, 1905, xxvii, p. 33.

[1] In a case which came under observation after this paper was written, recovery occurred after a single hæmorrhage, the size of half-a-crown, had appeared in the skin of the abdomen.

DISCUSSION.

Dr. E. CAUTLEY said he thought that purpura of the type described by Dr. Rolleston was an extremely common phenomenon in all babies who were suffering from cachexia or marasmus, whatever the causation. He had always regarded it as a very grave symptom, and frequently it was a terminal phenomenon. He doubted whether there was any connexion between the purpura, which was nearly always petechial, and infective diarrhœa *per se*. He thought the only connexion was in the fact that the infective diarrhœa was a prolonged attack, leading to profound malnutrition, and consequently the purpura. The distribution of the purpura in his cases was exactly similar to that which Dr. Rolleston described. It was far most frequent in the lower part of the abdomen, between the umbilicus and the pubes, and towards the groins. Sometimes it involved the whole abdomen; frequently it was on the chest, and sometimes it appeared on the inner side of the arms and thighs. Occasionally it was widely distributed. Though it was a serious disease or symptom, he agreed with Dr. Rolleston that it was not necessarily of fatal import. One sometimes saw it in patients on their admission into hospital. Some of these recovered on suitable treatment, with the improvement of the nutrition. He had not seen purpuric eruption of this type in acute infective diarrhœa, unless the attack had been of prolonged duration.

Dr. F. J. POYNTON said he did not feel so sure that these cases had no immediate connexion with infective conditions. When one began to subdivide purpura into various kinds according to the distribution and other characters, one encountered difficulties. This year he saw a case of purpura in a child with acute infective diarrhœa who was only ill five days, and who died very rapidly of heart failure. Unfortunately there was no post-mortem examination. The purpura appeared on the abdomen. Purpura, he suggested, was much more common in some outbreaks of acute infective diarrhœa than in others, just as he believed that œdema, which was so much talked about this year, was more common this year than it had been in recent years. He would like to hear if Dr. Rolleston was not of the opinion that both purpura and œdema were more frequent in some outbreaks of gastro-enteritis than in others. He would also like Dr. Rolleston to make it clear whether he was speaking of a special form of purpura, which some members laid stress on in these marasmic children, or of the purpura in the acute infective diarrhœa of infants.

Dr. F. PARKES WEBER asked if in these purpuric cases small retinal hæmorrhages were met with also. In regard to the cases of purpura in children associated with diarrhœa, the cases of purpura in grown-up people associated with tuberculosis might also be referred to. In a few cases undoubtedly purpura associated with tuberculosis could be regarded as a mode of fatal termination of the tuberculosis. In one case he remembered that the lungs after death looked

more like spleens (owing to the lung tissue being the site of diffuse interstitial hæmorrhage), and not like ordinary spleens, but like " red hardbake " spleens, because there were white caseous patches in the dark red hæmorrhagic lung tissue. There were also cases of minor forms of purpura associated with tuberculosis, in which retinal hæmorrhages might occur, and in which recovery from the purpura took place. He would like to know in regard to purpura in children associated with diarrhœa, if retinal hæmorrhages had been found either in fatal cases or in those which recovered.

The PRESIDENT said he failed to see why this condition was specially associated by Dr. Rolleston with acute diarrhœa, because all members would be familiar with it as a terminal condition in all cases of wasting. He asked if the authors thought there was anything distinctive about the purpura in connexion with acute infantile diarrhœa. The name he had given it was " ante-mortem purpura," because he regarded it as an ante-mortem sign. Having watched the condition with great interest for some years, he could say that he only remembered two cases in which the patient had recovered after this form of purpura had become established. He regarded it as a very definite type of purpura, and its situation was striking. He believed it was usually associated with prolonged intestinal disturbance, and the wasting of the abdominal wall was often out of proportion to the wasting in other parts of the body. He regarded the great wasting of the abdominal wall as the predisposing cause of the purpura, owing to the want of support to the vessels in the abdominal wall.

Dr. MORLEY FLETCHER asked if in many of the cases of purpura which came under the author's care cultures of the blood had been made before or after death. Dr. Rolleston mentioned one case which gave the Gaertner bacillus in pure culture, and this reminded him of a patient whom he saw at St. Bartholomew's Hospital this summer. This was an infant who was admitted suffering from what was thought to be epidemic diarrhœa, and in that case the paratyphoid B bacillus was found in pure culture in the blood. The child did not survive its admission more than forty-eight hours. Hæmorrhages occurred over the whole of the body and limbs; and post mortem there was found thrombosis of the cerebral veins. In that acute fulminating type of case the hæmorrhages were not of the type described by Dr. Rolleston with which one was familiar, restricted mostly to the abdomen and chest, in the form of fine petechiæ. There were in addition large patches of hæmorrhages on the neck and limbs. He agreed with what had been said as to the frequency of the type of hæmorrhage described by Dr. Rolleston in many cachectic states in infants other than epidemic diarrhœa. Many years ago he also had been disposed to regard it as a sign of impending death, but in the light of later experience he looked upon it now less dismally. Still it was a very grave sign.

Dr. H. D. ROLLESTON, in reply, said that the object of the paper was to deal with the incidence of purpura in infants who had had, or were suffering

from the effects of, acute infective diarrhœa. Before the cases were analysed there was not any idea that the purpura occurred more frequently in cachexia due to infective diarrhœa than in cachexias due to other conditions; and it was of course fully recognized that purpura might be due to other cachectic conditions. Although the cases of infective diarrhœa in which purpura supervened had existed for some considerable time, this was not universal, for in one case purpura appeared on the second day of acute infective diarrhœa. He agreed with Dr. Poynton that œdema after infective diarrhœa appeared to be more frequent in some epidemics of diarrhœa than in others, and that the same was probably true of these purpuric eruptions. Retinal hæmorrhages had not been looked for, and blood cultures had not been made during life, for on consideration the technical difficulties of obtaining blood from the emaciated infants seemed too considerable.

Congenital Flexion of the Proximal Interphalangeal Joints of the Fingers.

By DUNCAN C. L. FITZWILLIAMS, F.R.C.S.

I FEEL that I must offer an apology to the Section for taking up its time with the consideration of a slight and usually unimportant abnormality of some of the smallest diarthrodial joints in the body. I bring before the Section some examples of a curious condition which, though it is mentioned in some of the text-books, is very inadequately and, as far as my own observations extend, is rather faultily described.

Congenital flexion of the proximal interphalangeal joint, or, as I prefer to call it hook-finger, is always most marked in the little finger, which is flexed to a right angle at the first interphalangeal joint, and resists any attempt to extend it beyond, though freely movable within this limit. The metacarpo-phalangeal and distal interphalangeal joints nearly always show a compensatory hyper-extension, so that the deformity resembles that of a hammer-toe acquired in later life. The deformity, if it affects the ring finger, is less marked, while the other fingers usually escape; in severe cases, however, all the fingers are affected, but in a lessening degree from the fifth to the second digit. I have never seen the thumb affected, nor are the other joints of the fingers flexed in any way. The condition is almost always bilateral, in only one case have I observed it to be unilateral.

Like other congenital deformities, it is most frequently noticed during childhood, and the first case I saw was that of a little girl brought to the hospital by her mother on account of the hook-like appearance of the digits. It may, however, persist to adult life unrecognized, and I am indebted to Mr. Lovell for the case of the girl, aged 18 years, in whom all the fingers in each hand are still in the flexed position; it was first noticed in her case at the age of 10.[1]

The deformity is, I believe, a true congenital deformity, that is to say, it is a developmental error, analogous to the congenital elevation of the shoulder described by Sprengel, and not a condition which is merely acquired before birth. My reasons for this opinion are that it is constant in its appearance, always affects the same joints, is usually bilateral; in severe cases it affects all the fingers in a constant and lessening degree from the fifth to the second; and, lastly, it is sometimes associated with other true congenital deformities of the same character, to which I will refer immediately. It is difficult, if not impossible, to imagine a state of affairs due, say, to bad packing *in utero*, which would maintain flexion of one interphalangeal joint long enough for contraction to be acquired, and at the same time permit the proximal and distal joints of the same digit to escape unscathed.

In adults, hook-finger has at first sight the appearance of a Dupuytren's contraction, but differs in that it dates from early childhood; no bands of palmar fascia can be felt, while the metacarpophalangeal joint is hyper-extended. There is nothing in common between it and the conditions known under the terms "snap-finger" or "hammer-finger"; it can easily be differentiated from any of the various forms of claw-hand.

In the "Manual of Surgery," by Rose and Carless, it is referred to under the term "Congenital contraction of the finger," and among others the following statements are made: "It is frequently inherited, and usually limited to the little finger; it may be associated with congenital hammer-toe. It is due to contraction of the central prolongation of the palmar fascia in the finger . . . the first phalanx is hyper-extended, and the second and third flexed." As far as my experience goes, I can confirm none of these statements, with the exception of that referring to the little finger; while the last I must controvert, as I have

[1] Since coming this evening a member of the Section has been kind enough to show me his own hands, which at birth were the subject of this abnormality, the left hand having the three inner digits affected severely, while the right-hand little finger only was slightly affected.

never seen the third phalanx flexed, but always in a position of hyper-extension.

I have not in my cases been able to trace any hereditary influence; though, reasoning from other congenital deformities, one would expect this to be the case. I am unaware of the condition of congenital hammer-toe, though quite familiar with the congenital displacement of the second toe, in which all the toes are extended; but the termina phalanges of the first and third toes lie in contact underneath that of the second toe; occasionally the second toe lies underneath the others, but still remains in a straight position; but I have never seen this displacement of the toe in association with the flexion I now speak of.

Hook-finger is usually associated with great laxity of the ligaments of the neighbouring joints. In one of the cases shown this is seen in an exaggerated form, all the digits being capable of marked hyper-extension; nearly all the joint ligaments of this particular child are very lax



FIG. 1.

Skiagram of the little finger of Case II, aged 8 months.

and even dislocation of both knees is easily produced. Other congenital abnormalities may accompany it, such as double talipes equino-varus, hydrocephalus, and a curiously thumb-like big-toe.

That contraction of the palmar fascia has anything to do with the cause of the flexion I do not believe, as relaxation of that structure by flexing the finger into the palm never produces any alteration in the range of movement of the joint at fault, nor does the hyper-extension of the metacarpo-phalangeal joint suggest tension of this structure. The same argument disposes of the possibility of its being due to contraction of the long flexor tendons. It is far more likely that the limitation of movement is ligamentous in origin and due to congenital shortness of the anterior or glenoid ligament; the abnormal condition of the ligaments of other joints is, I think, suggestive.

Abnormality in the shape of the ends of the bones is present at birth. Fig. 1, an X-ray of the little finger of a child, aged 8 months,

shows a forward development of the head of the proximal phalanx, together with a forward lipping of the base of the middle phalanx. Both these points can be seen again in fig. 2, the X-ray of the adult hand, where the thickening of the base of the middle phalanx is especially well seen; a distinct backward curve can be seen in the terminal phalanx owing to the constant hyper-extension.

If allowed to persist uncorrected, changes take place in the ends of the bones which later make it difficult to straighten the joint even after the ligament is stretched. In adult life, if the deformity is still present and more than one finger is affected, the usefulness of the hand may be interfered with and the wage-earning capacity diminished.

It is worthy of note in passing that a similar condition of flexion is present in the gibbons, a family which is probably in closer relationship

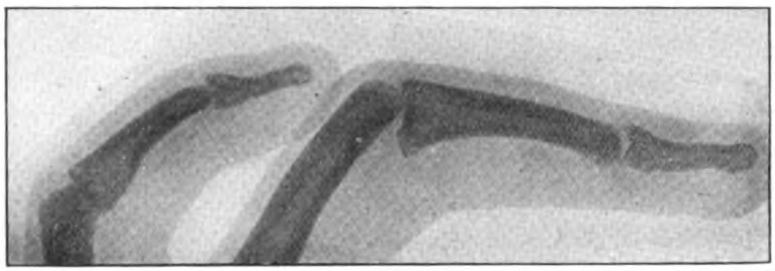


FIG. 2.

Skiagram of little and ring fingers of Case III.

to man than any other, but in them it is most marked perhaps in the third and fourth digits.

When seen early the treatment of the deformity consists in stretching the anterior ligaments of the joint at fault by gentle and frequently repeated efforts at extension. Care must be taken to apply the pressure only through the proximal and middle phalanges or the hyper-extension of the other two joints will be increased, while little effect is produced in the affected joint. So far I have not had occasion to divide the ligament, but should this be necessary it would be best effected by passing a fine-bladed tenotome into the joint from the side and detaching the ligament from the front of the base of the middle phalanx. This would not interfere with the insertion of the tendons in any way. The finger should then be kept straightened on a light splint.

CASES SHOWN.

(I) A boy, aged 1 year, with the little finger of both hands in the characteristic attitude; the ring fingers are bent almost as much as the little fingers, while the middle fingers of each hand are only slightly affected.

(II) A boy, aged 9 months, with the little fingers in the position described. Great hyper-extension is allowed of all the other fingers, while the ligaments of the knees are so lax that dislocation is allowed. This child suffers from other congenital defects: an almost hydrocephalic head, double talipes, equino-varus, double inguinal hernia.

(III) A girl, aged 18 years, showing the persistence of the condition into adult life; all the digits with the exception of the thumb are affected, the deformity increasing in severity towards the little finger. This case has been lent me this evening by Mr. Lovell, F.R.C.S.

DISCUSSION.

Dr. PARKES WEBER asked if other congenital deformities occurred in such cases, and if they occurred he would like to know whether they were of a kind explicable by some malposition *in utero*. The association of abnormalties in the shape of bones when present did not prove that congenital abnormalities in the shape of limbs were necessarily dependent on the abnormal shape of the bones. Bones and ligamentous structures could adapt themselves to fixed faulty positions, and their abnormalities in shape might be secondary to intra-uterine malposition of limbs.

Dr. JOHN THOMSON (Edinburgh) said he had been interested in the condition for some years. He saw it first in a marked degree in two children, brother and sister, who were typical examples of osteogenesis imperfecta. He had them under observation still; they were now about 13 and 16 years of age. He showed photographs of them. He had found a somewhat similar condition of fingers two or three years ago in a little boy aged 12 months, who ever since birth had shown an inability to extend the hip-joints, knee-joints, and elbows as well as the joints of the fingers. He believed the toes were also slightly affected. The toes were very long, and did not move as well as they should have done. He had seen one case in which the child was quite normal in all other respects; four out of the five cases of this condition he had seen had other abnormalities also; for example, in two there were marked deformities of the ears; he showed photographs of these. It had not occurred to him to suggest as the cause a malposition *in utero*. As far as he could make out, the chief change seemed to be in the articular surfaces of the bones and not to affect the tendons and ligaments.

Mr. FITZWILLIAMS, in reply, said it was very difficult to think of any position occupied by the child *in utero*, which could be responsible for keeping the proximal inter-phalangeal joint flexed sufficiently long to cause developmental errors, and leave the other phalanges free. That view was also weakened by the fact that it was always most marked in the little finger, and less in the others. In one child it was associated with other congenital malformations, for the child had club-foot. There was usually an association with extremely lax joints. In the case of this child, he could lay its fingers on the back of its hands, and he could dislocate both its knees. The child also had hydrocephalus. He did not think these conditions or the malformation of the ear in Dr. Thomson's case could be explained by posture.

Section for the Study of Disease in Children.

December 15, 1911.

Dr. G. A. SUTHERLAND, President of the Section, in the Chair.

The Treatment of Tuberculous Joint Disease in Children.

Addresses Introductory to a Discussion on the Subject.

(I) INDICATIONS FOR SURGICAL INTERFERENCE, WITH REMARKS AS TO THE AFTER-RESULTS.

By A. H. TUBBY, M.S.

THE terms of the reference are sufficiently vague to allow me to speak of the surgical treatment of joint disease in children somewhat widely, but the time allotted compels me to be brief. The discussion to-day has special reference to the indications for surgical interference, and the nature of the operations done. I have been asked to include in my remarks some notes on the treatment of abscesses; and finally, the after-results will be touched upon. It has been agreed that the various forms of tuberculous spinal disease and its complications render the ground too wide to be traversed, and therefore they are excluded. You will observe that, as befits this Section of the Royal Society of Medicine, we are dealing with the disease in children. It appears to me, however, expedient to extend that age, in this case, to the end of the fifteenth year, as it may be said to mark the end of the very active period of growth in bone; and, certainly up to this time that structure retains the characteristics of early life, and its reaction to tuberculous infection. It is to be hoped that the speakers will not limit themselves to hospital

and the poorer class of patients, but that they will also deal with well-to-do patients, although it is generally recognized that for the latter class operative interference is less frequently required. Time will not permit me to allude to the treatment of deformities arising from tuberculous arthritis, except in so far as they may necessitate operation.

In order to make quite clear the views which I entertain with regard to tuberculosis in childhood I may state that in a general sense I am in entire agreement with the opinion of Augustus Wilson,[1] who has stated "that tuberculosis of joints can be considered as such only in its incipiency, and that all conditions following this are to be looked upon as avoidable sequelæ." This axiom involves the postulates that early recognition of the affection and proper treatment, followed by careful and long-continued observation, are essential to the thorough and radical cure of the disease, without deformity and with the minimum loss of function.

Looking back upon my opportunities for clinical experience from 1882 onwards—experience which has been gained in the wards of two general hospitals, at the Evelina Hospital for Children, at the Royal National Orthopædic Hospital; and at the Hospital for Hip Disease, Sevenoaks—I am conscious that the conception of tuberculous arthritis has been undergoing during the last decade a radical change. Briefly put, we may say that, whilst formerly the affection was regarded simply as a joint disease to be treated as such, we now look upon it as a loss of resistance to the invasion of tuberculosis in a patient, manifesting itself as an inflammation and degeneration of one or more joints; or, to put it more baldly, the earlier idea was—the joint first and the general condition afterwards; whereas the present conception is that of the general condition of the patient first and the joint secondly. In adults this is true to a certain extent, but in children this remark is most apposite. If we contrast the reactions of adults and children to tuberculous bone affections we observe that the natural processes do not wall off the disease with such frequency as in adults. Painter remarks,[2] "This is apparently antagonistic to the fact that tubercle tends to become arrested more often spontaneously in children than in adults." In children, when an operation is performed, the highly vascular and cellular spaces are opened up with knife and chisel, and dissemination of infection into new and unguarded localities takes place.

[1] *Pennsylv. Med. Journ.*, January, 1906.

[2] *Amer. Journ. of Orthop. Surg.*, Boston, 1905, iii, p. 25.

In fact, whilst in adults the healing of tubercle is accompanied by the formation of a barrier of dense fibrous tissue, in children the young and rapidly growing tissues do not assume this sclerotic action. We may take it, then, that in children the general resistance to this disease is greater than the local walling-off power, and in adults the converse is the case.

In addition to these pathological observations, the truth of which it is difficult to deny, there are certain factors which are responsible for the change in surgical opinion during the last decade. These factors are four in number, namely :—

(1) The climatic treatment of bone and joint tuberculosis.

(2) The employment of Röntgen rays.

(3) The improved use of the new tuberculins, and the evolution of methods designed to check and control their action.

(4) The recognition of the value of supporting appliances, their wide use, and the ingenuity which has been displayed in inventing new apparatus or modifying old ones to meet various conditions of deformity or limitations of movement.

(1) Tuberculosis of joints should be treated by open-air methods, exactly as pulmonary tubercle is, and results are obtained which are very gratifying. In the year 1902, at the suggestion of Dr. Dawson Williams I collected statistics from some of the children's hospitals in London, and embodied the results of the observations in my articles: "The Urban Treatment of External or Surgical Tuberculosis,"[1] and "Is the Urban Hospital Treatment of External or Surgical Tuberculosis Justified?"[2] Also, "Tuberculous Cripples" in Kelynack's "Tuberculosis in Infancy and Childhood," p. 188,[3] and "Tuberculous Bone and Joint Disease in Childhood: its Effects on the Duration and Usefulness of Life."[4]

I venture to think that these articles may have been instrumental in inducing the trustees of the Treloar Hospital at Alton to alter their original conception of the objects of their institution, and to admit for treatment tuberculous rather than other kinds of cripples. The articles which I wrote showed first, that children with tuberculous joint disease are not fit subjects for systematic treatment in the wards of hospitals in towns and cities; and secondly that, when treated conservatively in the country, the number of operations required was much diminished; the

[1] *Brit. Med. Journ.*, 1903, i, p. 454.
[2] *Practitioner*, 1903, lxxi, pp. 313-19.
[3] London, 1907, Baillière and Co.
[4] *Brit. Journ. of Tuberculosis*, 1907, i, p. 214.

severity of the operations was decreased, and the results were more satisfactory in so far that more cures were obtained, and the cures were more permanent. Since these dates a great mass of material has come to hand, bearing out fully the conclusions of the writer, and enforcing these lessons. My opinion is strengthened by the results obtained at the Treloar Hospital and College at Alton, and at the Sevenoaks Hip Hospital. Of the former institution, Mr. H. J. Gauvain is the Resident Medical Officer, and I am indebted to him for the following statements: "The great majority of patients, taken early, are discharged with good movement. The wage-earning capacity of patients is, I believe, little, if at all, impaired in early cases, provided that judicious employment is chosen for the patient." I asked Mr. Gauvain to inform me as to the causes of death. He says: "You may be interested to hear that out of 336 cases, which have been under treatment here so far, only one death has occurred, and this case was that of a boy suffering from hip disease, who developed tuberculous meningitis eight days after admission. You ask me whether I have been compelled to operate. In only one case have I operated, for a caseous abscess of the hip. . . As for my opinion on the value of operative interference in the cases which have come under my notice, the results have so commonly been bad that it has made me very averse to operative interference on these joints."

We now pass on to (2): The use of Röntgen rays. In diagnosis they are invaluable, but the photographs must be of the best possible type, and so clearly focused that all the details of the structure of bone are quite distinct. By their help we can see the beginnings of the disease; we can watch its progress; we note the effects of our treatment, and check and regulate our methods; and, I may add that in some resistant forms of tuberculous synovitis systematic exposure to the X-rays has appeared to have some effect in causing retrogression of the disease. In all cases I now employ the X-rays systematically for the diagnosis of the disease and regulation of the treatment. As one proof of their practical value I may say that I have been astonished to find how frequently tuberculous disease is situated in the acetabulum as well as in the head of the femur. In fact, primary or associated acetabular disease exists much more frequently than is suspected.

(3) As to the use of tuberculin, we are happy in being able to hear the opinion of Dr. Butler Harris on this point.

(4) By the ingenuity of surgeons who have given special attention to the subject patients have been enabled to get up much earlier, and to be

treated by what may be called the ambulatory method, with all its advantages to general health, without running the risk of lighting up the disease or incurring a severe and disabling deformity.

It appears that I scarcely need evoke the opinion of those in this room on the treatment of very early and uncomplicated tuberculous arthritis. At any rate, the majority will agree that rest, protection of the joint, prevention of deformity and treatment of the sanatorium type are indicated. Tuberculous arthritis is a disease varying very much in intensity. In some cases the affection is virulent and the course of the disease is acute, resulting in rapid degeneration of the joint with extension to the shaft and the development of osteomyelitis in the cancellous tissue on the one hand, and on the other hand in rapid and destructive changes in the joint. In other cases the disease is comparatively mild and runs its course without any complications such as abscess, tuberculous diaphysitis, or the formation of sinuses, and the part recovers with little or no loss of function. In the early stages of the disease there is no possibility of telling which type exists, and unless we watch the progress of the disease constantly by using the Röntgen rays and by clinical observation and note the reaction of the patient to vaccination, we are quite unable to form an opinion as to its future course. Some cases of tuberculosis are obstinate and refractory, due not so much to the local conditions as to what, for want of a better term, is called "loss of resistance" on the part of the patient. It is in those cases that the question arises, "Shall we interfere surgically or not when the improved general methods I have indicated seem to have failed?" I say advisedly "seem to have failed," because the treatment of this affection is, in any case, a long business. It is often months before the inflammatory and destructive conditions subside and healing sets in, and it is usually two or three years before a joint can be regarded as functionally cured, whether it is mobile, ankylosed, or partly sound at the end of that time. So that my inclination is to persist or persevere with climatic and conservative measures rather than to allow my hand to be forced to interfere surgically. We have, however, under the present conditions in this country, to weigh carefully the patient's opportunities of climatic and sanatorium treatment. Unfortunately, it may be said with truth, and I trust without offence, that the successful therapeusis not only of pulmonary but also of articular tuberculosis is largely a question of means and opportunity. Sufferers with ample resources stand a better chance of recovery and useful joints than impoverished hospital patients.

Nevertheless, it is my deliberate opinion that in children, and to a less extent in adults, where other factors, such as the wage-earning capacity and the inability to obtain sufficient rest must be weighed, the less surgical intervention is practised in children under 15 years of age the better is the prospect of recovery of the affected part, of its regaining its functions, and of usefulness in life. It may be urged that many lesions, which have become quiescent in childhood, light up in adult life. No doubt this is so, but we must consider two points, namely, that several years of freedom from the disease have been gained, and the part has been useful during that time; also that tuberculous arthritis in adults is more amenable to radical surgical measures than in children. In this expression of opinion I am supported by many of the most recent writers on the subject, notably by C. F. Painter,[1] J. W. Sever,[2] and Augustus Wilson.[3] The last named goes so far as to say that "we now look upon the avoidance of surgical interference as most conducive to the recovery of the patient."

These remarks, then, apply not only to the cure of the disease, but to the question of complications such as affection of other joints and the onset of general tuberculosis. There is reason to believe, and some surgeons—e.g., J. W. Sever[2]—think they have evidence to show, that frequent curettings have been followed by implication of other joints, much earlier than when the case has been left alone. And some of us have been shocked from time to time by the sudden onset of tuberculous meningitis after a very trivial operation—e.g., scraping a finger which is the site of tuberculous dactylitis. The general complication has followed on the operation so quickly and so unexpectedly that it is reasonable to regard one as having a distinct causal relationship to the other.

Bearing all these points in mind, I pass on to speak more specifically of operations, and I have been asked amongst other points to elicit the views of this meeting upon the treatment of abscess. Its existence does not necessarily imply immediate evacuation. If the pus is deeply placed, and if the patient is placed at rest in good air, the fluid contents are sometimes absorbed, and the caseous matter becomes encapsuled. If, however, in spite of these measures the abscess increases in size its sac should neither be allowed to become adherent to the skin nor to open spontaneously, as an opening often forms in an undesirable

[1] *Amer. Journ. Orthop. Surg.*, Boston, 1905, iii. p. 24 et seq.
[2] *Journ. Amer. Med. Assoc.*, Chicago, 1910, p. 2128.
[3] *Pennsylv. Med. Journ.*, January, 1906.

position and is liable to septic infection. As a general rule, I am strongly in favour of that form of treatment introduced by Ménard at Berck-sur-Mer and rapidly coming into favour in this country, owing to the advocacy of Messrs. Jacques Calvé and H. J. Gauvain. The details are fully given in the *Lancet*, March 5, 1910, and the article is worthy of the most careful perusal.

When pus continually re-forms it is generally due to the extension of bone disease or to loose sequestra, which can be ascertained by means of Röntgen rays. Then, I advocate opening the abscess, invariably by more than one incision, so as to permit free access to the cavity and allow the pockets to empty. The fluid contents and the sequestra are removed without any violent scraping of the sac wall, which should be merely rubbed with a soft swab. Pure carbolic is then applied and left in for about a minute and washed out with absolute alcohol. The cavity is then dried and closed immediately so as to obtain primary union. It frequently happens that a little fluid accumulates subsequently beneath one of the wounds. A sterilized probe is then inserted, the fluid pressed out and the wound allowed to heal. I venture to say that drainage tubes are an abomination and a source of grave danger as they involve the risk of secondary infection; and, in the opinion of careful observers the activity of the tubercle bacillus is accentuated by the presence of pus-producing organisms. Further, there are grounds for saying that the onset of lardaceous disease is often determined by mixed infection.

The treatment of sinuses is always a matter of great difficulty. In 1908 injections of bismuth paste were introduced; but, as some patients were poisoned, paraffin wax has been substituted. Its efficacy and its mode of action are still *sub judice*.

Passing on now to the question of more extensive operations for tuberculous arthritis, "I do not believe that there has ever been a surgeon who has had to treat few or many such cases who has not searched for a radical plan of procedure, for some way of shortening the treatment and lessening the destructive action of the infection."[1] "Occasionally, we become enthusiastic temporarily over attempts to extirpate tuberculous foci in childhood. These radical operations, although commendable and to casual consideration reasonable, are not attended with such results as to lead the surgeon to persist."[2]

[1] H. L. Sherman, *California State Journ. Med.*, March, 1907.
[2] C. F. Painter, *Amer. Journ. Orthop. Surg.*, Boston, 1905, iii, p. 104.

What are the tests of the success of operation? They are complete eradication of the disease from the parts and retention of perfect function. We must admit that nearly all operations fall short of these ideals. Too often cure by operation means mutilation of the part and sacrifice of its functions. This remark may require modification when we deal with what may be termed focal or para-articular lesions. However, let us discuss what conditions call for erasion and excision of the joints of the lower extremity. As to the formal excision of joints, formerly done in great numbers, there is ample justification for the criticism that there is scarcely any operation which looks so promising and satisfactory on the operating table, and proves so disappointing afterwards. The disease is not cured because the infection is either too extensive or is not entirely removable; function is impaired or lost, and the patient is crippled by the deformity.

As to excision of the hip-joint, it has its opponents and its advocates. Of the former Professor Howard Marsh is perhaps the chief in this country, and has been most consistent, and of the latter Mr. G. A. Wright is prominent. I feel myself compelled to say that the opponents are more numerous than the advocates, their opinions seem to me more weighty and convincing, and their prognostications as to the final results more exact. If we refer to a recent text-book—the fifth edition of "The Operations of Surgery," by Messrs Jacobson and Rowlands—we find the matter discussed *in extenso*, and it appears that excisions may be done only in those cases where the difficulty in obtaining treatment on those lines which are known to give the best results, namely, by prolonged rest for years in special institutions (such as the country branch of the Alexandra Hospital, the Sevenoaks Hip Hospital, the Treloar Home at Alton, and the Liverpool County Hospital), is found to be insuperable. An excision of the hip is often an imperfect operation because of the involvement of the acetabulum and pelvis by the disease in such a way as to render its complete removal a matter of grave danger, and often of impossibility. If there are points which have been impressed upon me by systematic examination by X-rays of hip-joint cases coming under my notice, they are that primary disease of the acetabulum, formerly thought to be very rare, is not so; and that the number of cases of simultaneous involvement of both the femur and acetabulum is much greater than is thought. I begin to suspect that the total of the two conditions will prove to be not far short of 50 per cent. of the whole number of cases. One important point should always be kept in mind before performing the radical operation for tuberculous arthritis.

We should satisfy ourselves previously that we are able to remove the diseased area *in toto* without destroying the functions of the part, and without endangering our patient's life, and I ask, Has this always been possible when formal excision has been decided upon? An excision of the hip often proves to be an imperfect operation because the whole of the diseased part cannot be removed. The final results of the operation have been ably set forth by Mr. Ralph Thompson in the *Guy's Hospital Reports*, 1905, vol. lxv. He says: "Of 200 cases in which the hip-joint has been excised, seventy may expect to be the subjects of progressive disease, and of these thirty may be expected to die from continuation and spread of the disease; fifty-five will rank as partial failures, in that in them sinuses persist; seventy cases may look forward to a complete recovery; two cases, however, among these will have a flail-joint. The best result, a useful joint with free mobility and without sinuses, will fall to the lot of twenty-five of 200 people, or one in eight of those operated upon." Mr. Thompson has arrived at these conclusions from a study of forty cases. In two cases more than 10 years, in fourteen cases more than 5 years, in four cases more than 3 years, in twelve cases more than 1 year, and in five cases less than a year had intervened between the operation and the date of report. I question if these are better than those obtained by rest and splints. To some surgeons the presence of a tuberculous focus may be temptation to operate. I suggest that it is better to deal with the conditions as they arise, and try prolonged rest in the best surroundings. If abscesses appear, deal with them on the above lines. If sequestra form, localize them by means of X-rays and remove them.

Of the two operations practised upon the knee-joint I regard erasion as much over-rated. If its objects are to eradicate the disease and to obtain movement in the joint afterwards, then I am prepared to affirm that even in the event of cure of the disease fibrous ankylosis and loss of movement follow, or else that the degree of movement is so small as to be useless. Frequently, too, the disease reappears, partly from the irritation to which the part is subjected in its deformed position; and, even if it does not do so, the tibia is so often displaced backwards and outwards, and the knee-joint is so bent, as to render the limb an incumbrance rather than a help. The less the operation partakes of the nature of an arthrotomy and the more it partakes of that of an erasion, the less useful is the limb afterwards. I have performed limited arthrotomy of the knee in cases of localized tuberculosis. In one instance a tuberculous mass the size of a pigeon's egg had formed in the infrapatellar

pouch on the inner side, leaving the movements of the knee-joint entirely free. I dissected the mass from the subsynovial tissue without opening the joint, and the patient had a comparatively useful knee for three years. Then extensive involvement of the joint set in and excision was done. The safest course in tuberculosis of the knee-joint is to give a long and patient trial to conservative treatment, and, if this fails, to give up all hopes of a movable joint and do a formal excision, having regard to the epiphyseal lines, and endeavour to secure sound bony ankylosis and a straight limb. If we pass on to the ankle and the foot, these parts appear to present unusual facilities for operative treatment on tuberculous foci, and in many cases the results have been strikingly successful, especially when a single bone in the foot has been affected, or there is a localized focus in the tibia or fibula. We have, however, to remember that when we come to operate the appearances seen by the X-rays are always less severe than those we find on actual inspection, for the synovial membrane is frequently found to be extensively invaded. Even in dealing with the ankle and foot, those statistics which are available show the best results to be obtained by rest and conservative treatment. James Warren Sever, in the *Journal of the American Medical Association*, December 17, 1910, pp. 2128-2133, has given us comparative statistics on these points: One hundred and one cases of disease of the ankle and tarsus were operated upon, including forty-five excisions of the astragalus, fourteen removals of the lower end of the fibula and eleven removals of the lower end of the tibia. Good movement of the part followed in twenty-nine, slight in twenty-two, none in forty-seven. In only seven was there no subsequent deformity. In sixty-five thickening remained about the malleoli, in twenty-nine some definite distortion of the foot followed. The average duration of the disease from its onset, in the operative cases, until treatment was discontinued was 19·8 months—and of treatment itself 10·1 months. For comparison, results in eighty-eight non-operative cases are tabulated. Motion was good in thirty-two, slight in twenty-three, and none in thirty-three. Deformity was entirely absent in four, thickening was present in sixty-three, and distortion was present in twenty-one. The average duration of disease until cessation of treatment was 16·1 months in ninety-nine cases; the average duration of treatment itself was 6·8 months, giving an average in favour of the non-operative series of 3·3 months from the beginning of the treatment until it was no longer necessary. The figures show that "there was a distinct gain in time, also in the function and lack of deformity of the foot in the non-operative

series." The joints of the upper extremity present problems differing from those of the lower. Whereas the chief functions of the lower limbs are stability and weight-carrying, those of the upper extremity consist of highly educated movements and the importance of preserving them is undisputed. Operative interference, if it is likely to have the desired object, may therefore be considered at an earlier date than in the lower extremity. In children, however, erasion frequently proves to be better than a formal excision when conservative treatment has failed. In the case of the shoulder-joint there is, however, one exception to be made. Other surgeons have noted the fact, as well as myself, that tuberculous arthritis of the shoulder-joint is frequently followed by pulmonary phthisis. In one case I noted the sequence of tuberculous arthritis of the shoulder: a dry, apical pleurisy and phthisis, as if direct lymphatic extension had taken place. This observation may lead the surgeon to attempt eradication of the disease at the shoulder-joint at an earlier stage than would otherwise be the case.

There are two points on which discussion may be profitably provoked, namely, the question of focal, extra- or para-articular lesions, such as are found in the great trochanter in children, and the treatment of tuberculous dactylitis. As to the former, the point which advocates of operation should bear in mind is that there should be a sufficiency—as shown by X-rays—of healthy bone between the lesion and the limits of the neighbouring joint. If there is not, any opening up of the vascular and cellular spaces often leads to arthritis; and, of the comparatively few cases of this nature on which I have operated, I may say that the disappointments and failure to save the neighbouring joint have exceeded the successes in the proportion of three to one. Tuberculous dactylitis has been shown by Ménard and others to be an exceedingly formidable type of disease. Disease of the spine more frequently complicates it than does disease of the larger joints, and it is responsible for an undue ratio of generalized tuberculosis. Conservative and climatic measures should be carried out from the first, and if these do not give an early and satisfactory result amputation of the finger is desirable. The difficulty is to deal with those cases where the metacarpal bones are affected with implication of the neighbouring tendon sheaths. It is an obstinate and refractory lesion, but the same remarks apply to this operation as to operation in any other form of tuberculosis, namely, when conservative treatment has failed, and if an operation is done, let it be thorough.

To sum up: The objects of every form of treatment of tuberculous

arthritis must be: (1) To eradicate the disease; (2) to preserve the functions of the part; and the question is whether these results are more frequently obtained by climatic and conservative measures or by so-called radical operations. It appears to me that the balance of experience gained in treatment during the last decade shows that unquestionably conservative treatment is the better in childhood. Time is saved, the resultant deformity is diminished, and there is less risk of generalized tuberculosis when the treatment is non-operative.

(II) THE RESULTS OBTAINABLE WITHOUT OPERATION.

By Sir ANTHONY BOWLBY, C.M.G., F.R.C.S.

IT is tacitly recognized that the tuberculous diseases of the joints in children stand in a different position from similar diseases in the adult. But it is scarcely sufficiently recognized that this difference is largely due to the much greater softness of the bones and to the fact that the bones are growing in thickness as well as in length, so that consequently if bone is destroyed new bone very quickly takes its place.

I should like to allude briefly to the behaviour of a child's bone in the presence of tubercle, because it explains so much that is of clinical importance.

When tubercle attacks the cancellous tissue of an epiphysis the immediate result is the rapid liquefaction and absorption of the bone salts, so that in a very few weeks an X-ray picture will show considerable removal of the bony trabeculæ. There is but little inflammation, and it is to be noted that the process is not as a rule, but only as an exception, associated with the formation of pus, although large areas of bone may be completely removed by this process of absorption without the development of an abscess. Even dense, compact bone is also destroyed in the same way, and yet it is rare in children to find any pieces of dead bone of large size. The extent to which large areas of bone can be removed without suppuration is well seen in many cases of angular curvature due to spinal caries. But while the ease with which the bone is destroyed on the one hand causes irreparable damage in some parts, such as the head of the femur of an infant, on the other hand it enables the diseased tissues to recover

without operation in a way which is often impossible in an adult. Operations in the past have been commonly undertaken to remove carious bone because it was believed that recovery was impossible if it was left. I have often seen and felt large areas of tuberculous crumbling bone which would in past years have been excised, and which nevertheless have been able to recover under favourable conditions. In such cases the tubercle dies and the rarefied bone, freed from its assailant, is quickly strengthened by the growth of fresh bone and the deposit of fresh lime salts. The diseased bone is removed by the neighbouring and active healthy cells, and all that is not affected too deeply is left to recover, for Nature can easily discriminate between what is irreparably damaged and that which can be repaired, whereas the surgeon can only take away all the inflamed and softened bone, and thus he removes far more than that which is diseased and leaves no framework upon which new bone can be built up. It is by this absorption of tuberculous bone and the growth of new bone that the tuberculous joints of children are enabled to repair themselves in so marvellous a manner. But, while this process of destruction and repair is more easily demonstrated in bone, it is evident that the soft parts also have powers of repair denied to the adult. The influence on the nutrition of all tissues of the power of "growth," inherent in every child, is impossible to estimate. Yet I think that it is certain that where the formation of new tissue of every variety is in active progress throughout the body it must follow that the process of repair, which is, after all, a process of new tissue formation, must be also more rapid and complete. And we do find that thickened and swollen synovial membrane can and does subside in children with a rapidity which is only too often absent in the adult, while, on the other hand, it is but too well known how the tissues of the aged very rapidly succumb to tubercle and show very little capability of resistance or repair.

But, if the recovery from tuberculous disease of joints is to a great extent due to a capacity for resistance and repair, it necessarily follows that the chief factor of which we have the control is the nutrition and health of the child that is attacked. And, whilst it is obviously essential to protect the affected joint from the injury and strain associated with use of the limb, and to prevent that inflammation which follows on such injury, the ultimate defeat of the invading tubercle is to be accomplished alone by reinforcing in every possible way the health of the child, for it is the child alone who can destroy his enemy and repair the damage caused by the attack.

But if it be argued that operation can at once remove, in suitable cases, the whole of the tubercle of the affected joint, I would reply that, even if that is occasionally the case, it does not in any way minimize the necessity for the treatment of the child; for, apart from the fact that it is extremely difficult to certainly remove all tubercle without doing extensive injury, it is certain that in almost all cases of tuberculous disease of joints there is a tuberculous focus somewhere else in the body to which the joint disease is secondary. So that, whatever local treatment may be necessary for the joint, it is still of the utmost importance to make the general health and nutrition of the child our first care, and I have found as a matter of practical experience that if I am able to get a child to put on weight and to get fat the joint disease gives me little further anxiety.

I will not attempt in the time at my disposal to do more than indicate very briefly the general lines on which I think a child with a tuberculous joint should be treated. In the first place, good and plentiful food of an easily assimilated nature is of course essential, and I think that milk should always play an important part in every diet. The bowels should be regulated, and preparations of iron, cod-liver oil and malt should be judiciously administered. All other possible sources of ill-health should be eliminated, and especial care should be taken to remove septic tonsils and adenoid growths, and to see that carious teeth are properly treated.

In the next place, "rest" of both the child and the affected joint is required. The fixation of the joint is to be attained by such splints as may be best suited to the individual case, but well-moulded leather splints are preferable to those of plaster, and, in the case of the lower extremity, extension by weight is often preferable to a splint or will materially aid the latter. When the lower extremity is attacked I prefer rest in bed at first to any form of apparatus which allows of walking, and, even when the joint is one of the upper extremity, I am sure that it is wise to prevent for a time all active exercise, at any rate till the joint has become quite quiet and painless.

There is a very natural prejudice in the public mind against keeping a child altogether confined to the recumbent position, and fears are frequently expressed that the confinement will tell upon the health. I think myself that such fears are groundless, and, on the other hand, the health often improves at once when rest in bed has relieved the pain, and when the pulse and temperature have settled down and good sleep is secured.

Finally, there can be no doubt that the open-air life is most beneficial in these cases, and when it is possible to secure residence in a sunny and bracing climate the benefit is more marked. Even in towns something can be done with open windows and balconies to secure abundance of fresh air, but in the country or at the sea the conditions are of course more ideal. I have used injections of tuberculin R in a considerable number of cases, generally at intervals of about three weeks. I have never known it do any harm, and in some cases it has seemed to be beneficial. It is, I think, worth a trial, but I do not lay great stress upon its use.

The lines of treatment I have sketched are sufficient in most cases, and indeed for all cases, in my opinion, where the disease is in a very early stage. But in cases where abscesses have formed further treatment is required, and it is here that the advent of asepsis has so completely altered the whole picture of tuberculous joints; for, however much harm the tubercle may do, the contamination by the pyogenic organisms adds immensely to the danger to life, and there is no doubt that the great mortality in past years was more directly due to sepsis than to the tubercle itself.

There has been in years past a very great tendency to say, "This patient has already got an abscess, why should we trouble about antiseptics?" "You are going to make an incision into a part which already contains pus, why trouble further about keeping micro-organisms out?" I am afraid there is a certain amount of that belief still prevalent; but, as a matter of fact, if there is one condition more than another which absolutely necessitates the most strict antiseptic precautions, it is this condition of tuberculous disease. Think of it for a moment. Here is a patient who has a collection of fluid inside his body; that fluid is largely blood, serum and pus; it is maintained at a temperature of about 100° F. Can you imagine any more favourable nidus for the growth of most of the pyogenic organisms? Is it not very much the same material which you would use if you were cultivating bacteria in the pathological laboratory? But there is something more than that. If you expose healthy cut tissue to the attack of pyogenic organisms, in a large number of the cases, unless the organisms are very virulent and present in undue numbers, the healthy tissues are quite capable of repelling that attack and casting them off and destroying them. But in the tuberculous patient there is a good deal of tissue which is very lowly organized, and which is to a certain extent devitalized; in it a large number of cells are undergoing fatty degeneration. It is a part in which

blood-vessels are notably deficient, because tubercle destroys large numbers of small blood-vessels. Therefore, this tissue possesses very little resisting power, and if it be exposed to attacks of micro-organisms, it is very likely to succumb because it can offer very little resistance to their action. If pyogenic organisms obtain an entrance to a place where there is tuberculous disease, they have an infinitely greater opportunity of destroying the tissues than in healthy people. Can you wonder, then, that it is absolutely necessary to use the most strict antiseptic precautions in cases where there is tuberculous suppuration? And not only does tubercle predispose the tissues to this successful invasion, but the pyogenic organisms, in their turn, damage the tissues and allow them to be more easily invaded by the tubercle. A tissue which is being destroyed by a process of acute suppuration of course offers very little resistance to any other micro-organisms, and consequently to the tubercle bacillus. So once you have a mixed infection—that is to say, both tubercle on the one hand and pyogenic micro-organisms on the other—there is a vicious circle established. Tubercle favours the growth of pyogenic micro-organisms, and those organisms in their turn attack the tissue and render them more susceptible to tubercle. There can be no question about it that the advent of sepsis to a part which has hitherto been the seat of tubercle only immediately results in the absorption of septic material and in fever. It is because of these circumstances that antiseptics have become of such paramount importance in the treatment of tuberculous disease.

It has been suggested at different times that different antiseptic materials should be introduced into the abscess cavities and perhaps left there; for instance, iodoform emulsion is used for this purpose by many surgeons. I have used it myself, but I did not see any particular gain from it. Some surgeons have advocated the use of sulphur. I have also tried that, and again I did not see any benefit. So that my object in opening such an abscess is to open it aseptically, and, having cleared out the contents most completely and very gently, to see that it is adequately drained for, at any rate, a few days. In many cases the drainage tube can be taken from the cavity within a week or two. I think there is too much tendency to make numerous incisions. I am convinced of this, that in many cases where an abscess is already opened, you should be very careful about deciding upon the advisability of making counter-openings unless the abscess tracks to another place very near to the skin. If you make an incision into a deep-seated tuberculous abscess, say, of the hip, and if in order to reach it you have to pass

across several planes of cellular tissue, you necessarily introduce tubercle into these hitherto unaffected cellular planes, and when the pus drains through it will not only come out by your drainage tube, but it will infect different areas of tissue which are now exposed to it by your incision. So one result of opening a deep-seated abscess through several intermuscular spaces is that there may be an infection of each of those planes with tubercle, and there may be, in consequence, a wide extension of tubercle through the limb. That is what often happens, either with or without incisions, and in a certain number of cases pus makes its way amongst the cellular tissue planes, and infects a great many of them before an opening is ever made. If it has not done so, it is very advisable that in operating you should avoid this complication. It is not always that you can avoid it, but you can always avoid making unnecessary incisions, and it is not always advisable to make the incision for an abscess in the most dependent part, for if you sacrifice everything to doing this it may result in making the patient worse by opening up a large area of hitherto uninfected cellular tissue.

I have thus far spoken only of those cases in which there is either no abscess, or else one which is not yet opened. It remains to allude briefly to those other cases in which there are open and septic sinuses, and which are unfortunately all too common in hospital patients. In this class there is, of course, every gradation in the severity of the local or general sepsis, but in the larger number of them the limb can be saved by directing our efforts to the general health of the child, whilst doing all that can be done locally and by the use of vaccines to obtain aseptic conditions. I think that in these cases there is even yet insufficient belief in the recuperative powers of the children if only they are freed from pain and well fed in good hygienic condition. It is, of course, in this class that operations for the removal of the diseased limb are most often performed, but increasing experience of recoveries from apparently desperate conditions has caused me to resort to amputation less and less frequently; for I have seen children recover from the most extensive emaciation and amyloid disease, with hectic fever, and I have gradually come to consider that hardly any case is really hopeless.

The condition of a joint after recovery from tuberculous disease is a matter on which it is not possible to speak precisely in general terms, for not only must one consider that the utility of the affected limb depends very largely on which of many joints is the one involved, but it is evident that the stage of the disease in which the patient first

comes under treatment is of paramount importance. But it may be said truly that whatever joint is affected, that joint may recover completely in every respect if the disease is diagnosed sufficiently early. I do not say that every such joint will recover, but that there is no inherent impossibility of a complete recovery. In such cases the progress of the tubercle is arrested, and synovial membrane, cartilage, and bone are saved from destruction and restored to health. I have many times seen such complete restoration, even in hip disease, that it was not possible in after years to find any difference between the right and left limbs; and in many other cases, beyond a slight permanent impairment in muscular bulk, the limbs have been equally developed. Indeed, I consider that tubercle is a most curable condition in the joint of a child if only treated early enough and for sufficient length of time. The idea prevalent in the minds of the public that a tuberculous joint is something which is as dangerous to life as tubercle of the lungs is entirely erroneous. Almost all these cases, taken in an early stage, are really curable in the widest sense of the term.

But in proportion as articular bone and cartilage is destroyed and synovial membrane loses all its endothelial surface, recovery of free movement becomes impossible. How can a joint recover if all the structures that compose it are gone? When cartilage, ligaments, synovial membrane and articular bone are destroyed the joint itself is gone, and at the best its place is taken by a scar of fibrous tissue and bone. As regards the lower extremity, there is another point of great importance, and that is the interference with the nutrition of the whole limb and its imperfect development, with consequent shortness and lameness. These results are very little noticed when the ankle-joint is affected, and in only a few cases of knee disease is there any material shortening. For reasons not easy to explain this want of growth is most evident in disease of the hip. Much shortening may, of course, result from destruction of bone and dislocation upwards, but apart from this the nutrition of the whole limb is also impaired. One thing which is of great importance is that the earlier the child is attacked by hip disease the more likely it is to have permanent lameness. In very small children of 1 or 2 years of age the head of the femur is only a tiny thing, and if it is attacked by tubercle but little destruction of bone suffices to destroy the whole head. But there is also in many cases an arrest of the normal growth of the whole limb, leg as well as thigh, and this also is most evident in very young children. In the worst cases there may be ultimately a shortening of the tibia and fibula of more than an inch, and at

the same time a considerable diminution in thickness as well. It is not always by any means that in the joints with the worst tuberculous disease there is the greatest amount of dystrophy, for it may develop in quite slight cases and is not associated with any one particular method of treatment.

In the upper extremity the joints are not so liable to be attacked by tubercle, and of the three large joints the elbow is the most often affected. It is very seldom that a useful joint is not obtained in early cases, and complete restoration is not so important as in the case of the lower extremity, because a little shortening is probably unnoticed and slight stiffness is of little importance when compared with a similar condition causing lameness in the hip or knee. The prognosis, therefore, in all such cases, as far as a useful limb is concerned, is quite good, and of course treatment is here much easier when confinement to bed is not essential.

As far as complications are concerned I will only say that tubercle of the lungs is very rare in my experience, and very few of the children at the Alexandra Hospital for hip disease have suffered either from it or from tuberculous glands in the neck. On the other hand, a certain small proportion of patients die of general tuberculosis, although it is not possible to know whether the infection is derived from the joint itself or from tubercle in the mesenteric glands or elsewhere. A good many children are affected in more than one joint, and this without any tuberculous disease of the viscera, for in some such cases the bones and joints alone seem subject to the disease. Thus, I have seen a child recover from disease of as many as four of the chief joints without at any time being seriously ill or showing signs of visceral disease.

In bringing these introductory remarks to a close I would say that I have ventured to put before you a very favourable view of the curability of tuberculous joint disease in children, and I do so because after an experience of more than a thousand cases of hip disease I have good reasons for my conclusions. I suppose it will be admitted that of all the joints in which tubercle threatens life the hip-joint takes the first place. Yet I have been able to show that in 900 cases treated at the Alexandra Hospital up to the end of 1907 the mortality was less than 4 per cent., notwithstanding the fact that a very large number of the patients were in a very advanced stage of the disease and very septic on admission. And if these results can be obtained in hip disease in the children of the poor it is evident that far better statistics can be shown as to the recovery of patients with tubercle in other joints. Or take the experience of joint

disease in the children of the well-to-do classes. Is it not a fact that they nearly all make good recoveries and that death or amputation are both rare? And does not this point to the conclusion that the best way to treat most of the children of the poor would be to send them out of London to places where they would get fresh air, good food and good nursing?

I believe that at the present time there is more need for hospitals in the country for children with surgical tuberculosis than for any increase in the beds for children in London hospitals. If these cases of joint and bone tubercle could be treated in large numbers in such institutions it would free a great many surgical beds and would at the same time offer the opportunity of good recovery to patients who are often not admitted into hospitals till the disease is too far advanced for a satisfactory result to be obtained, whatever line of treatment may be then adopted. I am certain that at present more real good could be done by the building and endowing of such institutions in the country than by adding to the existing number of hospitals in this city; and I would further point out that for any given sum of money at least three beds could be provided for such cases in the country for the cost of one bed in town.

(III) THE RÔLE OF VACCINE THERAPY.

By A. Butler Harris, M.B.

I have considered it advisable, in discussing the rôle of vaccine therapy in this disease, to put before you the more recent work on the ætiology of tuberculous infection in children, particularly as so much stress is laid upon environment, and little enough has been said about infection by means of food. Further, it is not unprofitable to get a clear idea of the part played respectively by the human and the bovine strains of the tubercle bacillus.

I have included a few remarks on the diagnosis of tuberculous joints, because this subject is intimately bound up with the problem of immunization.

Ætiology.

The Royal Commission, in the final report [13] on tuberculosis, has published evidence which goes to prove that a considerable amount of the tuberculosis of childhood is to be ascribed to infection with the

bacilli of the bovine type transmitted to children in meals consisting largely of the milk of the cow. Bovine tubercle bacilli are apt to be abundantly present in milk as sold to the public, when there is tuberculous disease of the udder of the cow from which it was obtained. "This fact is generally recognized though not adequately guarded against. But these bacilli may also be present in the milk of tuberculous cows presenting no evidence whatever of disease of the udder, even when examined post mortem. Further, the milk of tuberculous cows not containing bacilli as it leaves the udder may, and often does, become infected with the fæces or uterine discharges of such diseased animal."

A striking experiment by Dr. Stanley Griffith, published by him in the appendix to the final report [14] of this Commission, corroborates these observations. He injected a non-tuberculous cow, which was giving milk innocuous to the guinea-pig, with 100 mg. of a culture of a tubercle bacillus of bovine strain. At the end of the first week after inoculation guinea-pigs fed on the milk of this cow developed tuberculosis. The cow died at the end of thirty days of acute generalized tuberculosis. The udder was normal and showed nothing of the nature of a tuberculous lesion either to the naked eye or on microscopical examination. He sums up the situation in the following words: "There arises, therefore, strong presumption that the milk of naturally tuberculous cows without udder tuberculosis will contain tubercle bacilli whenever in the course of the disease tubercle bacilli circulate in the blood-stream."

My own observations in general practice tend to throw grave suspicion upon milk as a source of infection. Tuberculous adenitis, the most usual tuberculous infection in children, is very rife amongst the children of the upper classes, more especially children who have been reared from infancy upon special cow's milk to which cream has been added, with a view to their more perfect feeding. If the milk has been sterilized the cream has not, for fear of scurvy rickets. This cream is derived from separated milk, during which process of separation it has been proved that tubercle bacilli are caught up by the rising cream and taken to the top. This class of infant and child is usually carefully guarded against the probability of infection from known cases of tuberculosis, and it is therefore unlikely that infection has occurred from environment, as may easily happen among the children of the working class. In the latter, Kenwood has pointed out that a tuberculous environment undoubtedly is an important factor in the ætiology of this disease. In this class one would, a priori, expect that the infecting organism would be of human origin.

It should be noted that in the final report of the Commission only four cases of joint tuberculosis are recorded, of which one was that of a girl, aged 14 years, the rest being adults. In all these cases the cultures and inoculation reactions followed those of the human type. Again, I find it recorded that in ten cases of primary alimentary tuberculosis four gave a bovine strain, and six a human. In another place, of twenty-eight cases of alimentary tuberculosis in children under 12 years, in which the material for examination was obtained after death, fourteen were bovine and fourteen human. Lastly, in the second interim report, 1907 [15], of fourteen cases of bovine type ten were primary abdominal, three had cervical glands, and one was that of pulmonary tuberculosis. Of forty cases of the human type of infection, eight were primary abdominal, ten respiratory, nine bone and joint, two genito-urinary, and eleven cervical glands. The evidence is, then, that in children infection of a bovine strain usually finds its way in through the alimentary canal. In a series of twenty cases of mesenteric gland tuberculosis detailed in the second interim report, eleven yielded cultures of the bovine strain. The cases recorded in the final report furnish additional evidence to that brought forward in the second report, that the tubercle bacillus of bovine origin can, when ingested, produce severe tuberculosis in children, and cause death through extensive local changes or generalization of the disease. The milk, however, from nursing mothers who were phthisical did not produce tuberculosis in guinea-pigs.

As has been stated above, the majority of infections with the tubercle bacillus in children come through the alimentary canal. Hence the inevitable conclusion that in bone disease the infection is more frequently bovine in origin. Still, it must also be admitted that a very considerable number of bacilli found in the joints of children give the human reactions. As far as we know at present, it does not appear to matter very much clinically as regards joint infections whether the strain is of bovine or human origin. The same tuberculin appears to do equally well in every case, and the opsonic indices come out equally whether one or the other strain is used. It does, however, matter in respect to the general hygiene of the young child. The surroundings must be as free from the tubercle bacillus as the food.

With regard to the evidence that milk is a potential source of danger, unless it is sterilized, it has been shown that the report of the Tuberculosis Commission affords ample evidence, and Koch's assumption may be considered to be conclusively refuted. It is interesting to note in this connexion that in countries where unboiled milk is drunk, and the

cows are kept in an artificial condition, as in this country, tuberculosis is rife. Brush has shown [1] that in Spain tuberculosis is a scourge, but across the Mediterranean, in Morocco, it is unknown, notwithstanding the fact that the hygienic surroundings of the Moors are, if anything, worse than those of the Spaniards. Again, in China, where no milk is drunk, there is little tuberculosis, but amongst the Tartars, who are a far finer race, it is very common; the latter, however, drink large quantities of milk. In South America, where all the milk is boiled on account of the hot climate, there is little tuberculosis; in North America it is a perfect scourge, so much so, that of recent years the Americans have awakened to the fact and have instituted milk depots.

Shennan [8] says: "Speaking generally, the great danger to be apprehended is from dairy cattle, which are kept to a large extent in ill-ventilated byres and cowsheds, rather than from cattle kept in the open; the former being herded together under conditions favouring the transmission of disease from one to another, and its retention in a fairly constant ratio. . . . As great a proportion as 50 to 70 per cent. of cattle kept under such conditions has been found affected with tuberculosis by the tuberculin test, and the udders are found tuberculous in ½ to 2 per cent."

Clifford Allbutt, on the other hand, fed not only his family but also that of a neighbour on the milk of a cow which was exceptionally tuberculous, for some considerable period, without anybody being the worse for the experiment. Isolated evidence of this character, which is purely negative, is of infinitely less value than that which tends to establish cause and effect.

There is ample evidence to show that tuberculous adenitis is often the precursor of a similar infection of the bones and joints. But whereas the former infection is extremely common, the latter occurs in comparatively few cases. The degree of adenitis is no guide as to whether the child may afterwards be infected as to its joints, if we except those massive infections with the tubercle bacillus which ravage the tissues simultaneously and indiscriminately.

Again, a child may be infected with adenitis to such an extent that operation may be necessary; he may make an apparent perfect recovery, and in two or three years may fall a victim to joint infection. Considering the frequency of tuberculous adenitis, and the infrequency with which other tuberculous infections follow, may we not suggest that the glands being the first defences of the body, do by auto-inoculations of a

favourable intensity afford a natural protection against further ravages of this organism by the natural and regular production of antibodies? The use of tuberculin in adenitis is such that in my own experience, and that of those who have used it regularly, the necessity of operative interference has been reduced to what may almost be called a minimum.

Diagnosis.

The diagnosis of a tuberculous joint in the earliest stages is often a difficult matter for the surgeon. Radiography has taken its place in the diagnostic outfit, telling us of the extent, if any, of the lesion; but it does not show, except indirectly, what is the nature of the infecting organism. It would appear that purely clinical diagnosis is liable to error, even in competent hands. Ely [3], of Denver City, has recently published the pathological findings of specimens from sixty-two joints, which had been sent him. These had all been operated on for tuberculous disease, but microscopical examination showed that fifteen were not tuberculous. I have had no means of ascertaining the percentage of error that occurs in this country, but to me it is inconceivable that it should be as high as 25 per cent. The fact that error is possible suggests at once that some additional weapon for diagnostic purposes is necessary. In tuberculous infection this is not a difficult matter. Various bacteriological methods have been exploited, and although efficient in themselves, some grave drawback has presented itself. The use of massive injections of old tuberculin has been abandoned, leaving us with Calmette's ophthalmo-tuberculin and von Pirquet's tuberculo-cutaneous reactions, and the reading of the records furnished by a series of opsonic indices. Calmette's reaction is out of favour with many, and von Pirquet's reaction, although according to Vallow (1911) [9] it is reliable as to the diagnosis of tuberculosis in the human subject, cannot differentiate the site of any particular tuberculous lesion. It cannot do more than indicate that the subject of the test is tuberculous, but it does not afford direct evidence that the lesion is situated in a particular joint.

The estimation of a series of tuberculo-opsonic indices in the hands of a skilled worker undoubtedly affords the necessary data as to joint infection. A simple routine is required. The limb is kept surgically at rest for a day or two before a sample of blood is taken. The joint is then moved, or the limb massaged, or a Bier's bandage applied, and a couple of hours afterwards a second sample of blood is taken. The limb

is once more returned to rest, and a few hours later a third blood sample is obtained. As the child is practically at rest throughout the whole time the samples are being taken, there is little fear that the products from other lesions, if any exist, will exercise a disturbing influence upon the curve of immunity.

The result of movement or massage upon the joint, if a tuberculous lesion is present, is to liberate substances into the blood-stream, which will have the effect of producing a fall or negative phase of the opsonic content in the blood. Later, the last test should show that a rise of the opsonic index has taken place to a higher level probably than existed during the initial stage of rest. In tuberculous joints the excursions of the index are no mere fanciful variations, but present, usually, a steep fall and rise. When these are not present, it may be concluded that the lesion, if there is one, is not tuberculous, but is due to some other micro-organism.

Maynard-Smith (1909), [6] has recorded, from the point of view of the surgeon, the results of cases treated in the Inoculation Department of St. Mary's Hospital for the previous three years. In this report he gives a table of eleven cases illustrating the employment of auto-inoculation tests in diagnosis. The procedure resorted to with a view to induce auto-inoculation was either massage or the application of a Bier's bandage for a period varying from a half to one hour. The fall in the opsonic index usually began about two to three hours after; the time of the after-rise was variable. In only one case did the result give doubtful evidence; one case, previously diagnosed as tuberculous, gave a negative tuberculo-opsonic result, but produced a variation with the gonococcus. The history subsequently confirmed this as correct. Maynard-Smith quotes another case, diagnosed as tuberculous, where the index to tubercle was normal, which subsequently proved to be one of simple arthritis.

My own experience in the use of the tuberculo-opsonic index as a diagnostic agent accords with the results obtained at St. Mary's Hospital. It is not possible to rely on a chance index when a decision is wanted; there must be evidence that definite auto-inoculation has occurred. The following case is a good example: In November, 1910, a child came under notice with eversion of the foot, limitation of movement of the hip-joint, pain on abduction and on tapping over the trochanter, lameness on walking, pain in the back, and some spinal curvature. Three indices were made under the conditions quoted above; these were 1·0, 0·92 and 1·23, showing but little variation. Radiographs

were taken, which showed nothing abnormal in the joint. The child had been under observation for the past year, but has not developed tuberculosis.

Sir A. E. Wright [11], in conjunction with Staff-Surgeon S. T. Reid, formulated data with regard to the tuberculo-opsonic power in cases of strictly localized tuberculosis, into which category tuberculous arthritis usually falls. They find that the opsonic index is low, and uniformly low—in exceptional cases as low as one-sixth of the normal. In the same communication they show that fluid drawn from a tuberculous focus will give an opsonic value many times less than that of the patient's blood. Thus if fluid from a tuberculous joint be aspirated, and its opsonic index taken, it will be found to be practically *nil.*

Sir A. E. Wright [12] and his fellow-workers, S. R. Douglas, John Freeman, the late J. H. Wells, and Alexander Fleming, published an important communication (1907) dealing with the phenomena which occur in the production of auto-inoculation. The study of the charts of the opsonic indices given under these conditions, particularly with reference to tuberculous arthritis, clearly proves the value of the opsonic index as a diagnostic agent. Exercise, massage, Bier's passive congestion, hot fomentations on the joint, all yield the same curves—an initial fall (the negative phase), followed by a rise of the opsonic index. If rest is enforced, a cessation of the fluctuations of the index ceases, denoting the stopping of auto-inoculations.

The presence of other organisms in addition to the tubercle bacillus, such as the staphylococcus, can equally well be demonstrated by the opsonic method.

If fluid can be obtained from the joint in sufficient quantity, it may be aspirated and inoculated into guinea-pigs for diagnostic purposes. This method, although an absolutely positive one, is not always available, as it necessitates the investigation being conducted in an authorized laboratory.

The objections to the use of the opsonic index are, that it is a very laborious method, and, except in the hands of one constantly practising the technique, is liable to error. I am aware that various workers have condemned the findings of the opsonic index as being unreliable, and have stated that the limits of error are as great as the variations which are claimed to be produced by disease. Statisticians, to mention Karl Pearson (1910) [7] and Greenwood (1909) [4], have examined the records of laboratories where the opsonic technique is as perfect as it humanly can be made, and they have calculated that the error is

such as to invalidate the employment of this method as a gauge of the variation of the immunizing content. On the other hand, those who have used this for purposes of diagnosis, as well as for regulating the inoculations, have for the most part not yet, even after several years of careful testing, abandoned it. It is now seven years since Wright began to use the opsonic index extensively, and although it may now have been given up largely during the period of routine treatment (simply because those who employ inoculation are more conversant with the clinical manifestations), it still remains a most valuable aid to diagnosis. Especially is this the case in the disease under discussion, because the auto-inoculations can be produced or controlled at will.

Treatment.

The general principles of treatment have already been dealt with by the previous speaker. It remains to discuss:—

(1) Methods which will produce auto-inoculations.

(2) The administration of tuberculin.

(1) Treatment by auto-inoculation in the case of joint infection possesses the insuperable difficulty that the fundamental principle of keeping the joint absolutely at rest is violated, except in the method of passive congestion associated with the name of Bier. Even under the latter conditions the dose introduced into the system cannot be regulated, and, as has been shown in the remarks dealing with diagnosis, a steep negative phase may be produced. The use of Bier's method is naturally confined to such joints as are somewhat remote from the trunk, and, further, it is easy to realize that the distal joints are less liable to pour an excess of inoculating material into the body than more proximal ones. It is interesting, therefore, to note that Ely [2], who has had a very wide experience with this form of treatment, asserts that ankles and wrists do better than knees and elbows, whilst for hips, shoulders and spines the method is not to be commended.

The only advantage that can be claimed for any method of auto-inoculation in the disease before us is that an autogenous vaccine is introduced. This, however, appears to be of little moment, as I cannot find any evidence that one strain of tuberculin is better than another, when employed for therapeutic inoculation.

(2) The administration of tuberculin. This section of the subject may perhaps be most conveniently presented to you for the after-discussion, if I proceed to elaborate the problems that arise from it.

Problem I.—Having ascertained that the joint or joints are tuberculous, under what conditions should tuberculin be used?

I have reminded you previously that movement of the joint produces auto-inoculations, with variations of the tuberculo-opsonic index, and also that the index of a patient at rest, suffering from a localized tuberculous lesion, is invariably low. Lastly, the fluid or pus removed from a joint contains practically no immunizing material. Accepting these statements as facts beyond dispute, it follows that vaccine therapy cannot be in any way antagonistic to, or supersede, the simpler forms of surgical treatment, such as rest by extension, aspiration where there is excess of fluid, and consequent pressure hindering and preventing free circulation in the tissues of the joint.

Tuberculin inoculations aim rather at raising the general resistance of the body to a normal level, and thereby incidentally giving the tissues in the immediate neighbourhood of the tuberculous lesion a better chance than they otherwise would have of destroying the infecting organism and effecting a repair. Nature aims at this by tending to immobilize the joint; and the surgeon aids Nature by making that immobility complete, by restoring circulation through the removal of pressure, and in more advanced states by excision and drainage. Artificial immunization can also aid Nature when the surgeon has played his part.

We may take it, therefore, that tuberculin should not be injected until all chance of auto-inoculation, either by natural movement, massage, or operative interference, has been eliminated. It is a fundamental principle in vaccine therapy that delibérate artificial immunization must not coincide with chance auto-inoculation; otherwise, as has been repeatedly shown, two inoculations may occur close upon one another, resulting in the disastrous production of a severe negative phase. Disastrous, because during the negative phase the invading organism has opportunity for further multiplication and advance.

The reason why rest is so all-important is that during exercise repeated auto-inoculations are occurring, with the result that an intensive and prolonged negative phase is produced.

These are the premises in vaccine therapy which seem to fit in with the ordinary procedure in clinical surgery.

In tuberculous joints there occurs at times the formation of abscess under pressure from within. It is obvious that immunizing substances from the blood will not reach the infected area, if the pressure in the

cavity is higher than that of the blood. Consequently, vaccine therapy is of no value as regards the lesion until this pressure has been removed by drainage, and, if necessary, erasion or excision. Lastly, if the abscess fluid is found to contain other organisms than the tubercle bacillus, it may be worth while to employ other vaccines beside tuberculin. This will depend upon the estimation of the patient's resistance to these secondary organisms.

When possible, at the outset of treatment one proceeds with the estimation of a series of opsonic indices taken under conditions of complete rest. Unless these show a marked variation, or are abnormally high, a preliminary dose of $\frac{1}{20000}$ mg. to $\frac{1}{40000}$ mg. of tuberculin is injected subcutaneously. It seldom happens that any marked reaction follows so minute a dose. Yet it is usually sufficient to raise the opsonic content to an appreciable extent without causing a negative phase of any importance clinically. It has been found that the injections may, with advantage, be repeated about once a week, as the immunization curve, heightened by the preceding inoculation, begins to fall again. There is no apparent object in increasing the dose, unless it appears that the response to the injection is insufficient for material progress. The larger quantities used five or six years ago in this country, though small in comparison with those of Continental workers, have for the most part been abandoned, as not only unnecessary but even harmful. It is interesting to note here, that the introduction of these minimal doses was a direct result of the tracing out of the opsonic curve day by day during the course of treatment, and not of purely clinical observation.

Problem II.—Can tuberculin be given safely without recourse to the opsonic index?

Yes. There is no doubt that it is, nowadays, far more possible, in the light of the information acquired from the data already furnished by the opsonic index, to employ tuberculin safely and successfully as a routine practice. The very diminution in the size of the dose, and the greater limits of time adopted, both make for increased safety in administration. I will quote some remarks from a letter from Dr. Eyre to me bearing on this point. In Guy's Hospital, "in almost every instance of treatment of tuberculous arthritis, Koch's tuberculin T.R. has been used. Exact figures, as in the case of the other statistics, cannot be given. . . . The interval of inoculations varies between eight or nine days to seventeen days, and the injections are given on general clinical

grounds, although in 1905-06 the value of those clinical grounds was controlled by observations of the opsonic index. Even now we frequently use the opsonic index to determine whether or no an initial dose is a suitable one, and how soon it should be repeated."

Mr. A. Richardson, Resident Surgical Officer, the General Infirmary, Leeds, kindly informs me as follows: "We do not use the opsonic index in our treatment with tuberculin. We use the B.E. tuberculin. We do not differentiate between human and bovine strains. We only find good done in early cases. The dose starts at $\frac{1}{5000}$ mg. and reaches $\frac{1}{1500}$ mg. The injection is given every ten days. We do not use the index for determining when the limb may be used."

Dr. Loveday, of the Manchester Royal Infirmary, tells me that he uses the opsonic index for diagnostic purposes, and also to guide the dosage at the commencement of treatment. "Having established a suitable dose by the index, I continue treatment now without it, except in special cases or as occasion may arise. I formerly used T.R., but now B.E., both of human type. Inoculations are generally given every fortnight. The dose varies from about $\frac{1}{50000}$ mg. to $\frac{1}{5000}$ mg."

It appears from the correspondence which has reached me in connexion with this subject, that the above represent the course followed in hospitals where there are facilities for carrying out the work, but the majority of surgeons do not avail themselves of this method of control, and a greater number still do not use tuberculin at all.

Problem III.—What is the proper form of tuberculin to use, seeing that infection may be of either bovine or human origin?

In discussing the ætiology of this disease I produced evidence to show that the infection might be either of bovine or of human origin. From the early days of therapeutic immunization it has been usually the practice to use Koch's T.R. of human origin. This custom has apparently continued in the centres where tuberculin is most employed —namely, at the London Hospital, at Guy's, and at St. Mary's.

Bovine has been used, but I am informed that, on the whole, these children do better on the human strain of T.R. Eyre tells me in this connexion that he always uses the latter. "We do not," he says, "differentiate between human and bovine strains in treatment. The only time we make use of bovine tuberculin is when a case is complicated by the existence of tuberculosis of the lungs. Such cases we

find usually stand injections of bovine tuberculin better than human tuberculin."

Nathan Raw's views are, I believe, peculiar to himself. He considers that the two strains of bacilli are antagonistic to each other, and that infection with one variety protects against the other. I do not myself presume to criticize his statements or his results, except to remark that they do not coincide with the findings of the workers on the Royal Commission.

I have used Koch's T.R. for several years, and more recently the B.E., both of human origin. For a short time I used Béraneck's preparation, and abandoned it for Denys's method. But it was difficult to arrive at any precision of treatment with either of these substitutes. I now use the bacillary emulsion, as it conforms more closely to the ordinary type of a standardized emulsion of the dead microbe.

Problem IV.—What are the therapeutic effects of tuberculin in this disease: (a) *As regards the general condition of the patient?* (b) *As regards the local condition in lessening the time necessary for complete rest?* (c) *Diminishing the necessity for operation by stopping the progress of the disease in the early stages?*

I have found it advisable to split up the consideration of the therapeutic effect of tuberculin under these headings in order that we may more easily discuss the several factors concerned, and also because of the absence of any statistics definite enough to guide us at the present time to a simple conclusion as to its value. The first question is easy to answer, particularly if we inquire as to what happens in a far commoner tuberculous infection—namely, tuberculous adenitis.

In both these conditions children improve as to their general health. Their appetites and tempers become more like those of normal children; the dry, powdery, ill-nourished skin gets supple and moist; they increase in weight, and the glands diminish, unless they are already bags of caseous material. Children suffering from adenitis amongst the poorer classes usually come to the hospital as out-patients, so that it is to the tuberculin and not to a change of environment, as in the case of in-patients, that the improvement is due. In the case of tuberculous arthritis, the children are necessarily admitted as in-patients, so that this additional factor of improved environment is introduced.

Dr. Gordon Pugh, who is in charge of Queen Mary's Hospital for

Children at Carshalton, says in reference to this point: "There is some difficulty in estimating the value of any special treatment, for the majority of our patients come from poor surroundings, but the fresh air and good food alone work great changes in the patients." I may mention that tuberculin is used at this hospital, but he remarks: "The experience in the use of tuberculin here is not sufficiently extensive to allow me to arrive at any conclusion as to its efficacy."

Of direct negative evidence I have come across but little. Mr. Seymour Barling, of the Birmingham General Hospital, writes as follows: "The reason the administration of tuberculin was given up by myself personally, was that I failed to see that it produced improvement other than that which could be produced by ordinary palliative measures. . . . Provided that one can get the cases early, and have reasonably good conditions for carrying out palliative treatment, the results of joint tuberculosis in children so treated are on the whole exceedingly good." It is only fair to say that the doses used were $\frac{1}{500}$ mg. to $\frac{1}{1000}$ mg. of tuberculin. It may have been that these excessive amounts had something to do with Mr. Barling's negative results.

Loveday and Ramsbottom (1908) [5] on the other hand, find that in a series of twenty-two cases of tuberculous adenitis treated by tuberculin in much smaller quantities ($\frac{1}{50000}$ mg. to $\frac{1}{5000}$ mg. T.R.), ten derived considerable improvement, "in that there was great improvement in the patient's general health, with complete disappearance or marked reduction in the size of the glands." . . . . . "Of seven cases of tuberculous bones and joints (six joints and one bone), six showed considerable improvement." The evidence here is not direct, as the authors do not state the ages of the patients.

Dr. Moore, of the Royal Sea Bathing Hospital, Margate, writes as follows in answer to my queries: "The opsonic index was used, but chiefly for showing positive and negative phases, and at the commencement of treatment. We use human tuberculin now; we have used bovine. I consider that tuberculin does not shorten the time. I cannot say that I have seen any one case which might be said to owe improvement to the use of tuberculin. If improvement is present, it is due to rest, feeding, &c. I have seen a considerable number of cases, and have often seen cases do badly with it. As regards the dose, we start with $\frac{1}{2000000}$ mg., or $\frac{1}{1500000}$ mg. or $\frac{1}{1000000}$ mg., push up to $\frac{1}{500}$ mg., and watch for any reaction. We do not control the doses by the opsonic index, as we do not find it reliable. We do not use the opsonic index

for determining the time when the limb may be used. As regards the statistics, I have not got them, but the number is relatively small."

Dr. F. W. Andrewes informs me that at St. Bartholomew's Hospital the surgeons employ tuberculin at their own discretion, but he does not think that any of them are now treating tuberculous arthritis with it. They use tuberculin at times for diagnostic purposes. Opsonic methods are not employed as a routine. Dr. Topley, of Charing Cross Hospital, cannot find any records of tuberculin having been employed at this Hospital.

Dr. Archibald Campbell, of the Royal Infirmary, Glasgow, answers my inquiries as follows: "The opsonic index is now only occasionally used for diagnostic purposes, because of the number of estimations that have to be made on a given case to make it reliable. We use T.R., T.B.E. tuberculo-sero-vaccine S.B.E. of Meyer, both human and bovine strains, and also Béraneck's tuberculin. We not infrequently mix bovine and human. Some of our surgeons stipulate bovine for one case, human for the other. We do not use the opsonic index now; we used to, but the work became so heavy that we had to give it up. As to whether the use of tuberculin cuts short the necessary time for surgical rest, it is purely a question for the surgeon. The doses of tuberculin supplied to the wards vary from $\frac{1}{10000000}$ mg. to $\frac{1}{2000}$ mg. of tuberculin."

I think, without saying more, it is generally agreed by those who have worked systematically by minimal doses with tuberculin, that improvement in the general health in cases of localized tuberculosis usually does take place, and that the small particular evidence available does not contradict this observation in the cases under discussion.

I come now to the goal to which my footsteps have been somewhat tardily approaching. I say this advisedly, because if I were able to bring before you an overwhelming array of statistics either for or against the use of tuberculin in this affection, it would not have been necessary for me to have stopped to examine so many points in my argument. It appears from investigation that there are not many who have done much work in this matter; what has been done is at present small in amount; a little of it has been tabulated, the major part not at all; and, on the other hand, there appears to be a prejudice held by many surgeons against the use of tuberculin at all. Such prejudice, I think, is based for the most part partly on insufficient data, and partly on the adverse results obtained by imperfect methods of use. In institutions, where the work has been systematically carried out through the help of a laboratory, the use of tuberculin has increased,

and the results obtained are, to say the least, encouraging. It may be argued on the other hand that the laboratory has to justify its existence, and that therefore too favourable a complexion is put upon the results, at any rate as far as tuberculin is concerned. But I am going to give you a few statements from those who have availed themselves of the services of the laboratory, which go to show that there is some benefit from the use of vaccine therapy in tuberculous arthritis.

The first paper in which there is any attempt to deal thoroughly with this question was published by Maynard-Smith (1909) [6]. Nineteen of his cases were children of 16 years and under. They came under treatment between 1906 and 1908, and the majority had tuberculin for at least a year. Of these, fifteen had been under treatment with rest and splints, but were not getting better. They either had sinuses, which would not heal, or painful swellings. Four had been operated on previously. In 1909, sixteen of these nineteen children were getting about, and in every case, though in some there was some limitation of movement, it appeared that the foci were healed.

These figures seem to show that not only was the time necessary for rest cut short, but also that there was distinct evidence to prove that tuberculin had produced a positive result, when other measures had failed.

Western (1907) [10], in a paper on the results obtained in the Inoculation Department at the London Hospital, says: "We have had under treatment seventeen cases of tuberculous disease of joints and synovial sheaths. Of these, six patients ceased to attend before treatment had been in progress for one month. Of the remaining eleven cases, seven are either completely cured or show very great improvement, two show no improvement, and two show slight improvement. The two cases showing no improvement are both cases of senile tuberculosis in patients over 60 years of age. The ordinary treatment with splints or plaster has been carried out in conjunction with inoculations, and in a few cases Bier's method of passive congestion has been added, with apparent benefit. Inoculation treatment of joints and tendon sheaths is, therefore, in our experience, attended by good results, especially in young subjects and in early cases, and given such conditions a cure and good movement may be expected. Where there is much destruction of tissue and sinuses are present, progress is slow and movement will probably be considerably limited." Two illustrative cases are quoted: A boy, aged 6 years, was under treatment with a splint for the knee for two years without any improve-

ment. His temperature was 98° F. to 102° F. Inoculations were begun on April 4. On May 19 the pain had ceased; temperature was normal. In December the synovial membrane was still slightly thickened. There was free movement of the joint to a right angle. There was great improvement in general health. The second case was still more striking. A girl, aged 15 years, had worn a Thomas's splint for tuberculous arthritis of the knee since 1899. The condition in November, 1906, as noted by the surgeon in charge, was swelling of the knee, thickening of the synovial membrane, and some pain in the joint. Inoculations began on November 9. "Now, May, 1907, the knee appears normal. There is no pain or local swelling. Movement can be carried out within 30 degrees. The splint has been discontinued." This is striking evidence that tuberculin is a most active factor in stopping the infective process, particularly in the synovial membrane.

Dr. Western, of the London Hospital, answers my circular letter as follows: "Thirty-seven children under 14 years of age have been treated with tuberculin. The opsonic index has been used in doubtful cases for diagnosis. I consider this method is reliable for this purpose. We have used T.R., B.E., and Perlsucht B.E. We do not differentiate between human and bovine infections in treatment. Bovine has been used in some cases, but as the strains have not been isolated from each case in order to differentiate treatment, I cannot answer in definite figures as to whether tuberculin has any effect in shortening the time during which the joint has to be kept surgically at rest. The dose of tuberculin varies from $\frac{1}{10000}$ mg. to $\frac{1}{500}$ mg. or larger. We used to control the doses by the opsonic index, but do not do so now. We sometimes use the opsonic index for determining the time when the limb may be used."

I had hoped, when I had embarked upon this inquiry, to have obtained from the hospitals where such work is being carried out statistics which might have been—and which certainly ought to be—produced. The correspondence, however, has brought out:—

(1) That in many hospitals no tuberculin treatment is used.

(2) That often, where employed, it has not been carried out systematically and no special records have been kept.

(3) That where the work has been carefully followed up statistics are not available at present, but the results are definitely in favour of using tuberculin.

(4) The number of cases of tuberculous arthritis in children which

are treated in the general hospitals are not many, compared with those that find their way to the children's hospitals. In the latter there is seldom the staff necessary to carry out this work in its entirety, so that here, again, one has suffered disappointment.

Dr. Eyre, of Guy's Hospital, is unable to give exact figures as to the duration of treatment, but he has a distinct impression that the period of surgical rest is shortened very considerably. "I have," he writes, "one particular case in mind where, after a period of two years' surgical rest and a plaster splint, fluid from the joint (a knee) contained virulent tubercle bacilli, and the X-rays showed a considerable amount of active disease. After six months' treatment with tuberculin the limb was again in use. Usually, of course, surgical rest is maintained for a much longer period than this."

My own experience has been derived entirely from patients of the better class, and has been necessarily very limited as regards tuberculous arthritis, but wider in respect to adenitis. I have had, however, this advantage, that the cases have come under notice early, whilst the infection was purely tuberculous and confined to the joint. Further, the environment of the children has not been changed as regards food, light and air, so that one has had no disturbing factors outside the surgical rest and the use of tuberculin to take into consideration.

I have had under my personal observation ten cases; all were in the early stages of the disease, except one which had recurred after an intermission of five years. This one was, as far as the nature of recurrence, an early one. With one exception they were all the children of the better class. The average length of time in which they were kept in splints varied from six months to fifteen. Every case has apparently recovered the free use of the joint, and there have as yet been no recurrences except the one mentioned above, in which a second recurrence took place at the age of 15 years. A third recurrence occurred in the same hip-joint two years later. This has, however, again rapidly yielded to tuberculin without surgical rest being adopted.

Every one of the earlier cases was tested repeatedly for the opsonic index; and it is only during the last two and a half years that this has been abandoned as a routine practice, because it was felt that one's clinical experience had, from the knowledge acquired from many observations of the index, become sufficiently formulated to enable one to run, as it were, without these leading strings.

My impression is that these cases benefited by the tuberculin; indeed, it not infrequently happened that the child flagged if it was

discontinued, while at the same time the index usually dropped. After cure had been effected, I mean by that free mobility, without pain, and without fluctuations in the opsonic index, incident upon exercise, I have found it necessary in several cases to continue the tuberculin at intervals of from a month to six weeks, particularly in the winter months. This was frequently the outcome of observations on the part of the mother. The child would flag, lose appetite, and if an index were taken it was often in the neighbourhood of 0·6; tuberculin under such conditions seemed to act as a tonic. In the case of adenitis this is, in my experience, the usual clinical occurrence, so that I do not think it is the hazy surmise of a general practitioner, but an important clinical fact.

One other point, which I think is important to bring to your notice. In two cases tuberculin did not seem to have any beneficial effect until the children were sent to the seaside. In the first case in which this happened there was no response in the opsonic index when at home. The child was removed to Westcliff; but that change alone did not seem to be of much use. Directly, however, tuberculin was started again, the index rose with a concurrent and general improvement. This child to-day, four years later, is well and strong, and the hip-joint shows no limitation of movement, and to the X-ray appears to have entirely healed, as far as the initial lesion in the bone is concerned.

These numbers are too small to base any definite conclusions on, but I venture to think that the results have been remarkably even and that the period of healing has been less than usually obtains even amongst the children of the upper classes. Then, again, recurrence has been remarkable for its absence. One certainly used to expect in a weakly child, with adenitis preceding the joint infection, that even when the latter was apparently cured there was not infrequently a recurrence within a couple of years. Operative measures have not been found necessary; whereas, formerly, I am sure that erasion at the least would have been performed in some of the cases. I am not aware that of recent years, since the advent of tuberculin, any more care, surgically speaking, has been taken by myself; the only additional feature besides tuberculin has been the employment of X-ray pictures before and towards the end of treatment.

Broadly speaking, it is evident that children have a great recuperative faculty in respect to tuberculous arthritis, and it would appear that this is stimulated and called into action by the judicious use of tuberculin.

Problem V.—When may treatment be stopped, that is, when can it be said that the joint is cured?

Clinical evidence, coupled with that of an X-ray picture, is not absolutely reliable. Add to these a normal opsonic index for a month or two, and the position is very much strengthened. Can it be said, however, that a joint once affected with tubercle is ever absolutely cured?

Exercise must be very gradual and the estimations of the opsonic index during this stage are of the utmost value. The opsonic index will vary at once if exercise is producing any auto-inoculations. If this occurs, rest again is imperative. Indeed, if it were not possible to use indices at any other stage it is imperative to employ them at this point. Maynard-Smith [6] raises the question as to when a cure has been accomplished, and suggests that something besides clinical methods is necessary. He is hinting at the employment of the opsonic index to detect auto-inoculations as described in the earlier part of his paper, when discussing the diagnosis of tuberculous joints.

Future Care and Prognosis.

Problem VI.—When may supervision be abandoned?

It is impossible to state whether tuberculin tends to prevent recurrence if only given during the active and convalescent stages of an attack. It is tempting to suggest prophylactic inoculations (with periodic estimations of the opsonic index) over a period of several years until adult life has been reached.

As to the question of cure, no one can say definitely when absolute cure has been effected. The microscopical appearances may show nothing, and even the index may remain normal for a long while, and yet the disease may recrudesce in the same joint months, even years, after. I remember being shown a photograph of a lad at school which had been taken some eight years before, and showed him in the fancy dress of a hussar, and one leg in irons. His relatives could tell me nothing very definite as to why he wore irons or how long he wore them. This boy went afterwards to the University. He worked and rowed for his college. He noticed towards the end of his time at Cambridge that he got stiff in the hip-joint, but did not pay much attention to it. He came down and saw me for general seediness. I noticed that he stood with his left foot slightly everted. Examination suggested a slight

want of mobility in the joint. He saw a surgeon, and was X-rayed. He was put on extension, and during the next few weeks opsonic indices were made, which also were condemnatory. The recrudescence had not occurred for twelve years, and no doubt was brought on by excessive work and exercise. He recovered, apparently, and was able to read for and pass a professional examination. He obtained work in his profession and after two months of this he complained of pain and stiffness again in the same hip. An X-ray showed that the acetabulum had now become infected, although the former slight ulceration on the edge of the articular surface of the head of the femur had not broken out again. He stopped work, had several months' extension of the limb, combined with tuberculin, and when everything had apparently healed went to South Africa to a relative. There he lived an out-of-door life away from his books, and returned home a year and a half afterwards. He began reading at once for his final examination, and within a month was again under observation. His indices were again variable, and there is very little doubt that again he had a recrudescence of his old trouble. The attacks were evidently never very acute at any time, but still sufficient to spoil his life for the time being. In this instance tuberculin was given in the last three attacks, and with very marked advantage both locally and generally, but it did not exterminate the tubercle bacillus from his left hip-joint.

Conclusions.

(1) The infection is often of bovine origin, and occurs through the alimentary canal, the infecting food being usually milk.

(2) Among the working class, environment, as well as food, is an all-important factor.

(3) The occurrence of auto-inoculations, as evidenced by the fluctuations of the opsonic index, is a valuable guide not only to diagnosis but also to the determination of whether or no a cure has been effected.

(4) Apart from the preceding conclusion it is not necessary to employ the opsonic index during the whole course of treatment, provided that the case, clinically, is pursuing a favourable course.

(5) Tuberculin, given in small doses, under conditions of surgical rest, is a remedy which tends to accelerate the rate of recovery.

(6) Children have a naturally recuperative power, which apparently is called out by the use of a bacterial vaccine in certain cases which do not automatically respond to surgical rest, and improved environment.

TABLE GIVING SUMMARY OF INFORMATION SUPPLIED FROM THIRTEEN HOSPITALS IN RESPONSE TO A CIRCULAR LETTER FROM THE WRITER, DECEMBER, 1911.

Hospital and name of informant	Number of cases treated	Is opsonic index used for diagnostic, &c., purposes?	Strains of tuberculin	Effect of tuberculin	Index used for determination of cure	Dose of tuberculin
Guy's Hospital, Dr. Eyre	No statistics	In doubtful cases	T.R.	Distinctly +	Always	$\frac{1}{10000}$ to $\frac{1}{1500}$ mg.
The London Hospital, Dr. Western	Of 117 cases, 37 under 14	Yes, for diagnosis; not for control now; is reliable	T.R., B.E.; Perlsucht B.E.	+, but gives no figures	Sometimes	$\frac{1}{10000}$ to $\frac{1}{500}$ mg. or more
Royal Infirmary, Manchester, Dr. Loveday	No statistics; most cases go to Children's Hospital	For diagnosis; reliable for initial doses	Formerly T.R., now B.E.; both human type	+, but no figures	Not stated	$\frac{1}{50000}$ to $\frac{1}{5000}$ mg.
Royal Infirmary, Glasgow, Dr. Archibald Campbell	No statistics	Now only occasionally, owing to time method takes	T.R., B.E.; tuberculo-sero-vaccine of Meyer, S.B.E. (both human and bovine), Beraneck's tuberculin; frequently mix human and bovine	Not answered; is purely a question for the surgeon	Not stated; evidently not used	$\frac{1}{100000}$ to $\frac{1}{1000}$ mg.
St. Mary's Hospital, Paddington, Dr. Freeman (verbally)	Not made up recently	Yes; not for routine	T.R. usually; human	+	Yes	$\frac{1}{50000}$ to $\frac{1}{5000}$ mg.

Queen Mary's Hospital for Children, Carshalton, Dr. Gordon Pugh	No statistics	No	T.R. ? not extensively used	No definite answer	No	Dose not given
Charing Cross Hospital, Dr. Topley	Tuberculin not used					
Royal Infirmary, Edinburgh, Dr. Scott Craig	Number of cases	treated insufficient	to furnish any data			
General Infirmary, Leeds, Mr. A. Richardson	No statistics	No	B.E.	Only does good in early cases	No	$\frac{1}{3000}$ to $\frac{1}{1500}$ mg.
St. Bartholomew's Hospital, Dr. F. W. Andrewes	No statistics	Opsonic methods not used	Tuberculin used	by surgeons at their own discretion; that any are using it now		does not think
St. Thomas's Hospital	No details	furnished				
Birmingham General Hospital, Mr. Seymour Barling	Speaking generally, tuberculin is		not used by the staff. Mr. Barling being unsatisfactory	himself abandoned		tuberculin as
Margate Royal Sea Bathing Hospital, Dr. H. Moore	No statistics; but numbers are relatively small	Was used for controlling doses; not used now; was not reliable	Human now, have used bovine	Does not shorten the time for surgical rest	No; not reliable	$\frac{1}{2000}$, $\frac{1}{1500}$, $\frac{1}{1000}$, up to $\frac{1}{500}$ mg.

Number of hospitals where tuberculin is regularly used, 8. Number of hospitals where opsonic index is used—(*a*) for diagnosis, 4; (*b*) for control, but not regularly, 3; (*c*) for judging cure, 3. Number of hospitals which have discarded tuberculin, 3. Number of hospitals which have discarded the opsonic index as being unreliable, 1. Number of hospitals which do not use the opsonic index, but use tuberculin, 4. Number of hospitals which have been unable to give any information, 2.

Tuberculous Arthritis and Phthisis.

Is there any relationship between tuberculous bone disease in children and phthisis in after-life? [16]

"As regards phthisis, there is very little evidence that it is ever due to infection with the bovine virus. The cases in which bovine bacilli have been isolated from the cavities of the lung are few, and not free from doubt. On the other hand, there is good reason to think that primary abdominal tuberculosis is not uncommonly of bovine origin. (*Vide* findings of the Royal Commission.) Weber, one of the German Commission, in a later report, states that of eighty-four cases of tuberculosis in children which were exhaustively studied, no fewer than twenty-one were infected by the bovine bacillus; six of these were tuberculosis of the cervical glands, while thirteen were cases of primary abdominal tuberculosis. In the whole series of eighty-four patients there were twenty-one cases of primary abdominal tuberculosis, of which seven gave the human bacillus."

If we hold the theory that the majority of bone cases in children have become infected through the alimentary canal, and if we also bear in mind the fact that a large number of children of the upper classes become infected every year with tuberculous joints and glands, and that these have in very many instances been very carefully protected from any possible contamination with phthisical cases, but have had an abundance of raw milk and cream, it is not unreasonable to argue that a tuberculous bone does not necessarily lead to pulmonary tuberculosis in adult life.

Nathan Raw holds a still stronger position than this, and thereby possibly confuses between cause and effect. He maintains the antagonism between the two strains of tubercle bacilli. We have, however, no other parallel to this in the bacteriological world.

Dunn has shown, from data derived from a number of post-mortem examinations, that over 70 per cent. of the population of these islands have suffered from some tuberculous lesion or other, and that the vast majority of these lesions, amounting to 67 per cent., occurred between the second and the tenth years. What proportion of bone cases were contained in these figures it is not possible to estimate, but, at any rate, seeing the high incidence of pulmonary tuberculosis, it is not improbable that many even of the bone cases subsequently developed phthisis. But it does not necessarily follow that one took causal precedence of the other. It is suggestive rather of a coincidence occurring naturally with high percentages.

REFERENCES.

[1] BRUSH, E. F. *Arch. Ped.*, Philad., 1890, vii, p. 551.
[2] ELY, L. W. *Surg., Gyn., and Obstet.*, Chicago, 1910, x, pp. 63-69.
[3] ELY, L. W. *Ibid.*, pp. 561-572.
[4] GREENWOOD, MAJOR, jun. *Biometrika*, 1909, vii, pp. 505-530.
[5] LOVEDAY and RAMSBOTTOM. *Med. Chron.*, Manchester, 1908, 4th ser., xv, pp. 145, 223.
[6] MAYNARD-SMITH. *Brit. Med. Journ.*, 1909, ii, pp. 1046-49.
[7] PEARSON, KARL. *Biometrika*, July, 1911, viii, pp. 203-224.
[8] SHENNAN, T. *Encycl. Med.*, Edinb., 1902, xii, p. 437.
[9] VALLOW. *Practitioner*, 1911, lxxxvii, pp. 689-98.
[10] WESTERN, G. T. *Lancet*, 1907, ii, p. 1450.
[11] WRIGHT, A. E. "Studies on Immunization," 1909, pp. 150-156.
[12] *Idem. Ibid.*, pp. 376-433; reprinted from *Lancet*, 1907, ii, pp. 1217-36.
[13] Royal Commission on Tuberculosis, Final Report, 1911, vol. ii, part 2.
[14] *Ibid.*, Final Report, 1911, vol. iii.
[15] *Ibid.*, Second Interim Report, 1907.
[16] "Tuberculosis," *Encycl. Med. (Suppl.)*, 1910, xv, p. 447.

DISCUSSION.

Mr. ROBERT JONES (Liverpool) said all present would agree that there had been three most admirable opening papers to the debate, so that but little was left for others to say. He was enchanted with the delightful pathological views which Sir Anthony Bowlby had expressed, and he was in practical accord with the opinions of both the surgical openers. He recognized with them, as he thought all other practical surgeons would, the greatest possible difference between tubercle in the adult and that disease in the child. In the adult one operated almost immediately; one wished to fix the joint as soon as possible, rather than wait for years for a cure. But in children he believed he had only five or six times in his life excised a joint, although on eight or ten occasions he had been compelled to amputate. He agreed with both speakers that tubercle in the early stage nearly always ran a benign course if certain things were recognized. The first was that the affected joint should have absolute rest in correct position; the second was good food; and the third, good country air. For many years he had been associated with hospitals where children were treated not simply indoors during the night and out of doors during the day, but where they had lived out of doors night and day. That had been the case for years at West Kirby, and at Baschurch, where sixty children lived throughout the summer and winter, in spite of snow and sleet, in the open air. His observations led him to say that there was the greatest difference between

the results of cases treated in indoor hospitals and those treated out of doors. He regarded tubercle as a very dangerous disease, indeed, when children were treated in town and in general wards. When they were treated partly out of doors and partly indoors there was great improvement, but the ideal was to have the children sleeping in the open air during the night as well as living in it during the day. When speaking of rest it was well to make quite sure what was meant by it. So often one heard of rest being given when the part affected was not absolutely fixed. Immobilization should be complete, and attention should be paid to the position of the limb. In the case of the wrist it was very important not to allow the wrist to be put on a straight splint; it should be kept in a hyperextended posture, so that the grasp of the hand would be good. Whatever appliance was used for the hip its object should be to keep the thigh in a good position of abduction. And in regard to the shoulder, the arm frequently was so placed that it hugged the chest, with the humerus adducted, as often occurs in the case of the hip. These cases should be so treated that if ankylosis occurred the movements of the shoulder-blade would give a good mobile arm. If there was ankylosis, or one had failed to secure good position, osteotomy of the upper part of the humerus, to fix it by operation so that the shoulder-blade muscles could be utilized, was the best thing. One important point about the fresh-air treatment was that the children became extremely hungry, which was a very good sign. Moreover, they did not catch cold. Plenty of food should be given. With regard to the tests of recovery, he believed the children were often taken out of their splints too soon. Dr. Butler Harris's suggestion to try passive movements first, and then an opsonic test, he regarded as rather dangerous. He (Mr. Jones) believed one could only be sure when recovery had occurred by making an experiment of observation. Supposing an elbow was fixed with the hand close to the chin, and that the case appeared cured, the sling should be dropped a little, and if in a few days the elbow remained fixed in the old position, recovery with firm ankylosis had occurred; but if it dropped and became fixed in the new position, it indicated that recovery had not yet occurred. If, however, the child could move it back to the chin, recovery with movement had occurred, and the range of movement could be steadily increased. If the forearm dropped ten or fifteen degrees and the elbow became stiff, it was what he called a clinically unsound ankylosis, and it would continue to drop until it reached a useless angle, and become ankylosed there. This was true also in regard to the hip and other joints. One should notice

whether there was an increase in the angle of deformity, or a decrease in the angle of use. These were two sound tests. Sir Anthony Bowlby mentioned what he had himself observed over and over again—namely, the rarity with which one found death from tubercle except when complicated by sepsis. His practice was never to open an abscess until it came to the surface, and then only by a very small puncture, and to see that the teeth in every case were clean. After trying various methods, his conclusion now was that it was better to leave a tuberculous abscess severely alone. In a very large proportion of the cases the abscess disappeared. In the hip-joint an abscess often started from the hip and travelled slowly under the fascia, and became shut off from the joint; an incision could then be made into the nomadic abscess, which was clear away from the joint.

Sir W. WATSON CHEYNE, C.B., F.R.S., said that there was not much for him to criticize, as his views had been, on the whole, very well expressed by preceding speakers. There was no doubt that when it was first recognized that these affections of the joints were tuberculous in nature the impulse was to operate, and that quite an unnecessary amount of operation was done in the early days. As time had gone on the operative treatment of tuberculosis in children had become less and less, till at the present time he very seldom operated on tuberculous joints in a child. There might be several reasons for this change in practice. In the first place, one learned by experience when it was or was not necessary to operate, but he could not help coming to the conclusion that tuberculous disease of joints was of a milder type at the present time than it was formerly. This might be due to one of two reasons, or to both combined. In the first place, it might be that the bacilli were less virulent, for it was known that diseases gradually died out, apparently by the reduction in virulence of the organism. On the other hand, at the present time we lived under very much better hygienic conditions, and it might be that this had improved the resisting power of the body to such an extent that the tubercle bacilli could not spread so readily as in persons who were badly nourished or lived under bad hygienic conditions. As a matter of fact, the best results were obtained in the children of people who could afford to send them away from town and keep them in good hygienic surroundings. He did not think, however, that operation for tuberculous disease in children should be put out of court altogether. For instance, we had a class of cases in which the disease

began as a small focus especially in the bone, and if only these cases could be recognized at an early stage, it was often possible to cut short the disease by removing the deposit before it had infected the joint. He had had several cases of this kind and had been struck by the fact that although this condition was also well recognized by other surgeons it did not seem to be followed by the logical conclusion—namely, operation. He had had several very excellent cases in which operation had cut short the disease. Again, in certain joints, not only could the disease be cut short, but also the functional result would be better if operation were performed, than if expectant treatment were carried out for a very long period. The chief joints to which he referred were the elbow and the ankle; in the ankle more especially a stiff joint was very serious, whereas by removing the astragalus, and taking away the whole synovial membrane, a very excellent movable joint was obtained, and the disease was at the same time cut short.

As regards what had been said about tuberculous abscesses, he was quite in agreement. He thought that they ought to be aspirated as a rule, because in the present day, when the so-called aseptic treatment was so much in vogue, it was impossible to prevent the entrance of organisms into the abscess cavities where drainage tubes were employed; on the other hand, with more rigid care this was quite a feasible thing to do. Considering, however, the state of matters as regards the treatment of wounds, he quite agreed that the safest plan was repeated aspiration, with, if necessary, injections of such materials as iodoform and ether, iodoform and glycerine, &c. For his own part, however, he preferred opening these abscesses and removing as much of the diseased wall as possible.

With regard to tuberculin, he had used tuberculin (T.R.) in almost all cases for a long time, but he had to confess that he had not yet seen any result from the use of it, which he was not familiar with in cases where it had not been employed; in fact, he doubted whether tuberculin as at present used was of any real value. It might be objected that tuberculin had not been properly administered, but as regards that he had always employed experienced bacteriologists to carry out that part of the treatment. In several cases where the tuberculin had been used by men whose knowledge was above suspicion he had seen fresh tuberculous lesions develop during the course of the treatment just as if no tuberculin had been employed. For example, in a case of tuberculous epididymitis which had been under treatment for about eighteen months, the epididymis on the opposite side became

affected while the treatment was still going on, the clinical course being practically the same as where no tuberculin had been used; in fact, the infection of the second epididymis had occurred sooner than one would have expected. He believed that a great deal of what was written about the excellent results of tuberculin depended on the fact that it was carried out by men who had no clinical experience of the mode in which those cases progressed under other treatment, and were inclined to attribute any improvement they met with to the employment of the vaccine. Although he still continued to use tuberculin he felt convinced that the bacteriologists had not hit upon the right doses or the right frequency in administration. It always seemed to him a very curious thing that such long intervals should be necessary between the doses. As a matter of fact, he saw at Frimley and elsewhere very remarkable recoveries in cases of phthisis with graduated exercises without the use of tuberculin, and the theory of the bacteriologists was that in these cases there was a constant auto-inoculation going on, which produced the benefit; but if that theory was correct, then we had the patient getting doses of tuberculin every day—and presumably somewhat large doses, seeing that the patients worked hard—and he was inclined to think that in practice also one ought to give the tuberculin much more often, and possibly in larger doses, than was at present done.

Mr. H. J. GAUVAIN (Alton) said that as Medical Superintendent of the largest hospital in the kingdom for the treatment of tuberculous joint affections in children, he had opportunities of observing the effects of treatment by others, and of undertaking treatment himself, which were probably unique in this country. He was ardently in favour of conservative treatment, but that did not mean he was not alive to the importance of radical treatment under certain circumstances. Mr. Robert Jones said he would leave a tuberculous abscess alone for some time, and then open and curette it, when he had hopes that the abscess was not connected with the primary focus; the necessity, however, must often arise for interference before such separation had occurred. Sir Watson Cheyne had advised dealing by radical measures with a tuberculous focus adjacent to but not involving a joint, on the grounds that the joint might later be involved. He thought that in many cases one could tell whether such early operation was advisable, because his experience had led him to form conclusions as to the course the disease would probably take. There was a certain class of patients which he would call the "cachectic tuberculide," in whom the disease is

very virulent—these patients formed a class apart and might be clinically distinguished. They often had red or sandy-coloured hair, long eyelashes, a sallow complexion, they did not seem to get burnt even when exposed to the strong rays of the sun. In these persons the disease ran a very acute course, and in them he advocated early operation if the joint was not involved. They would, indeed, probably generally recover without operation under prolonged conservative treatment, but cure would be long delayed, and complications almost certainly occur. When patients had not these characteristics he would prefer to adopt conservative measures, as the disease would almost certainly be arrested without the joint being implicated. When abscesses did arise he invariably aspirated, and he had concluded that this was the best and safest way of treating them. He regarded the process of cure in an abscess after aspiration and injection as different to that in an abscess after incision. There was a view prevalent that conservative treatment and convalescent treatment were identical, but he did not agree with that. Sir Anthony Bowlby made a plea for children to be treated in convalescent homes; with this he disagreed. The opener of the discussion had summed up the condition very happily in one important sentence, "The disease is arthritis occurring in a tuberculous patient, and is not merely a joint affection"—with this statement he cordially agreed. That being so, treatment should be on two lines—general and local—the latter being undertaken in circumstances most favourable to the patient, away from town and in the best country air. He believed that general treatment should be supplemented by very strict local treatment, in country hospitals which were specially equipped for the purpose. This yielded strikingly good results, so good, that as time went on he thought it would be the regular practice for children suffering from tuberculous joint disease to be despatched to such hospitals as soon as the disease had been diagnosed. Too often such cases were sent to those institutions only when they had become hopelessly deformed and riddled with sinuses, and even when cure could be accomplished they remained helpless cripples. If his suggestion were carried out he believed there would be 95 per cent. of recoveries, mostly without deformity. But such hospitals must be properly and adequately staffed, and in such hospitals each case must be treated strictly according to its special requirements. It was bad treatment for every case of tuberculous hip to be placed in a Thomas's splint; it was as bad as syringing every diseased ear or irrigating every affected eye. The ordinary convalescent homes could not undertake the special treatment required, nor employ expert

assistance. Usually they were situated in localities not specially selected for the treatment of surgical tuberculosis. They were often the outcome of a genuine but perhaps imperfectly directed philanthropy. Treatment too frequently consisted of that which could be given by a local medical man attending once or twice weekly for an inadequate remuneration.

Mr. J. JACKSON CLARKE said one point he thought might be considered was the percentage of cases of tubercle of joints, apparently cured in infancy upon the lines of treatment indicated by previous speakers, which recurred in later life. Though the discussion was limited to cases occurring in persons of not more than 15 years of age; he did not think it should exclude the later history of joint disease in the same patients. In a certain number of these the disease appeared to remain latent in a joint, the patients being only apparently cured of their disease. He remembered one such case, that of a man about 35 years of age, who, after having some massage, was admitted to hospital with intense arthritis of the knee. On opening the joint he found an extensive tuberculous synovial infiltration and an old abscess cavity in the head of the tibia, and he dealt with it by excision, as he did with other joints which had resisted conservative treatment. Another case was that of a girl, now aged 20 years, upon whom he operated at the age of 12 years. After making the bone sections he wired the femur and tibia together with wire loops. These wires could now be seen to have been rent asunder in the growth of the bones and several of the fragments embedded in bone had been carried to a distance of some inches. With regard to removal of bone foci, a focus occurring in a bone close to a joint presented itself clinically as a case of joint disease. He had an instance which bore on the point in hospital at the present time. A child had been treated for knee disease, and suddenly there was the formation of a large abscess in the bone in the front of the knee. He opened this and found an escape of pus into the abscess cavity from a minute hole in the epiphyseal end of the tibia. The hole led to a cavity in which were many necrotic fragments, the remains of the epiphyseal centre of ossification. The fragments were removed by means of a Volkmann's spoon in the gentlest way, in order to prevent extension to the knee-joint itself, and some iodoform emulsion and carbolic were put into the cavity. A Bier's bandage was applied for a time, but had now been removed, and the abscess cavity was rapidly healing. As far as his experience with tuberculin had gone it coincided with that of Sir Watson Cheyne;

whenever a case seemed to require it he had employed it through experts, but he had not seen a case of commencing cavity formation which had resisted other treatment ameliorated by it. In his experience, it was in the very cases which needed much assistance that tuberculin seemed to fail. He was in agreement with the surgical principles enunciated by former speakers.

Mr. W. G. SUTCLIFFE (Margate) said there was little to add to what had been said. He had had opportunities of watching the kind of cases under discussion as he was medical officer at one of the homes of the Metropolitan Asylums Board, where about 4,000 children had been through his hands, most of them suffering from surgical tuberculosis. It was not sufficiently appreciated how long such cases took to heal; it took many years to cure a case of tubercle; the average time for a tuberculous knee or hip was three or four years. Hip cases did best if kept continuously in bed for months; even after that, if the child got up, the limb might begin to alter its position and be flexed and adducted in spite of a Thomas's splint, and all the evidences of recrudescence could be seen. This was another evidence in favour of what Mr. Gauvain urged—proper skilled supervision. It was usual to get these cases late, not early. What was to be done with a bad hip-joint case where there was an abscess and the hip dislocated before the appearance of sinuses? Was it better to aspirate the abscess and leave the head in position, or should the head be excised straight away? He had gone through the various stages. First he tried to put the head into its place; then he proceeded to excise them; now his practice was to clean the abscess out and leave the case alone until it had subsided. A couple of years afterwards the deformity could be corrected by osteotomy. He was not certain which was the better practice. Tuberculin had been used at Margate, but his experience was much the same as that of Sir Watson Cheyne.

Mr. E. M. CORNER: There is no doubt that if a tuberculous arthritis in a child can be persuaded to heal without operation, it is better for the patient. Thus non-operative cases give better results than those operated upon; and for the very obvious reason that the non-operative cases are those of patients who can get well by their own efforts, whilst the cases which come to operation are those of patients who cannot. Therefore the results should not be compared. Under such circumstances, non-operative treatment can be pursued and per-

severed with in the children of the moneyed classes, who have almost unlimited time at their disposal. And, with the loss of health and schooling, inseparable from non-operative treatment, it is improvident and wrong to depend long on non-operative treatment in dealing with hospital cases whose time to be expended on preparations for life is very limited. In them operations should be undertaken sooner; and the rich and the poor not treated alike. Thus the first point which I would urge for consideration in the indications for surgical interference in tuberculous joint disease in children is that of the circumstances of the child and its guardians.

In the next point I would suggest that we turn from the circumstances of the guardians to the disease in the child. Clinically, there are usually two points on which reliance can be placed—the duration of the disease and the presence of an abscess. If the disease has not subsided within reasonable time, operation should be undertaken. What is "reasonable time" in one case, or situation, differs from what is "reasonable time" in another. For instance, the more obvious the disease the shorter the time reasonable to wait before operation. Hence what is meant by reasonable time for a particular case can only be decided when considering that particular case. No rule should be formulated as to what is a reasonable time in the abstract. Clinical experience and commonsense are the only reliable guides. What is reasonable time for the child of a rich man is too long to be reasonable for the child of poor parents with a limited time for schooling and early and severe competition in the struggle for existence.

As yet no consideration has been made as to the kind of operative treatment. Now is the time when this has to be considered. I have put down "the presence of an abscess" as an indication for the cessation of non-operative treatment and the commencement of operative treatment. And in practice, particularly private practice, it is the slow, insidious formation of an abscess which often makes operative treatment imperative. Hence the question of what treatment is to be adopted. The answer to this is to be found in the statement that no extensive operation should be undertaken when inflammation is acute or active, except under compulsion. That is to say, for operations of expediency, do them *à froid* and not *à chaud*; that is, do "interval" operations, as in appendicectomy. Hence, if the tuberculous joint is *à froid*, do an erasion which is full and complete because, as with cancer, the greatest hope of success depends on the thoroughness of this first operation. Temporary drainage with Kocher's tubes (twenty-four to forty-eight hours) removes

much serum and pabulum for micro-organisms, ever present in the wound, to grow on. In the modern days of clean operations and dressings, such temporary drainage can be carried out without infecting the wound, to the great advantage of the patient. Hence temporary drainage is a good thing, should be practised, and is a great help in obtaining union by first intention in the deeper parts of the wound. If the diseased part is *à chaud*, a secondary infection of the part has occurred, and the patient should be put to bed to see if the *chaud* will subside. If it will not, it is best to be content with incision, curetting, flushing, and closing the abscess, and wait, before undertaking an extensive erasion, for the affected parts to become more quiescent.

At this place reference must be made to cases of tuberculous abscesses in the neighbourhood of joints, particularly of the hip and the knee. Besides opening the abscess and curetting, the joint should be treated by rest and splintage; as, if these cases are watched, the joint disease often becomes manifest later.

In all operations on tuberculous joints the strictest attention must be given to the perfect co-aptation of the skin edges. To that end attempts are made to prevent the formation of an unhealed spot. The formation of a sinus is disastrous, and inevitably leads to the secondary septic infection of the part. Then the part should be reopened from time to time and sulphured.

These forms of operative treatment can be continued till the patient gets well or amputation is necessary. Tapping of a tuberculous abscess, though it succeeds sometimes, is too uncertain a method to be recommended for use except under exceptional circumstances, such as when the patient is very ill from phthisis. Tapping and injection is also a procedure so uncertain as to be inapplicable in daily practice, and is only likely to be of real use in dealing with very leisured classes. Incision, with temporary drainage, being far more safe and certain, is preferable.

Many speak of "radical" operations, which can only mean the removal of most diseased tissue; all can never be removed. All radical operations should be of the nature of erasions. Formal excisions of joints are operations of the past. Arthrectomies are erasions performed on joints. Their results, as regards the shape and function of the limb, are often so bad that an erasion should never be done unless the disease compels its performance. Erasions should always be done widely, so as to remove as much disease as possible and to leave healthy tissues in the wound.

It may be asked if early operation should replace treatment by rest and splintage. My answer to such a question would undoubtedly be "No," because a successful early operation inflicts as much, or more, harm on the functionable capabilities of the joint as does the tuberculous disease in a joint that gets well. In addition, I would assert that much which is called tuberculous disease of joints, and particularly early disease of joints, is nothing of the sort, but an arthritis due to the presence of other organisms; for instance, from such a joint I have had a streptococcus cultivated. These reasons are sufficient for not replacing the treatment by splintage with early operation.

It may also be mentioned that I believe that there are a number of cases of chronic arthritis which are often regarded as tuberculous, and which are not tuberculous. I have had grown for me from joints regarded clinically as tuberculous, streptococci, staphylococci, and pneumococci. In fact, these cases are sufficiently numerous for their non-recognition to complicate seriously the question of the results gained hitherto in instances of what were regarded as tuberculous joints.

Treatment with tuberculin is in the great majority of cases wholly unsatisfactory; as is also any attempt to watch the clinical progress of the case by means of those "guesses" called the opsonic index. But, and quite unexpectedly, I have seen good results apparently due to tuberculin. They are exceptional, and why they should occur in some cases and not in others I do not know.

Mr. Lockhart Mummery: Everything in these cases depends upon the stage of the disease at which the patient first comes under treatment, and upon the surroundings of the patient. Where the parents are well-to-do and the patient is seen early and can be treated under the best conditions, results are excellent both as regards recovery and subsequent function of the diseased joint. Unfortunately, we have to deal with a large number of cases who cannot be treated under the most favourable conditions, and it is with regard to the treatment of these cases that improvement is needed. Like most surgeons of the present day, I was taught to believe that tubercle of the joints and bones required drastic surgical interference, the object being to remove completely all tuberculous tissue. Excisions of joints, careful and thorough dissection of synovial membrane, and dissecting away glands in the neck as if this were malignant disease, were supposed to be the correct treatment. When I began to see large numbers of cases of tuberculous joint disease at the Queen's Hospital, I was at once struck by the very unsatisfactory

results. Very many cases failed to be cured, and those in which the disease was arrested almost invariably suffered from serious functional disability. For some time now I have adopted the exactly opposite line; doing as little as possible when obliged to operate; not attempting to remove all diseased tissue, but merely to put the joint in a condition as favourable as possible to recovery. When pus forms I let it out through a small incision and remove any caseous material, and immediately sew the wound up again.

All cases of hip disease are treated by extension for a fortnight or longer, and then fitted with a double Thomas's splint, which is not removed until all symptoms of active disease in the joint have been

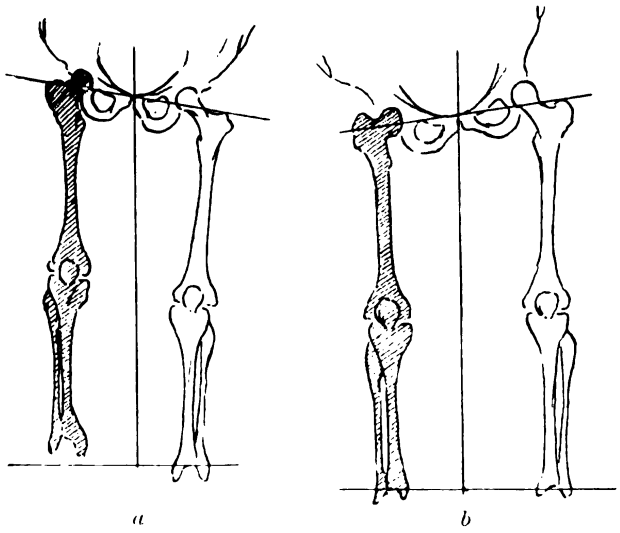


FIG. 1.

Diagram to show advantages of abduction of ankylosed limb. Affected limb shaded.

(*a*) Usual position of ankylosed limb if not corrected—slight adduction. Result: increased shortening to bring limbs parallel for walking.

(*b*) Best position—abduction. Result: tilting of pelvis to bring limbs parallel for walking—gets rid of shortening and brings knees to same level.

absent for six months, taking care to err on the side of keeping the child in splints too long rather than too short a time. When abscesses form they are opened, evacuated, and closed at once under scrupulously aseptic conditions. I have entirely abandoned excision of the hip-joint, and believe far better results can be obtained by conservative methods.

The worst cases are those in which secondary infection has occurred. The only thing to be done in such cases is to ensure free drainage and

make every possible attempt to get rid of the secondary infection by careful dressing. In this connexion I have found packing septic bony cavities and old sinuses with gauze soaked in formalin give very good results in some cases. My practice has been in cases where secondary infection already exists, to scrape out thoroughly all sinuses and pockets, cut away diseased skin, and by flushing get rid of as much septic material as possible. Then to plug all the cavities of the wound with gauze soaked in formalin 5 per cent.: stitches are put into the skin wound but not tied. The gauze is removed in twenty-four hours and the stitches tied. In some cases I have been able in this way to close discharging sinuses. I believe vaccines prepared from the patients are useful in these cases in aiding us in getting rid of the secondary infection, but tuberculin, in my

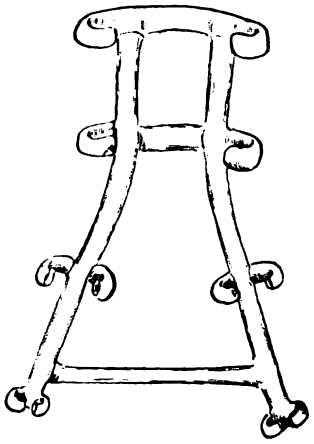


FIG. 2.

Modified double Thomas's splint.

experience, is useless. There is one point in connexion with hip disease which has not, I think, received the attention it should, namely, that in those cases of hip disease in which ankylosis of the joint with shortening is the desired result, enough care is often not taken to get ankylosis in the best position; that is to say, in abduction. A child with a lower limb abducted to 30 degrees and the hip shortened by, say, 1½ in. and ankylosed is in a vastly better position than one who, as is too often the case, has the limb in a slightly adducted position. If the limb is fixed in abduction the child can, by tilting the pelvis, bring the two feet to the same level, whereas, if the limb is fixed in adduction the child has to tilt the pelvis in the wrong way, in order to get the limbs sufficiently

parallel for walking, thereby still further increasing the shortening of the limb on the affected side (fig. 1). My own practice has been to have the double Thomas's splints made with the legs widely separated (fig. 2) and where necessary to perform tenotomy of the adductors. I should very much like to hear the views of other surgeons as to the best method of obtaining ankylosis in an abducted position in these cases, as it is often a difficult matter.

With regard to operative treatment, my own belief is that operations upon these cases should be confined to incision and evacuation of pus (or aspiration), with immediate closure of the wound. Drainage should not be employed unless secondary infection has already occurred, when it is, of course, necessary. Radical operations, such as excision and erasion of joints, should not, in my opinion, be performed. Operations such as tenotomy, with the object of preventing subsequent deformities and to relax contracted muscles pulling upon the joint, might, I think, advantageously be used more often than at present.

Careful attention to the child's general health, and especially to its digestive functions, I believe to be most important, and vaccines other than tuberculin I think are useful in cases where secondary infection has occurred.

Like Mr. Tubby, I look forward to the time when all these cases will be treated, until they are well, in convalescent homes in the country.

Mr. A. H. Tubby said that, owing to the length of the meeting, he would not venture to enter upon a reply, but would content himself with thanking the speakers for their remarks and his audience for the sympathetic attention which had been accorded to him. The discussion had left upon his mind an impression of remarkable unanimity, and he was very glad to have had the opportunity of opening it.

Dr. A. Butler Harris, in reply, said it seemed to be the opinion of many that tuberculin was not of much use. He hoped that he had brought forward some evidence to show that in certain cases it was of some use. In reply to the remark of Mr. Robert Jones, criticizing him for moving a joint under treatment, he wished it to be understood that he would not do this unless he could see some indications that cure had probably been effected.

Section for the Study of Disease in Children.

January 26, 1912.

Dr. G. A. SUTHERLAND, President of the Section, in the Chair.

Cases illustrating the Late Results of Muscle Transplantation for the Relief of Talipes Valgus (Paralytic).

By DOUGLAS DREW, F.R.C.S.

THESE cases are brought forward mainly to illustrate one point, i.e., the tendency to the development of a slight degree of talipes varus as the result of transplanting the tendon of the peroneus brevis on to the tibialis posticus for the relief of paralytic talipes valgus. All the cases were well-marked examples of the deformity; in one the deformity was associated with equinus and in another with calcaneus. An interval from one and a quarter to three and a half years elapsed between the onset of the paralysis and the operation for the relief of the deformity. In all the cases the same operation was performed for the relief of the valgus part of the deformity, transplanting the tendon of the peroneus brevis on to the tibialis posticus by passing it across, beneath the tendo Achillis, and over the other tendons at the inner side of the ankle. The operation was most successful in remedying the valgus, but in every instance a slight degree of varus has supervened.

DISCUSSION.

Mr. DREW regretted that only one of his cases had come. It was curious that all of them developed a slight degree of varus after an interval of about two years from the operation. In order to remedy the varus which had resulted from the transplantation, he detached the peroneus brevis from the tibialis posticus after an interval of six years, but it had not cured the varus, which now appeared to be due to overaction of the tibialis posticus, which had recovered from the paralysis. As was well known, infantile paralysis went on improving for years, and it might be urged that in this case he had operated at too early a date; as a matter of fact it was the shortest interval he had allowed—i.e., a year and a quarter from the onset of the paralysis.

Dr. W. J. MIDELTON said it was known that infantile paralysis was due to an infection, and the question was whether anything could be done to hasten the restoration of cells of the spinal cord and improve the muscles locally. Massage and electricity might be good, but he thought that the method of counter-irritation which he had carried out in other muscle conditions was better. It was a revival of the old-fashioned acu-puncture with counter-irritants, by which one produced a local hyperæmia and thus restored the nutrition. In the case of a lady who had sprained her ankle thirty years previously, her foot having since become more and more useless, his treatment by counter-irritation resulted in great benefit. He had also used this method with success in the case of a man who had injured his foot forty years previously.

Case of Cerebellar Tumour.

By ERIC PRITCHARD, M.D., and SYDNEY STEPHENSON, C.M.

FIVE years ago patient received a blow on the head. Three years ago he vomited for no apparent cause, and since then he has been subject to intermittent attacks of headache, vertigo, and sickness, which are preceded by optic prodromata, which usually take the form of fortification spectra with a brilliant range of colours. There are no paralyses and no disturbances of a psychical or sensory character beyond those stated. There is slight inco-ordination in walking and Romberg's sign is present. Wassermann negative; von Pirquet negative. History of tubercle in mother's family.

Eyes: Right vision $\frac{5}{6}$ (two letters); left vision, $\frac{5}{6}$ (six letters). Pupils, 6 mm. to 6·5 mm. Hippus, especially in left eye. The pupils respond to light, both directly and indirectly. When the eyes look straight at an object—as, for example, the ophthalmoscopic mirror—they jerk slightly up and down. There are also jerking movements on looking inwards, outwards, and upwards. Double papillœdema—that is to say, swelling of each optic disk (summit seen with + 4·0D spherical glass) without inflammatory signs or retinitis. The retinal veins are relatively large and the retinal arteries relatively small.

Mr. STEPHENSON regretted the absence of Dr. Pritchard, as he was responsible for the diagnosis of cerebellar tumour. The patient had double papillœdema. The disks were swollen, but there was no evidence of inflammation, and in particular there was no inflammation of the retina. The boy attended the hospital on account of severe headaches, vomiting, and vertigo, and those symptoms, coupled with the double papillœdema, made one suspect that the underlying cause must be an intracranial tumour.

Two Brothers with Hæmophilia.

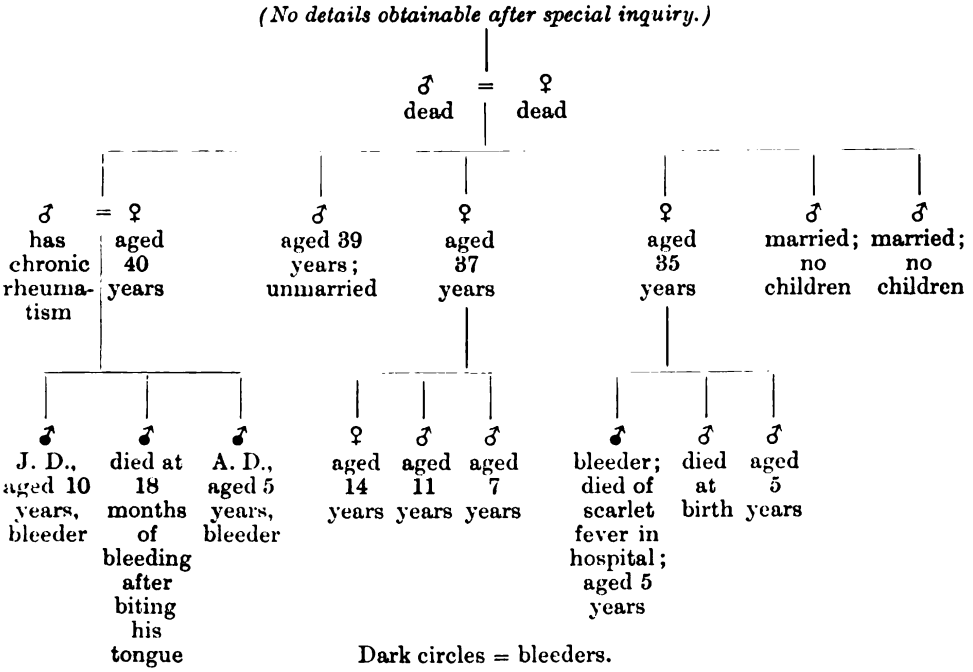
By THEODORE THOMPSON, M.D.

J. D., AGED 10 years, a thin, poorly nourished boy. Symptoms noticed since infancy. Frequent bruising on very slight trauma. Bleeds for one hour if he scratches his hand. Sometimes there is slight bleeding from the mouth, in which there are several carious teeth. On one occasion there was severe hæmorrhage from the gums (1908). For several years has had attacks of pain and swelling in the knees. The left knee was first affected and is now enlarged, and the synovial membrane thickened. The right knee is swollen by synovial effusion. No gastro-intestinal hæmorrhages. Coagulation time (Wright's method), five minutes (normal, three minutes).

A. D., aged 5 years. Stout and well nourished. Extensive bruising of the skin and deeper structures occurs on very slight injury. No bleeding from mouth, nose, or intestines. The only family history of bleeding is on the mother's side.

HISTORY OF BLEEDING IN THE PAST GENERATIONS.

(No details obtainable after special inquiry.)



Dark circles = bleeders.

Deformity of the Chest.

By R. C. Jewesbury, M.B.

Girl, aged 12 years. The mother first noticed " something wrong " with the right shoulder about ten months ago. There was no pain

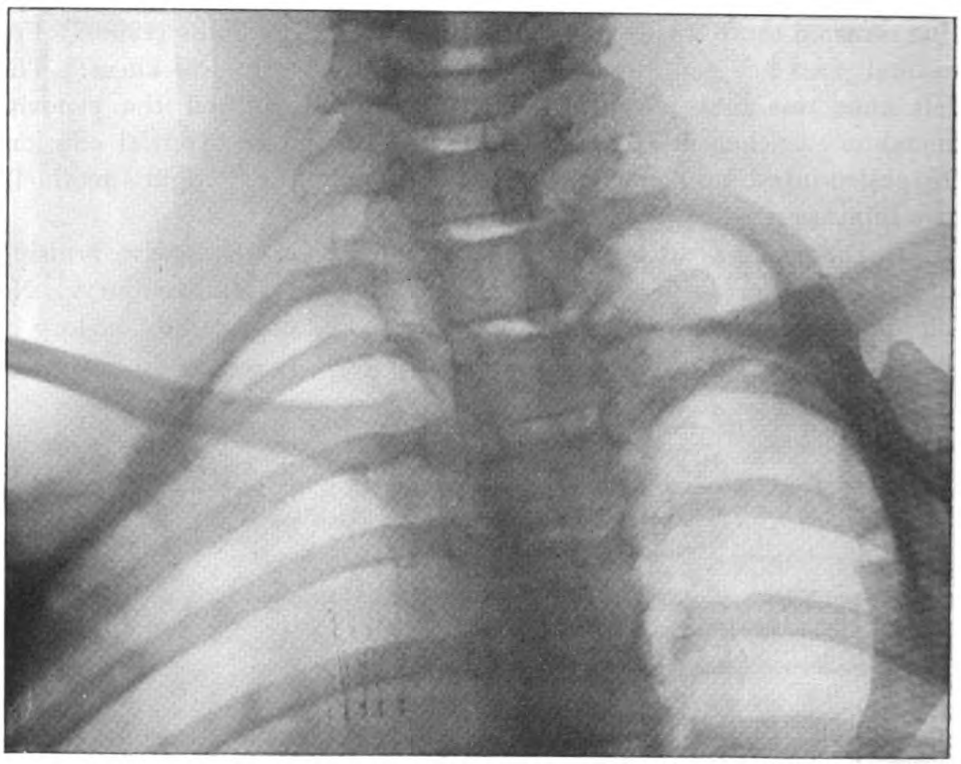


Fig. 1.

Postero-anterior view of the thorax. There is to be seen an obvious deformity of the upper ribs, on the right side. The first is possessed of a process (anteriorly) which articulates with a similar process joining from the adjoining surface of the second rib. A supernumerary seventh cervical is definitely present on the left side, and a small one on right, which is just to be distinguished in this view.

or loss of power and the general health was excellent. History of diphtheria three years ago and the usual childish ailments. She had a severe fall down fifteen stairs when $3\frac{1}{2}$ years old, and was considerably

hurt at the time. Family history good. The upper part of the right side of the chest is much flattened, the first and second ribs are sunken, and bony thickening is felt on these ribs below the clavicle. The pectoral muscles on the right side look wasted, but there is no loss of power. Lungs normal.

Skiagrams by Dr. Ironside Bruce (figs. 1 and 2) show deformity of first and second ribs right side. Bony outgrowth from first rib projecting downwards and articulating with similar growth from second rib. Also cervical ribs well developed on left side and rudimentary on the right.

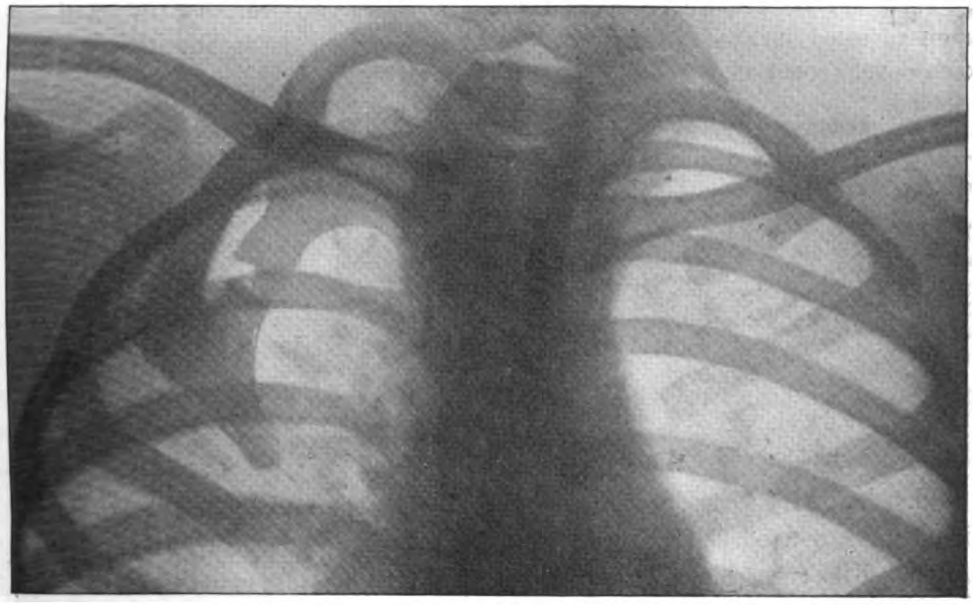


FIG. 2.

Antero-posterior view of upper part of thorax and dorsal vertebræ. The cervical ribs on right and left are now plainly to be seen, well developed on the left and quite small on right. The second rib right posteriorly is very badly developed. No sign of deformity of vertebral bodies, of cervical or dorsal region, is observable.

DISCUSSION.

Dr. MORLEY FLETCHER suggested that the electrical reactions should be carefully tested on the two sides, and the two pectorals especially compared. His opinion was that the right pectoral was less developed than its fellow. He did not think this was due to a cervical rib; and it was important to notice that the cervical rib was on the left side. The deformity or malformation

between the first and second ribs might cause some nerve lesion, which had involved chiefly the pectoral on the same side, causing some slight failure of development. In a case of congenital absence of pectoral muscle there was sometimes a similar falling in of the upper part of the chest.

The PRESIDENT (Dr. G. A. Sutherland) said the possibility of congenital non-development of the pectoral muscle had been suggested, but that was usually associated with atrophy of the ribs. A condition like the present case, in which there was some additional growth about the ribs, was not common in his experience. With regard to the possible association between supernumerary rib and wasting of the pectoral muscle, last year he saw a girl, aged 20 years, a student at Oxford, who had all the signs of an extra cervical rib. A skiagram showed a well-marked supernumerary rib. Subsequent examination revealed marked wasting of the trapezius and pectoralis major, with non-development of the breast on the right side.

Dr. JEWESBURY, in reply, said he would have the electrical reactions tested. He had not felt justified in recommending operation; there seemed to be none of the symptoms associated with supernumerary ribs.

Hemiplegia—Right Side.

By R. C. JEWESBURY, M.B.

BOY, aged 4 years. Full-time child. Rather prolonged labour; no instruments used. The child has been weak down the right side from birth. Fits from birth, chiefly affecting the right side, causing twitching, with loss of consciousness. Fits getting much less frequent. He began to get on his feet when 3½ years old; he is beginning to use his right arm and hand now, and started talking quite lately.

Family history: Three other healthy children. One miscarriage before first child. Parents healthy.

Marked atrophy of right side of face, narrowing of right palpebral fissure, weakness of muscles on right side of face, right pupil contracted. Weakness of right arm and leg; hand and foot cold; shortening of right leg. Talipes. Right knee-jerk increased. Babinski's sign present on right side. Coloboma of the right optic disk.

DISCUSSION.

The PRESIDENT said it was unusual to find in an ordinary case of right-sided hemiplegia marked atrophy like that on the left side of the face, and particularly the microphthalmos on that side. He did not see how that could follow from the lesions which caused the hemiplegia. He asked Mr. Stephenson whether the microphthalmos on that side was associated with the coloboma, or whether he found it in cases of ordinary hemiplegia.

Mr. STEPHENSON, in answer to the President, said he was not aware of the association mentioned. Microphthalmos was a congenital deformity; as also was coloboma of the optic disk.

Hemiplegia—Left Side.

By R. C. JEWESBURY, M.B.

GIRL, aged 7 years. Weakness down the left side was first noticed when the child was 7 months old. Difficult confinement, but no instruments. Began having fits of epileptic type affecting right side five months ago. Left arm and leg much wasted; marked loss of power. Extensors of arm especially affected. Walks fairly well; can raise arm above head, but unable to hold anything. Some shortening of both arm and leg. Weakness of left side of face. Reflexes increased on left side. Babinski's sign present on left side.

Hemiplegia—Right Side.

By R. C. JEWESBURY, M.B.

BOY, aged 4 years. Loss of use in right hand and arm noticed about one month ago. No convulsion or cerebral disturbance at onset and general condition good. No fits. Squint first noticed six months ago. Past history good. Whooping-cough when a small baby. Family history good. Marked loss of power in right hand and arm; the arm is usually held straight downwards and is kept stiff. Elbow-jerk increased if the muscles are relaxed. Grip feeble. He walks with the feet wide apart and keeps the right leg stiff. Weakness in right leg. Knee-jerk increased on right side if obtained when the muscular spasm is relaxed. Plantar response on right side extensor. Backward boy and speech difficult to understand. Wassermann reaction negative.

Congenital Pulmonary Stenosis without Cyanosis.

By F. Parkes Weber, M.D.

The patient (F. N.) is a fairly well built and healthy looking boy, aged 6 years. He is of very active habits, and there is no cyanosis, dyspnœa, or clubbing of the fingers. Examination of the heart shows that the apex-beat is rather too far to the left (half an inch outside the nipple-line), and that the dullness extends slightly too far to the right. The fact that the size of the heart is somewhat in excess of the normal is confirmed by Röntgen ray examination. Over the præcordium a harsh systolic murmur is to be heard, with its maximum intensity over "the pulmonary area" to the left of the sternum. The murmur is not carried up into the vessels of the neck. Blood count (May, 1911): Red cells 4,730,000 and white cells 11,000 to the cubic millimetre of blood; hæmoglobin 70 per cent. There is no history of rheumatism. The boy's mother died after his birth, apparently from puerperal fever.

Addendum.—The absence of polycythæmia in this case is probably not to be explained as a "relative anæmia"; that is to say, is probably not due to a deficiency in compensatory erythrocytosis connected with a condition of general cachexia, but it is probably to be explained by the absence of any physiological demand for secondary polycythæmia. It is therefore, like the absence of cyanosis, probably a favourable prognostic sign, indicating that there is as yet no deficiency in the supply of oxygen to the tissues of the body, and therefore that there is no need for any erythrocytosis. Probably a condition of cyanosis is the chief exciting cause of erythrocytosis when that symptom is found present in cases of congenital or acquired heart disease.

DISCUSSION.

Dr. Morley Fletcher asked if this patient was now in his normal state of health. In cases of pulmonary stenosis one sometimes found that the red cell count fell when the patient got an attack of bronchitis or failure of cardiac compensation. He remembered one case in which the red cell count was generally about 8 million, but it sometimes fell to about 5 million, and then the patient had to be taken into hospital until compensation was restored. Possibly

the present patient was in an anæmic condition which might account for the absence of the polycythæmia to be expected in a case of pulmonary stenosis.

Dr. T. R. WHIPHAM said that cases of congenital malformation of the heart were always interesting because they caused speculation as to the nature of the lesion present. He could not agree with Dr. Weber's diagnosis of the case, as he thought that the normal appearance of the boy, who showed no clubbing or cyanosis, and the absence of polycythæmia were against there being any pulmonary stenosis. On the last point, however, he did not lay too much stress, as cases of congenital pulmonary stenosis were met with in which there was no increase in the number of red corpuscles. In addition, the thrill in this case seemed to him to be more towards the apex of the heart than over the pulmonary area, and he thought that the maximum intensity of the murmur was in the fourth interspace rather than higher up. He regarded it as a case of deficient interventricular septum or some other malformation of the heart.

Dr. CLIVE RIVIERE thought the case did not follow the ordinary lines of pulmonary stenosis, and agreed that there was some doubt about the nature of the lesion. There appeared to be some thyroid enlargement, an uncommon condition in a boy of that age; it was not clear that this had any connexion with the disease from which he was suffering.

The PRESIDENT said he always admired the courage of anyone who gave a diagnosis in connexion with congenital disease of the heart. This did not seem a clear case of pulmonary stenosis. To those who had not seen the heart under the screen, the dilatation of the heart seemed to be more on the left side than on the right. The murmur was a very loud one, and no doubt congenital in origin.

Dr. PARKES WEBER, in reply, said he was glad that those who had spoken agreed that the case was one of congenital cardiac disease. With regard to Dr. Whipham's suggestion that the case might be one of patency in the interventricular septum, he thought the murmur was too far to the left for that. There was a remote chance that it might be a case of patent ductus arteriosus, in spite of the murmur being systolic, not the long, continuous, wavy murmur present throughout the whole cardiac cycle said to be characteristic of patent ductus arteriosus. There were some cases which had been proved post mortem to have been instances of patent ductus arteriosus in which the murmur heard during life had only been a systolic one. On the whole, he thought that the present case, in spite of the position of the apex-beat, was one of congenital pulmonary stenosis without any other congenital defect. As Dr. Morley Fletcher remarked, the red cell count showed that there was no secondary polycythæmia. This might be due to a defect in the general state of health giving rise to what one might term "relative anæmia," but there were many cases known of congenital pulmonary stenosis in which the number of the red

cells was not above the standard for ordinary persons. There was certainly a diffuse moderate enlargement of the boy's thyroid gland, as Dr. Clive Riviere pointed out. The boy did not appear ill. What drew attention to the presence of the cardiac abnormality was a medical examination at the boy's school, or a suggestion by the school doctor that the boy should be medically examined

Lymphatism in a Boy.

By Edmund Cautley, M.D.

Boy, aged 6½ years, the eldest of three children. The family history is unimportant. He cannot be said to be unduly prone to infectious disease although he has had measles, varicella, and bronchitis. Last year he was under treatment by a colleague for anæmia. His left testis is undescended. The signs suggestive of lymphatism are the following:—

(1) The boy is tall for his age.

(2) There is a somewhat triangular patch of dullness over the manubrium, extending more to the left than the right, with its base upward.

(3) Some general enlargement of the lymphatic glands and spleen.

(4) Adenoids and hyperplasia of the circumvallate papillæ.

(5) The heart beats slowly, and remains slow although the boy is very frightened. The first sound is loud and rather slurred at the apex (? a small heart and relative aortic stenosis). Red cells 3,200,000, white cells 50,000 per cubic millimetre.

(6) Pupils large and complexion pale.

(7) Thin skin with excess of subcutaneous fat.

Specimen of Tuberculous Tumour of the Dura Mater in a Child, aged 14 months.

By Edmund Cautley, M.D.

The patient was the third child, born at term and weighing 10 lb., and breast-fed. The mother had had no miscarriages. Two other children had got pertussis. On November 7, 1911, he was admitted to Guy's Hospital for wasting of three months' duration, but did not

improve very much. At this time he was weaned. He was admitted to the Belgrave Hospital for Children on December 27 with a history of slight cough of fourteen days' duration, and general convulsions at mid-day, with cyanosis and loss of conciousness. The temperature at 3.30 p.m. was 97° F., and examination revealed general bronchitis. At 6 p.m. fresh general convulsions occurred, perhaps a little more on the left than the right side, and the temperature rose to 101° F. Next morning the child seemed well. On examining the chest in the afternoon there were definite signs of consolidation of the left upper lobe, most marked in the first and second interspaces near the sternum. The liver was unduly large, and there was distinct evidence of rickets. The case was regarded as one of bronchopneumonia of the left upper lobe, possibly due to pertussis, though the history of prolonged wasting and the enlarged liver were in favour of tuberculous disease. The child weighed 14 lb. 5 oz., and progressed favourably for ten days, except for loss of weight. On January 4 he had lost 4 oz. On January 8 he had lost another 9 oz. In the evening he had further fits and his temperature rose to 100·6° F. Two days later he had more convulsions and died. Throughout the illness the pulse-rate was unduly high and the rhythm not at all suggestive of a tuberculous affection of the brain.

Autopsy.—Situated under the manubrium sterni, superior to the pericardium and encroaching on the left lung, was a large suppurating tuberculous mass. It had apparently begun as a tuberculous gland in the anterior mediastinum, and, breaking down, had extended into the upper lobe of the left lung, which showed on section tracts of pus and small cavities. The glands in the posterior mediastinum were also enlarged and caseous. Some recent adhesions were present in the left pleural cavity. The abdomen was not examined. Attached to the dura mater, and apparently growing from it, was an irregularly shaped tuberculous tumour about the size of a large hazel-nut. It was situated on the right side and caused a corresponding depression, about $\frac{1}{2}$ in. deep and $\frac{3}{4}$ in. in superficial diameter, in the superior parietal lobule posterior to the ascending parietal lobule. There was no general dissemination of tubercles in the brain or lungs.

Shortening of the Left Femur.

By P. Lockhart Mummery, F.R.C.S.

Child, aged 7 years. History of breach presentation at birth. The mother stated that she first noticed something wrong with the left leg at the age of 2 weeks. When the child began to walk at the age of 14 months it was noticed that he limped. He was treated at the age of 3 years for infantile paralysis. For the last two years he has worn a high boot on the left foot, and there does not appear to have been any marked increase in the shortening during that period. He has had the usual infantile diseases, but there is no other history of importance. The left leg is 1½ in. shorter than the right, the shortening being in the femur. There is a slight difference in all the bones on the left side as compared with the right, but with the exception of the femur no more than can be accounted for by the fact that the child has walked mainly with the right leg. X-ray photographs show a very slightly reduced angle of the neck of the femur on the left side, but no other deformity except a slight thickening in the neighbourhood of the upper epiphysis.

Mr. Mummery believed that the shortening of the femur was due to injury at birth to the upper femoral epiphysis. He stated that he proposed to remove a piece of the centre of the shaft of the right femur in order to make the legs the same length and bring the knees level. He proposed to remove slightly more bone than was necessary to correct the length in order to allow for any subsequent difference in development between the two femora. He did not think that there would be much increase in the shortening if his original supposition was correct, as most of the growth would in future be from the lower epiphysis.

DISCUSSION.

Mr. Norbury suggested the advisability of trying the effect of a high boot for a time to see how much growth there would be. If necessary a portion of bone could be resected later on.

Mr. Mummery, in reply, said the child had worn a boot for two years, and judging by the boot now there had been very little increase in the shortening during that time.

The Radical Cure of Inguinal Hernia in Children.

By PHILIP TURNER, M.S.

THE whole question of inguinal hernia in children, both as regards ætiology and treatment, has been very thoroughly discussed in recent years, the latest paper being one read by Mr. Kellock only a few weeks ago before the Surgical Section.[1] The chief object of the present paper is to bring forward a modification of the usual operation for the radical cure of inguinal hernia in children. Before giving the details of this there are—though I wish to avoid topics already fully discussed—certain general points bearing upon the necessity for operative treatment which I should like to briefly mention. Let me first of all make it clear that my remarks throughout apply not only to hernia in infants but also, and indeed chiefly, in children up to the age of 12 years, or even slightly older.

The interest taken in this subject is thoroughly justified, both by its frequency and by its importance, for at the present time a boy with a hernia on leaving school and starting in life is placed at a serious disadvantage. Formerly this was the case only when he wished to enter one of the Public Services, but the effect of recent legislation, especially the Workmen's Compensation Act, has been to greatly increase this disability. As the result of the liability of employers under this Act most of the large employers of labour, such as railway companies and many large private firms, insist on all applicants for employment undergoing a medical examination. In the majority of cases the presence of a hernia, however slight, whether supported by an efficient truss or not, will lead to rejection. Boys leave our public elementary schools about the age of 14 years, and such a lad, the subject of hernia, certainly is placed at a considerable disadvantage in the labour market. The rejection is not infrequently a surprise both to the boy and to his parents, the presence of the hernia being often quite unsuspected. Nor is this to be wondered at in the case of children of poorer parents, for it is by no means infrequent to come across an intelligent adult with a bubonocele, or even a well-defined hernia, who is quite unaware of the fact. One result of this disability is that in the out-patient rooms of our hospitals large numbers of young adults apply for admission for radical cures of

[1] *Proceedings*, 1912, v (Surg. Sect.), pp. 26-32.

hernias which could have been more effectively treated at an earlier age with less inconvenience to the patients.

Without entering into any controversy as to the congenital origin of every hernial sac, it is, I think, generally agreed that at any rate a large proportion of the hernias which appear in adults are not true acquired hernias but are the gradual or sudden protrusion of abdominal contents into congenital sacs which have persisted from infancy. Not infrequently in such cases a history of hernia during childhood which was "cured" may be obtained.

The importance of the early recognition and treatment of hernia has not yet been fully grasped by the general public. A hernia, too, in an older child is more easily overlooked than in an infant which requires constant attention from its mother. Too frequently a swelling in the groin is regarded as a "weakness" which the child will outgrow. Till recently, nasal obstruction, chronic otitis media, squint and other troubles were regarded in much the same way. I would suggest that the medical inspection of school children which is now systematically carried out might be used as a means of spreading this information. It might not be feasible to examine the inguinal canals of each individual child, but much might be done by pointing out to parents and to teachers the importance of swellings in the groin and the desirability of the early recognition and treatment of hernia.

With the treatment of hernias by trusses we are not here concerned. Though spontaneous obliteration of a patent processus vaginalis may occur during the first few months of life, provided that the sac is not distended by the continued or intermittent descent of abdominal contents, it is generally admitted that after the first year of life closure of the sac is extremely improbable. There are many objections to children wearing trusses, and the most that can be expected from such an appliance after the first year is that it will keep the sac empty. A cure by means of a truss after this age is practically certain to be apparent only. The orifice, though it may contract, is not obliterated, and the sac is still present, ready to receive some abdominal viscus which may be forced into it as the result of some sudden or long-continued strain in adult life. Operative treatment is thus strongly indicated when a definite hernia is present in children over 12 months of age as well as in infants at an earlier age, when the hernia cannot be satisfactorily controlled by a simple soft truss.

The essential point about an inguinal hernia in a child is the presence of the sac; there is no congenital weakness or deficiency of

the abdominal wall. If the sac is completely removed and the descent of the contents, with distension of the inguinal canal, thus prevented, the abdominal muscles may be expected to undergo their natural development and a normal strong inguinal canal will then result. If, on the other hand, the hernia is untreated the development of the muscles in the inguinal region will be interfered with, so that some weakness, especially of the posterior wall of the canal, will ensue. If the hernia is supported by a truss the continuous pressure is certain to interfere with the development of the abdominal muscles, so that in this way a truss will actually be harmful. The object of the operation in children is thus complete removal of the sac, and this I would add should be effected with the minimum amount of damage to normal structures. Any attempt to strengthen the inguinal canal in growing children is thus unnecessary, and indeed, owing to the delicacy of the structures involved, the necessary traumatism is likely to be actually harmful.

Two methods of removal of the sac are in common use. In the first the aponeurosis of the external oblique is divided from the external abdominal ring upwards and outwards as far as the internal ring. The sac is then separated from the cord and after it has been freed as high up as possible it is transfixed, ligatured, and removed. The canal is then reconstructed by suture of the external oblique. Though the sac is completely removed an objection to this method is that it involves considerable injury to delicate structures. The vessels of the cord may be lacerated, leading to severe hæmorrhage. Suture of the external oblique, owing to the thinness of the aponeurosis, may be unsatisfactory, especially the attempt to reconstruct the external abdominal ring. This is due partly to the normal tension in the aponeurosis in this situation at right angles to the canal and partly to the fact that the aponeurosis gradually fades away into the intercolumnar fascia. In the second method the inguinal canal is not opened up but the sac is separated from the cord below the external abdominal ring. It is then drawn down as much as possible, ligatured, and removed. The objection to this method is that the sac is not necessarily completely removed, but that a small funnel-shaped process of peritoneum may remain at the internal abdominal ring. I think that this method is suitable for infants but not for older children. Mr. Kellock, I notice, advises this operation, from which he has had excellent results, but the age of the oldest of his patients was only 3 years.

During the past few months I have operated in the following way, which ensures as complete removal of the sac as in the first method,

with as little, if not less, injury to normal structures than occurs in the second. A short incision is made parallel to and slightly above Poupart's ligament, exposing the aponeurosis of the external oblique. The incision stops a short distance above and to the outer side of the spine of the pubis so that the external abdominal ring is not exposed. A short incision (about ½ in. in length) is then made through the aponeurosis parallel to and just above the middle of Poupart's ligament. The internal oblique is thus brought into view. The lower edge of the divided external oblique is then gently raised and after it has been separated from the internal oblique the lower edge of the latter muscle is drawn upwards and outwards by means of a small blunt hook. This brings into view the sac and cord covered by the cremaster just below the internal abdominal ring. While an assistant retracts the internal oblique the cremaster is torn through longitudinally by two pairs of dissecting forceps. A blunt dissector can now be insinuated beneath the sac and the cord so that these structures can be drawn forwards through the small opening in the external oblique. The cord is now spread out as widely as possible, when the edge of the sac is easily seen and secured. The sac is now readily separated from the vas and the the other constituents of the cord as high as the internal abdominal ring. The isolated portion of the sac is now grasped and drawn upwards either by the fingers or by Spencer Wells forceps, and by means of dissecting forceps, blunt dissector or gauze is separated from the lower part of the cord. Separation is here also, as a rule, quite easy, though the sac is always more adherent at and below the level of the external ring. The sac, now completely isolated, can be drawn down, ligatured and removed, the stump disappearing from view beneath the internal oblique, which is then allowed to fall back into position. The spermatic vessels should not have been injured so that there is no bleeding from the cord, which is replaced in the canal. The small incision in the external oblique is then closed by a few catgut stitches.

I have now used the above method in about twenty-five cases, the age of the youngest patient being 3 months and of the eldest 12 years. I have, however, used a similar operation in selected cases in adults by continuing the incision down to, but carefully preserving, the external abdominal ring intact and then inserting sutures between the conjoined tendon and Poupart's ligament behind the spermatic cord, as in Bassini's operation.

Radical cure of inguinal hernia in a child may occasionally be a very troublesome operation; the sac may be firmly adherent to the coverings

or the vessels may be so spread out over it that they are very liable to laceration in the process of separation. Again, the sac may be so thin that in the process of separating it from the vas deferens severe lacerations may occur which render ligature of its neck a matter of difficulty. I should like to say here that separation of the sac in the situation described above—viz., just below the internal abdominal ring—is always easier both in children and adults than at or below the external ring. The fact which I wish to emphasize is that the sac can easily be isolated and freed in this position without interfering with the external ring or opening up the inguinal canal. When once the neck has been separated the subsequent isolation of the sac is easy. In no case have I had a hæmatoma or any swelling of the testicle. In two cases the sac was continuous with the tunica vaginalis, but these presented no difficulty, for the testicle was readily drawn up into the lower angle of the wound and the sac then divided just above it.

The small incision in the external oblique inflicts the minimum amount of damage on the inguinal canal. Indeed, in a sense this structure is not opened up at all, for beneath the incision is a thick muscular layer formed by the internal oblique and the transversalis. The canal is only exposed when these structures are retracted upwards and outwards. Suture of this small incision is a very simple matter compared with suture of the anterior wall of the canal.

This method can be carried out equally readily in infants and in older children. Generally speaking, I believe that this method is always as easy as, and frequently is easier than, the other methods.

My own experience of the results of the operative treatment of inguinal hernia in children is similar to that of other surgeons, namely, that they are excellent. All the cases I have treated in the above-mentioned way have been under my care in the last six months. I cannot, therefore, speak of ultimate results after such a short interval. I cannot imagine, however, that they will be less satisfactory than the results obtained by other methods.

As I have already stated, I bring this operation before the Society as a simple method of effecting the most essential part of a radical cure, namely, complete removal of the sac, with the minimum amount of injury to the inguinal canal or the spermatic cord.

DISCUSSION.

Mr. H. S. CLOGG thanked Mr. Turner for bringing forward what was to him a new idea. It was not fair to criticize any operation until one had tried it, and therefore he did not feel justified in criticizing this operation. It would seem to him that working through a small incision in the external oblique would render the difficulties of the operation in a little baby, and especially a little fat baby, greater. Admittedly it was more easy to separate and isolate the neck of the sac than any other part, and he always commenced the isolation of the sac at that point, with the canal freely opened. He thought the separation of the sac was always perfectly easy, but as it was covered with various layers of fasciæ, it was easy to make the mistake of attempting the separation before the sac was thoroughly exposed. The sac of an infant's hernia was very delicate, and tore easily. If this happened at the neck the sac might retract under the internal oblique, and only be found with difficulty through the small incision recommended by Mr. Turner. He (Mr. Clogg) had never found any difficulty in repairing the external oblique and restoring the external abdominal ring. The external oblique and intercolumnar fascia he overlapped and sutured.

Mr. HENRY SKELDING (Bedford) remarked that the method just described was not quite new, as he himself read a paper on hernia before the South Midland Branch of the British Medical Association last year, in which he advocated a similar method, though he did not enter into detail so much as Mr. Turner had now done. He advocated approaching the ring from the upper side instead of from below, and was pleased to find that Mr. Turner had found it so much simpler. In fat children especially it was important to get as near the ring as possible so as to prevent injuring the parts, which one could scarcely help doing where there was a large sac which it was difficult to dissect away. It was much easier when one approached it just as it emerged from the ring. With regard to the stump in very young children in whom the hernia was very small, it was only necessary to ligature it; but when the hernia was large, and the sac also was large, he nearly always adopted the method of transplanting—i.e., leaving the two ends of the catgut with which he had ligatured the cord, and re-threading them in the way recommended by Mr. Lockwood. He presumed Mr. Turner did this operation as early as he could get the patient, and he did the same. He had had a case of strangulated hernia in a baby, 5 weeks old, and there seemed no reason why one should not do the operation quite early. Another point was that with the upper incision the patient was kept much cleaner, and infection was not so likely as when the lower incision was made. He used nothing but horse-hair skin suture and collodion; he did not use a bandage or pad, nor did he, except in rare cases, ligature vessels, so that there was very little to disturb the patient.

Mr. NORBURY said he had seen a fair number of operations done on quite young children, and had performed a considerable number himself; he found one could get right up to the internal ring without splitting the external oblique at all. It had been pointed out by Mr. Murray and Mr. Hamilton Russell that the place to put a ligature was on the neck of the sac. One could tell that situation by the fact that the peritoneum was thickened there, and from the presence of subperitoneal fat. In the cases he had dealt with he found that by pulling gently on the sac one could always see that thickening, and so he considered it was unnecesary to split the oblique muscle at all.

Dr. MIDELTON asked how far Mr. Turner believed in the use of trusses. He would also like to know how early in infancy Mr. Turner would do the operation. Some time ago he had sent into a provincial hospital a child, 3 months old, for an operation for the radical cure of hernia, and the surgeon said it was absurd to send so small a child for that purpose.

Mr. PHILIP TURNER, in reply, said he feared he had not made himself clear to Mr. Clogg. It was not that the cut margins of the aponeurosis did not unite, but that there might be considerable difficulty at the time of the operation in drawing the two edges nicely together. It was not uncommon, when dividing the whole anterior wall of the canal, to find when one got down towards the external ring that there was much tension on the aponeurosis, which retracted at right angles to the axis of the canal, or one found that the aponeurosis gradually faded away into the intercolumnar fascia. These two facts and the thinness of the aponeurosis sometimes rendered suture there a difficult matter. He held that it was better to employ the simpler of two methods even if the other was equally effective. Now that he was in the habit of doing the operation, he found it as quick and quite as easy as any other. Occasionally one found very unexpected things in hernia in children. He had had one puzzling case. It was in a child who apparently had an ordinary hernia projecting from the external ring, but when he hooked up the spermatic cord, he could not find the sac there. On several occasions he had seen the hernia projecting at the external ring, so he knew there must be a sac. Therefore he extended the incision to the external ring, and was able to look inside the canal. There he found the sac, which was quite separate from the cord at its upper end, and pierced the transversalis fascia some little distance below and internal to the internal abdominal ring. If one came across an unexpected difficulty, there would be no objection to enlarging the incision and transforming the operation into the other variety. He purposely did not discuss the age for the operation. The youngest patient in his series of cases was 3 months old, which simply meant that was the youngest patient he had had with the condition since he commenced to use this method. A more interesting point was the oldest age at which the operation could be done

His oldest patient was 12 years of age, and he would not hesitate to treat in the same way patients several years older even than that. In adults, in selected cases, he had incised the external oblique, and by making a larger incision down to, but not injuring, the external ring, he could readily put in deep stitches between the conjoined tendon and Poupart's ligament. He had not said much about treatment by trusses. In very small infants he considered that it was good to support the hernia by a soft truss until the child was older. Any hernia seen over 12 months of age was unlikely to be cured by a truss, and should be operated upon.

Section for the Study of Disease in Children.

February 23, 1912.

Dr. G. A. SUTHERLAND, President of the Section, in the Chair.

Athetoid Movements.

By JAMES TAYLOR, M.D.

A GIRL, aged 5½ years. Seventh child; the first two died a few hours after birth; third, fourth, fifth and sixth, alive and well. Patient born at full term; was born "feet first," and was "black and blue" when born. Never walked or crept even. At the age of 2 years movements of head and hands and also of feet noticed. Now she speaks badly (she never spoke well), but her intelligence is fair. She has involuntary athetoid movements of hands, also of head; legs are stiff. The reflexes are difficult to obtain on account of the rigidity, but the knee-jerks are present and the plantars are flexor.

DISCUSSION.

Dr. TAYLOR said that the question of interest in all such cases was the nature of the lesion. There was a general consensus of opinion that the chief lesion probably was in the optic thalamus. What the exact nature of the disease was he did not know. In many cases of this kind there was a history of difficulty in labour, and this child was born cyanosed, so that there was reason to suppose that the lesion might be a hæmorrhagic one, and the symptoms would be explicable by injury, either to the optic thalamus itself or to the connexions between that and the cortex, resulting in the want of co-ordination between the cortical and lower structures.

Dr. LEONARD GUTHRIE said these cases showing athetoid movements were of interest because of the doubt as to their pathology. For many years it had been supposed that they were connected with disease of the optic thalamus, and Dr. Taylor suggested that the movements were due to uncontrolled action on the part of the cortex. But according to recent investigations by Head

and Gordon Holmes, a different aspect could be put on the question. These authorities seemed to show that athetoid movements occurring in disease of the optic thalamus were really the primitive normal movements of the intrinsic organ, only they were cut off from cortical control. That view was opposed to the one stated by Dr. Taylor. Head and Holmes found that lesions, particularly in the lateral nucleus of the optic thalamus, cut off all the cortical paths which impinged on the optic thalamus, the result being that the optic thalamus could exercise uncontrolled reactions in response to stimuli. Therefore it was open to question whether athetoid movements were due to cortical irritability or to want of cortical control over the thalamus. It seemed likely that the explanation advanced by Head and Holmes was the correct one.

Dr. HUTCHISON said he would have thought that the extensive paralysis in the legs of this child indicated a widespread cortical lesion; and this would accord with Dr. Guthrie's statement of the explanation of Head and Holmes, rather than that of Dr. Taylor. It was difficult to see how a lesion of one optic thalamus could affect both legs as was the case here.

The PRESIDENT (Dr. G. A. Sutherland) remarked that it was not implied that there was no cortical lesion in this case. He was struck by the microcephalic appearance of the skull, and thought there must be deficient development of the cerebral lobes. He asked whether Dr. Taylor regarded the condition as due to imperfect development of the cerebral lobes, or to meningeal hæmorrhage at birth.

Dr. JAMES TAYLOR, in reply, agreed that there were probably extensive changes in the case, both in the cerebral cortex and in the basal ganglia; but he wished to direct special attention to the athetoid movements with reference to the ætiology. In reply to Dr. Hutchison, there was no doubt that in this case there was a bilateral lesion, and that if one optic thalamus was affected the other was also. His view was that both optic thalami and cortices were involved. Another interesting fact was that although the legs were very spastic there was a good flexor response from each sole. Such cases were of great interest, especially in regard to the function of certain parts of the brain.

Paralysis of the Muscles of the Neck (? Poliomyelitis).

By R. HUTCHISON, M.D.

A BOY, aged $2\frac{1}{2}$ years. Admitted to the London Hospital on October 28, 1911. Ten days before admission had "feverish attack," and both ears discharged. Weakness of neck noticed since. Was in hospital six weeks before that for pneumonia. On admission was no

more able to hold up his head than a newly born baby. Paralysis seemed to involve both sternomastoids, both trapezii, and retrocolic muscles. It was more marked on the left side. No other muscles affected. Skiagram of spine negative.

On February 6, 1912, there was imperfect R.D. in the trapezii and sternomastoids. Other muscles could not be satisfactorily tested. The paralysis is now much less marked than when the patient first came under observation.

DISCUSSION.

Dr. HUTCHISON added that it was difficult to arrive at any other diagnosis than the one he had suggested, but there were certain difficulties. The mode of onset was not typical, although it occurred at the time of year when poliomyelitis was most common—namely, late summer. But it began in a deceptive way, with feverishness and a discharge from the ears. The paralysis was now much less than two or three months ago. He showed the case in order to have his diagnosis either confirmed or refuted.

Dr. F. PARKES WEBER remarked that this localization was rare for acute anterior poliomyelitis, but he noticed a recent French paper in which two cases of acute anterior poliomyelitis (in brother and sister) were described. In one of them (a girl, aged 1½ years) the localization of the paralysis in the muscles of the head and neck was a striking feature, but muscles of the limbs were likewise affected.[1]

Dr. JAMES TAYLOR said there was no doubt that in the initial condition of infantile paralysis one saw the neck muscles affected. He remembered seeing a case of the kind in which the paralysis was very profound at first, so that the child was unable to move its head from side to side, or to lift it; and if it were raised a little by the hand, unless the hand continued to support it, it went backwards in the same way as in the present patient. In that case it was obvious that the paralysis affected both sides. But that, as well as the paralysis of the upper limbs, cleared up completely, and the paralysis that was permanent was confined to the thigh on one side and the leg on the other. So it was easy to conceive that in any particular case there might be a paralysis limited, as it had been in Dr. Hutchison's case, to the muscles of the neck on one side. In rare cases of infantile paralysis the abdominal muscles might be affected on one side. He did not think one could imagine any other than the suggested pathology for this case, unless one assumed an actual lesion of the nerves, the spinal accessory and the nerve supply of the retrocollic muscles, as a local condition. He regarded Dr. Hutchison's view as the more likely one.

[1] Lemoine, *Gaz. des praticiens*, Paris, Oct. 1, 1911; *Brit. Med. Journ.*, Epitome, February 3, 1912, Abstract No. 50.

Case of Hysterical Vomiting and Achylia.

By R. Hutchison, M.D.

A girl, aged 10 years. About two years ago patient's mother died suddenly. Vomiting set in immediately after this event, and continued in spite of treatment in two hospitals until she came under observation



Case of hysterical vomiting and achylia.

three months ago. She was then much emaciated (weight 1½ st.), the skin dry, brown, and scaly. No visceral disease. Bowels somewhat loose. Test meal showed complete achylia.

Under treatment by isolation, suggestion, and the use of hydrochloric acid and pepsin the vomiting has entirely ceased, but the nutrition has not much improved.

DISCUSSION.

Dr. HUTCHISON added that since he came to the meeting he had discovered that the child had already been twice shown in that room. But the case was worth showing again, as changes had occurred in the condition of the patient. The vomiting began after the sudden death of the child's mother to whom she was much attached, and continued for two years. She was a Jewess, and hysterical vomiting was more frequent in members of that race than among others. The girl had been treated twice in hospitals previously, once in Great Ormond Street Children's Hospital, and once in the Victoria Hospital, but without much benefit. She was then, as now, much emaciated. He had no difficulty in stopping the vomiting by isolation and the use of the stomach-tube once or twice and by moral suasion. The vomiting had not recurred. On giving a test meal there was complete achylia, and on testing again some weeks afterwards there was no change in this respect. The suggestion he made was that it resulted from atrophy of the mucous membrane of the stomach following prolonged inanition. It was comparable with the condition met with in marasmic babies after prolonged diarrhœa, and in which post mortem the bowel was found to be atrophic. Possibly the same condition was present in the stomach in this case. On discovering the achylia, hydrochloric acid and pepsin were given, but the result was disappointing, for she did not grow and gain weight. He thought one might fairly say that not only had she had hysterical vomiting, but that she was passing into a condition of infantilism with chronic inanition, comparable to the pancreatic infantilism described by Byrom Bramwell, the intestinal infantilism of Herter, and which might occur in some degree in any child who was habitually starved.

Dr. CHARLES W. CHAPMAN suggested, as a help to nourishment, that coconut oil should be rubbed all over the child's body while other measures were being tried. It should be done night and morning in front of a fire in cold weather.

Dr. ERIC PRITCHARD remarked that Dr. Hutchison did not call attention to the fact that before the child was taken into hospital she suffered from prolonged diarrhœa and intense pain in the lower part of abdomen, symptoms suggesting the possibility of enteritis or colitis. The notes showed that at one time the motions consisted largely of mucus, and sometimes blood, and such a condition would be improved on the treatment by pepsin and other gastric digestives. Hysterical vomiting was a somewhat bold diagnosis in a child of this age, especially as the patient might be the subject of chronic bowel trouble, such as was met with occasionally without obvious symptoms pointing to bowel trouble. He believed the pancreas must be very much at fault because of its deprivation of the natural stimulants to secretion.

The PRESIDENT said it was an uncommon kind of case; and hysterical vomiting coming on at 8 years of age and persisting for two years required some consideration. At the Brighton meeting of the Society for the Study of

Disease in Children a similar case was shown, and the general idea was that it was anorexia nervosa, and that the child required feeding up. But the end came shortly afterwards, and a cerebral tumour was found post mortem. When hysteria was suggested in any case so young, great care should be taken first to exclude every other possible diagnosis. He remembered a case of cyclical vomiting in a child who was much reduced in condition. Despite the hospital methods of treatment, and various forms of diet, the child continued to lose weight and the vomiting persisted. He then stopped the dieting, and put the child on a course of isolation, massage, and feeding up, with the result that about 2 st. in weight was gained and the vomiting ceased. The explanation of these cases was still uncertain. With regard to achylia, there was a tendency to teach that the stomach was a somewhat superfluous organ, and that the food could be well digested without it. If this were true he did not see why the achylia should result in so much wasting as this child showed. He hoped the case would be shown again later, after further treatment had been pursued.

Dr. THURSFIELD remembered a case he saw some years ago, in which the wasting was almost as extreme as in this case. The patient was a girl, aged 10 years. It extended over many months, and its nature was finally solved by the child in one of her vomiting attacks bringing up a large round worm. She then got rapidly well. He suggested that in the present case the blood should be tested for eosinophilia, as there might be intestinal infection. Hysterical affections, however, in children under 10 years were not so uncommon that one need hesitate to accept that diagnosis as a cause of wasting and vomiting.

Dr. WILLIAM EWART asked whether the term hysterical was used here in a specific sense, or in a general metaphorical acceptation, and merely to imply that it was "functional," or at any rate not traced to any structural causation. It was desirable to make a strict distinction between any undiagnosed and assumedly functional cases, and the strictly so-called hysterical, first for the sake of the patient, but also for that of the advancement of knowledge. He was satisfied that in the past many cases described loosely as "hysterical vomiting" were due to unrecognized, but recognizable, physical conditions. Many years ago the idea that defective teeth were the cause of many ailments brought down upon him some harmless ridicule. But clinical operation has now changed. The worst of his series of cases of that sort was that of a young woman who had gradually reached an advanced stage of dilapidation under the diagnosis of "anorexia nervosa." The recognition and appropriate treatment of an extensive dental caries led to a rapid recovery without any remaining trace of the alleged neurosis. These obvious cases had ceased to be overlooked. But the same caution applied to the possible influence of unsuspected abnormalities lower down in the alimentary tract, or indeed of reflex agency in any other region.

Dr. HUTCHISON, in reply, said that he had not used oil inunction in this case, but he knew that the friends had rubbed in cod-liver oil, for the child smelt of it when she first came to hospital. Criticisms had been quite fairly

directed against his diagnosis of hysteria. He used the term in no Pickwickian sense, but in the ordinary sense in which it was used in medicine. He defended his diagnosis on two grounds: that the vomiting began after a moral shock, which was the usual starting-point of all hysterical vomiting, and that it disappeared under treatment of a kind which could be described as treatment by suggestion. He could not reconcile those two facts with anything but hysteria. Dr. Eric Pritchard laid stress on the intestinal condition. Remembering the achylia, he would have been surprised if the child had not had some secondary intestinal catarrh, but the diarrhœa had certainly not been sufficient to interfere with her nutrition, and undoubtedly there might be extreme emaciation in cases of pure achylia. The vomiting in this patient tended to occur in the middle of the meal, and it only occurred in certain circumstances. If the meal was taken in bed the child was sick, but if allowed to have the food at the nurse's table, or if she were given bribes, the vomiting was controlled. Her stomach had been washed out once, and she did not like it, and a threat to repeat it resulted in absence of vomiting. All these facts pointed to the condition being hysterical. He agreed with Dr. Thursfield that hysterical manifestations were not very uncommon below 10 years of age; he had seen in quite young children hysterical cough, hysterical tachypnœa and hysterical fits. He had not had a blood count taken, but he believed santonin was given to see if the child had worms. But even if worms were present he did not think they would account for the whole of the clinical picture.

Green Teeth, subsequent to a Prolonged Jaundice in the First Weeks of Life.

By H. THURSFIELD, M.D.

THE boy was first seen at the age of 3 weeks. The history was that he had been jaundiced from birth, or possibly from the third day of life only, and that during the first week he had passed very black stools, and had a purulent discharge from the navel. When seen the umbilicus was perfectly healthy; the liver and spleen were not enlarged, and the child, though small—6 lb. 5 oz.—seemed quite healthy. The jaundice was at this time deep, and remained so for the next seven weeks, slowly disappearing. When it had gone the boy put on weight rapidly and at 4 months of age weighed 9 lb. He was not seen again till he was 9 months old, when he was brought again for an attack of diarrhœa. The two lower central incisors were then a vivid yellow tint, which has become now green. The tint varies considerably; is occasionally quite bright, at other times dull.

Dr. THURSFIELD added that he had hoped that Mr. James would have been present to report on the anatomical factors and the laying down of the dentine. Mr. James said there was no difficulty in believing that the bile-pigments were absorbed into the dentine in the first weeks of life. Experiments made with madder in the case of young pigs had resulted in producing pigmented teeth. The teeth were now of a less vivid colour than when the patient was first seen.

Anomalous Jaundice, with Enlargement of Liver and Spleen, and Bile-stained Teeth.

By FREDERICK LANGMEAD, M.D.

C. M., AGED 1 year 9 months. The second child, the elder being alive and well. Jaundice began between two and three weeks after birth and persisted until the baby was 1 year 3 months old. It gradually increased for the first two months of life, and the baby remained deeply jaundiced for twelve months. When first seen it was 3 weeks old. The liver was definitely enlarged, and the spleen reached down nearly to the umbilicus. The urine was bile-stained. The child's general condition was good. There was nothing to suggest syphilis. It was conjectured that congenital atresia of the bile-ducts was present, since the jaundice started when the child was about 2 weeks old, and was steadily deepening. However, acting on the principle that a syphilitic icterus was the only variety which was remediable, grey powder was administered. The liver and spleen both subsided rapidly and were not enlarged after about one month's treatment. The jaundice persisted and was accompanied by hæmorrhage from the bowel and stomach and beneath the skin. The first tooth erupted when the baby was 1 year old, and four were visible when the jaundice disappeared at 15 months. All were bright yellow in colour, obviously pigmented by bile. Since then, each tooth as it has erupted has proved to be jaundiced. The yellow colour in them began to change to green about three months ago, and now the coloration has almost gone. Wassermann's reaction was not tested for.

It is not generally recognized that jaundice may affect non-erupted and erupting teeth; perhaps, like the pigmentation of brain and cord which Schmorl has described, it is peculiar to icterus neonatorum.

DISCUSSION.

Mr. HOPEWELL-SMITH said it was a coincidence to have two cases with green teeth shown at the Section simultaneously, as he regarded the condition as distinctly rare. It was possible to stain the developing teeth of small animals by feeding them on madder. He thought the origin of the colour of the two present cases was different, as in Dr. Thursfield's case the colour was duller than in the other. In Dr. Thursfield's case he thought the colour was due to chromogenic bacteria, like the *Bacillus fluorescens motilis*, becoming deposited on the enamel cuticle or membrane. In Dr. Langmead's case the colour seemed to be beneath the enamel and the dentine, and was perhaps due to the blood circulating in the pulp being charged with bile-pigment. He had seen pink teeth, following a hæmatogenous pigmentation through rupture of the vessels after a blow, and oxyhæmoglobin passing through the tubes. Some of the rodents had red enamel, but the explanation in these instances should be sought from the physiologist and not from the pathologist; of course the causes were entirely different from those under discussion.

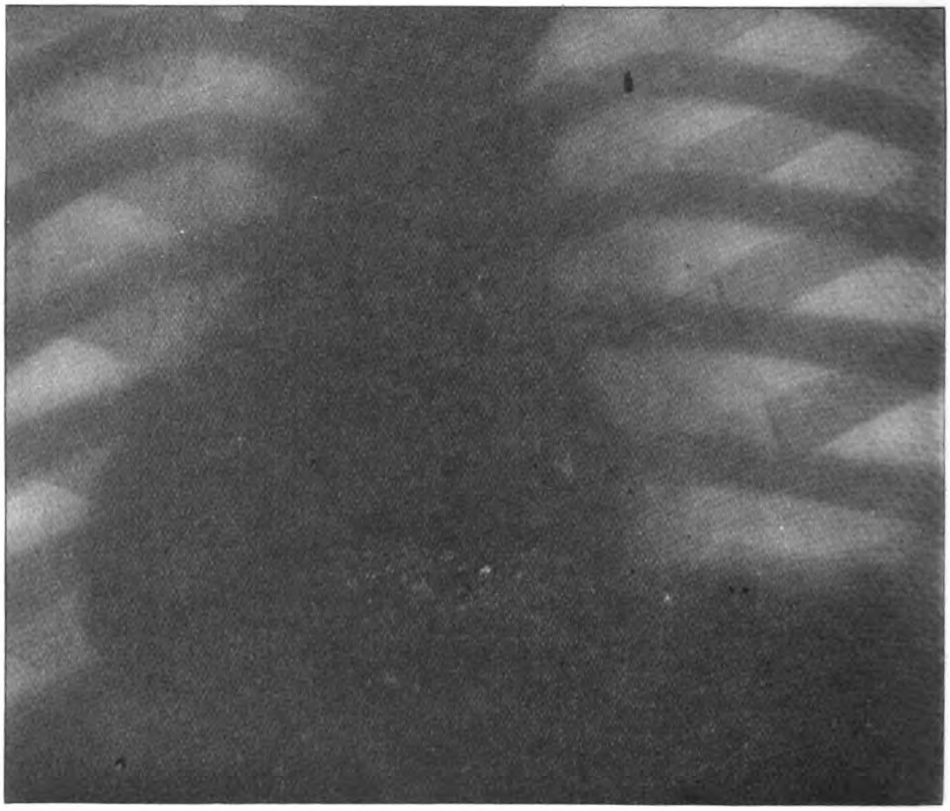
Dr. ERIC PRITCHARD remarked on the change of colour from yellow to green, a phenomenon which he did not think could be due to bacterial operations. It was not likely there were two sets of bacteria at work, one producing yellow and the other producing green; possibly the change in colour was due to the change in the reaction of the fluids to which the teeth were exposed, that they changed later to green owing to the alkaline state of the mouth with the establishment of salivary secretion. Therefore it would be interesting to hear at what date saliva was freely secreted in the mouth, and at what date the reaction of the mouth became alkaline.

Dr. PARKES WEBER remarked that Dr. Langmead said "It is not generally recognized that jaundice may affect non-erupted and erupting teeth; perhaps, like the pigmentation of brain and cord which Schmorl has described, it is peculiar to icterus neonatorum." The cerebrospinal fluid was coloured yellow in certain cases of icterus neonatorum, but that was probably explained by the presence of the condition described in Germany as "Kern-Ikterus," necrotic changes in the grey matter (of the basal ganglia of the brain, &c.) giving rise to imbibition of bile-pigment and consequent staining of the cerebrospinal fluid as well as of the necrotic tissue. In the same way salicylic acid (given by the mouth) could sometimes be detected in the cerebrospinal fluid. In regard to the green teeth in both Dr. Thursfield's and Dr. Langmead's cases, the coloration (at first yellow and then green) was evidently due to bile-pigment. It seemed natural that the pigment should be retained in non-vascular structures much longer than in structures devoid of blood-vessels. At the time of the jaundice the teeth were of course in much closer connexion with the blood-stream, and consequently were able to take up the bile-pigment.

Transposition of Viscera in a Girl, aged 12 Years.

By Leonard Guthrie, M.D.

The patient has complained of shortness of breath which prevents her from taking part in school games. She is fairly well developed, and has had no serious illness. The heart as seen in the skiagram is displaced



Skiagram showing dextrocardia.

to the right side, the apex is in the sixth space an inch inside and below the right nipple. A faint systolic bruit is heard at the base. Hepatic dullness is absent on the right side but can be made out on the left side from the level of the sixth rib downwards. Stomach resonance is well defined in the right hypochondriac region. The spleen is not palpable. There are no signs of pulmonary disease, past or present. The case is regarded as one of primary dextrocardia with transposition of viscera. Cyanosis is absent, and the finger-tips are not clubbed.

DISCUSSION.

Dr. GUTHRIE added that he did not regard the heart as malformed, but that it was a case of pure freak in position. One theory was that cases of transposition of viscera were examples of survival of one of twin monsters, though he did not know of any reason for this assumption.

Mr. PHILIP TURNER said that a few months ago he had a similar case in a boy, aged 10 years, who was admitted to hospital for operation on hernia; during the routine examination it was found that the cardiac impulse was to the right of the sternum. Nothing further abnormal was found in the chest; there was an excellent entry of air, and no sign or history of pulmonary disease. The liver dullness was on the left side, and stomach dullness on the right. The patient took the anæsthetic well, and after recovery from the operation a bismuth meal was given and a radiograph taken. That showed that the stomach was on the right side, and that the duodenum passed to the left. The cæcum was on the left side. The peculiar condition had no effect on his general health, and there was no bruit.

Dr. WILLIAM EWART elicited the fact that the patient was right-handed, and likewise also Mr. Philip Turner's patient. He stated that the case had afforded him for the first time the valuable opportunity of verifying by the test of *reversal*, and of demonstrating to two Fellows of the Society who witnessed his examination, the correctness of the *dorsal dullnesses* special to the various organs, of which the original account was published in 1899,[1] and was confirmed and completed in a comprehensive review of the subject of "Dorsal Percussion" in a paper read before the Royal Society of Medicine in June, 1910.[2] That verification was worth placing on record, although it would be regarded as superfluous by those conversant with the daily practice of dorsal plexigraphy. Much greater importance, however, attached to the satisfactory test which he had been able to apply in this case to the interpretation of two recent dorsal percussion signs, the "*Gastric Nucleus of Resonance* in the Back," and the "Upper-dorsal Paraspinal Dullness of the Minerbis." A "Stomach Sign in the Thorax," had been fully described in the paper quoted under the name of the "Gastric Nucleus of Resonance," as a circular resonance, about 3½ in. in diameter in the adult, easily localized by the inferior angle of the scapula which overlapped its upper part; and evidence had been furnished for the conclusion that its production was due to the upward transmission through the left hepatic lobe and the diaphragm of the resonant vibrations from the "Magenblase" or stomach balloon. Had it been due to a pulmonary resonance merely conducted along the ribs, the same thoracic conduction might be expected to persist in spite of the migration of the three-lobed lung into the left chest. It was found, however, that the gastric nucleus wandered into the right chest

[1] *Brit. Med. Journ.*, 1899, ii, p. 1167; and *Lancet*, 1899, ii, p. 261.

[2] *Proceedings*, 1910, iii (Med. Sect.), pp. 211-40.

with the transposed viscera. This identified it with a visceral rather than a parietal causation. A similar reasoning applied to the percussion result in this case in connexion with the dullness recently described by Cesare and Giacomo Minerbi of Ferrara, in the *Rivista Critica di Clinica Medica*.[1] Their interpretation of it would be fully considered at another place. Suffice it to say that it was ascribed by them to a visceral, not a parietal origin, namely to the arch of the aorta on the left side, and to the arch of the vena azygos major on the right. The dullness which they depicted at the side of the third dorsal spine was bilateral, but not quite symmetrical, either in degree or in extent of surface, the left section of it being larger and more definitely dull than the right, a point upon which he could agree with them. Here again the test of visceral transposition shed welcome light. The peculiarities of that asymmetry were reversed in confirmation of the main conclusion. This did not affect the questions as to the topographical features, the nature, and the precise causation of the dullness. These were so many points for a searching analysis, the results of which must be deferred for separate publication. As these transpositions were apt to pass unobserved in the ordinary routine of practice, and as they were so important in connexion with the diagnosis of individual clinical states, and moreover so valuable for the elucidation of a variety of clinical problems, it might not be inopportune to add to these remarks a practical suggestion. In all cases in which it was recognized, and particularly in early life, the existence of the peculiarity should be notified to its bearers, with the injunction that, whensoever in the future they might be seeking medical advice, they should invariably begin by informing the medical attendant that they were "Right-hearted, with transposed organs."

Dr. THURSFIELD thought the cause of the transpositions was that the primitive tube ordinarily took a twist, and that the budding-off of the various organs from that tube located the liver on one side, and the stomach and the spleen on the other, with the heart towards the left. If the twist were reversed in any case, transposition occurred.

Dr. GUTHRIE, in reply, said there was a systolic bruit at the base, but he did not consider it important, nor due to organic disease. The child was sent from school because she could not freely take part in the games, but he failed to make out that the child had anything but good health. She was right-handed. He believed the causation suggested by Dr. Thursfield was one generally accepted, but he did not know why the opposite twist should occur so very rarely if that were the explanation.

[1] Anno xii, N. 50 e 51. Firenze, 1911.

Case of Unusual Cardiac Bruit.

By J. A. TORRENS, M.B.

E. C., AGED 16, was admitted to St. George's Hospital under the care of Dr. Rolleston for influenza, when the cardiac murmur was discovered.

History: There is no history of rheumatism and no history of any cardiac disability at any time.

Present state: Well-nourished, well-developed girl. No cyanosis, clubbing of fingers, or dyspnœa. Heart: The heart is not enlarged to percussion, but the X-rays show a globular left ventricle of greater density than normal, suggesting some pure hypertrophy. The action is forcible and regular. There is a loud systolic murmur at the base, best heard over the aortic area, conducted up into the arteries of the neck, but audible over the entire thorax, back and front. On the back the murmur is loudest opposite the third dorsal spine, 2 in. from the middle line on the left side. The murmur is weakest at the apex of the heart. The second sound is everywhere quite distinct, and there is no diastolic murmur. Pulse: The pulse, though not collapsing, is not particularly well sustained.

DISCUSSION.

Dr. TORRENS added that the blood-pressure was 150 mm., which was unusual for a girl aged 16 years. He regarded it as a genuine case of slight aortic stenosis, probably congenital, not acquired.

Dr. C. W. CHAPMAN said that with regard to the bruit being audible over a large area, he had often pointed out that one could frequently hear bruits right down to the sacrum in children. It was probably a case of aortic stenosis.

Dr. THURSFIELD remarked that the child was admitted for influenza, which could produce a pericarditis comparatively innocuous, from which patients got well, but which left them with murmurs. His suggestion was that at some time the child had influenza, or scarlet fever, or a pneumococcal infection, with pericarditis, and that her symptoms were due to pericardial adhesions, near the base of the heart.

Dr. WHIPHAM said he thought that the murmur resulting from pericarditis was presystolic in time, and he did not hear such a murmur in this case. He doubted the existence of adherent pericardium and agreed that the case was one of congenital aortic stenosis.

Dr. PARKES WEBER regarded the case as a typical one of aortic stenosis. The murmur was characteristic in its situation and was conveyed along the great arteries. He did not agree that the stenosis was probably congenital. There were certainly a few cases which had been proved to have been examples of congenital aortic stenosis, but they were very rare. Perhaps this case was acquired from an attack of rheumatism, which disease was sometimes overlooked in children. Possibly what was thought to be influenzal pain was really pain due to rheumatism. There was likewise a history of an earlier painful illness.

Dr. TORRENS, in reply, said the diagnosis of influenza rested on rather insufficient grounds, for she recovered without treatment in twenty-four hours. He would have thought that pure aortic stenosis occurring in a patient so young as the result of rheumatism, without accompanying aortic regurgitation or mitral disease, was almost as rare as was congenital aortic stenosis.

Arthritis of the Shoulder and Hip (? Tuberculous).

By T. R. WHIPHAM, M.D.

THE patient is a boy, aged 14 years. When 3 years old he was held up by the arms, and after that his right shoulder "grew out" and the arm could not be raised above the horizontal level. Wasting of the shoulder muscles occurred, but nothing further was noticed until nine months ago, when he started walking lame on the left leg. There was pain in the left hip and knee at first, but this seems to have subsided and the patient has at no time been prevented from doing his work as house-boy. The right shoulder-joint is completely ankylosed and the muscles of the shoulder-girdle and arm are much wasted. There is no reaction of degeneration, and sensation is perfect. A skiagram shows complete absorption of the head of the humerus with displacement of the end of the shaft under the tip of the coracoid process of the scapula (fig. 1). The pelvis is elevated on the left side and the great trochanter of the femur is more prominent and at a higher level than on the right. The movement in the joint is considerably limited and there is evident shortening of the limb. The glutei and leg muscles are definitely wasted. A skiagram shows complete absorption of the head of the femur with elevation of the rest of the bone and erosion of the edge of the acetabulum (fig. 2). No abnormalities in other joints can be seen by means of the X-rays. Lately the patient has become somewhat deaf. There is discharge from both ears and

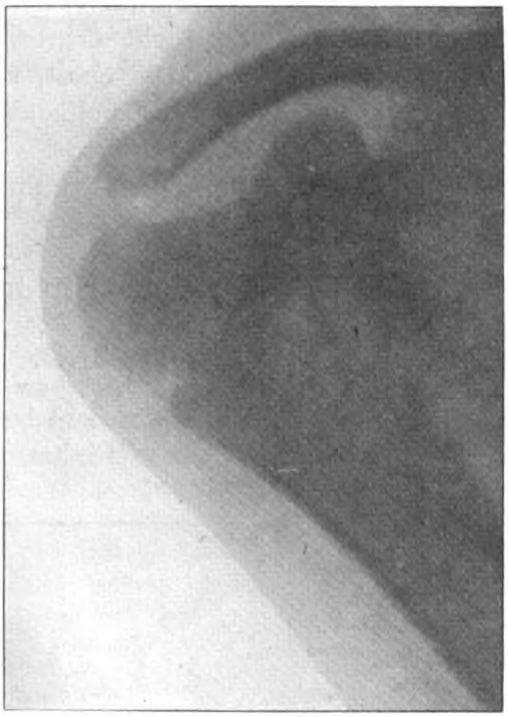


FIG. 1.

Skiagram of the shoulder-joint.

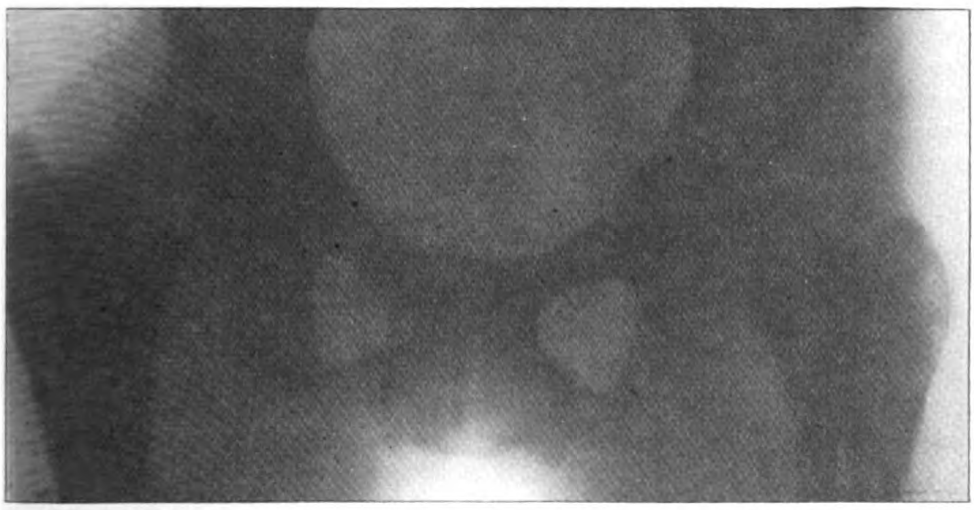


FIG. 2.

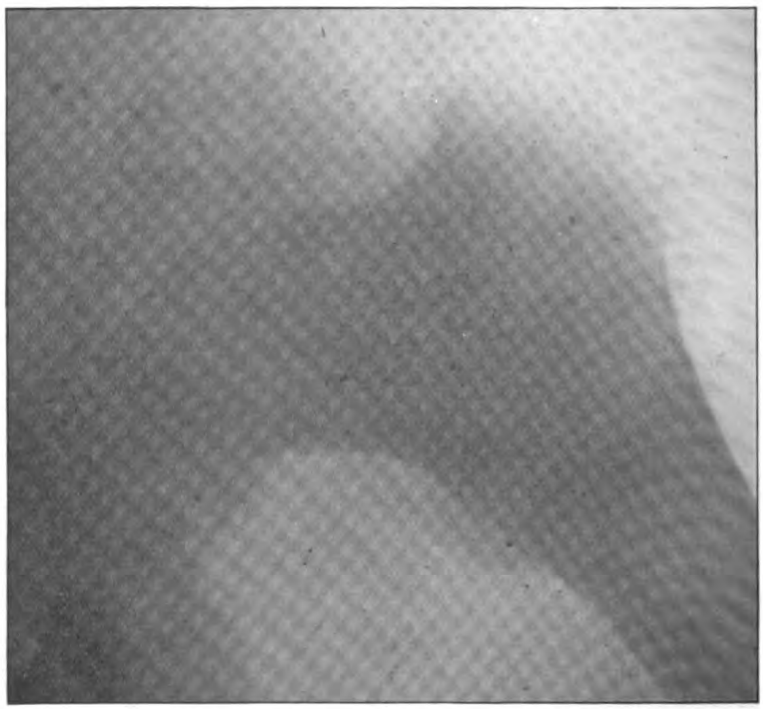
Skiagram of the pelvis.

the left tympanum is perforated; the septum nasi is deflected to the left and adenoids are present to a slight degree. There are no symptoms or signs of tuberculosis in the chest, and von Pirquet's reaction is negative. There is a history of pulmonary tuberculosis in the mother, who died at the age of 36 years. Five brothers or sisters died in infancy, but four others are alive and well.

Proliferative Osteo-arthritis of the Hip in a Youth.

By T. R. Whipham, M.D.

A year ago, at the age of 17 years, the patient began to experience pain in the left groin when walking. At first it was of the nature of a



Skiagram of the left hip-joint.

"pin-prick," but subsequently it has become more marked; it is only present when the joint is exercised. The patient walks with a limp on the left leg, which is $\frac{1}{2}$ in. to $\frac{3}{4}$ in. shorter than its fellow. The glutei and leg muscles are wasted and the greater trochanter of the femur is

very prominent. In the hip-joint the movements of flexion and abduction are limited and on rotation the greater trochanter approximates to the anterior superior spine of the ilium, indicating a certain degree of coxa vara. The movements are accompanied by creaking of the joint. A skiagram (*see* figure) shows extensive proliferation and lipping of the bones at the hip, together with a thickening of the neck of the femur and a reduction of the angle which it makes with the shaft.

DISCUSSION.

Dr. WHIPHAM regarded the first case as tuberculous, and considered that the condition was one of dry caries. He asked if it was advisable that the patient should have rest and extension applied. The patient did not seem to have got much worse clinically while doing his work as a house-boy during the last nine months, though possibly the pathological lesion had advanced. The second case was the antithesis of the first as it showed proliferation at the hip-joint, whereas the other showed absorption. He believed that true proliferative osteo-arthritis was rare in childhood, though cases had been reported even before the age of puberty.

Mr. SIDNEY BOYD agreed that in the first case both the joints were tuberculous. The case was a good example of the extent to which the disease might go in the shoulder-joint with scarcely any symptoms. The joint was probably fixed by tonic contraction of muscles, but from the movement of the scapula on the chest wall there was so much movement that the patient was not aware of the extensive trouble in the joint. He had not previously seen a case where there was so much disease at the hip with the patient still able to go about. If there was any doubt as to whether the disease was still active, he would suggest Wright's method of taking the opsonic index after a few days' rest and again after active movement, to see if there was any negative phase. In this case it was necessary to perform active movements, because one could not apply Bier's bandage to a hip-joint. The idea of movement was simply to produce auto-inoculation.

Mr. JOCELYN SWAN, referring to the first case, said he thought it was remarkable that there should have been so much bone destruction with so few clinical symptoms. One could feel much thickening of the bone and the upper end of the shaft, as well as the destruction in the head of the bone. Movement took place practically only by the movements of the scapula. The boy had very little pain, which was unusual for tuberculous joint. He thought the hip-joint of this case was also tuberculous and spoke of the active bony changes in both the head of the femur and in the acetabulum as shown in the skiagram. With regard to the second case, he suggested that the proliferative osteo-arthritis was infective in nature. The bad teeth might be the cause of the trouble.

Mr. H. S. CLOGG said he thought there was very little doubt that in the first case both the shoulder and the hip were tuberculous. With regard to the hip, he would not wait for the result of the opsonic index but would rest the joint at once. The history was of nine months' duration, and a disease which caused absorption of bone to the extent as shown in the skiagram in that time must be active, or have left the bone in a very softened condition. The X-ray photograph showed that there was considerable destruction in the margin of the acetabulum, and if the boy were allowed to walk about there would be produced more shortening than at present, and it was very important to limit the degree of shortening. He advocated the fixation of the hip-joint until the hip and acetabulum were firmly united by bone. The boy would then require some variety of osteotomy. With regard to the second case, he (Mr. Clogg) regarded it as an example of infective arthritis. The only primary focus of infection which he could discover was in the mouth. There was considerable periodontal disease, and he suggested this should be treated with a view to arresting the disease.

Mr. PHILIP TURNER thought the first case was tuberculous. He had seen several similar cases, in one of which there was also much absorption of bone in a short time, the boy meantime continuing with his work. Afterwards two cold abscesses appeared, one in front of the joint and one behind. In this case he agreed that the tuberculous process was active.

Dr. THURSFIELD suggested that tuberculin should be given subcutaneously. That was a much more satisfactory method of arriving at a diagnosis than the method of the opsonic index.

Section for the Study of Disease in Children.

March 22, 1912.

Dr. G. A. SUTHERLAND, President of the Section, in the Chair.

Leucodermia and Premature Canities.

By J. L. BUNCH, M.D.

THE patient first attended the Queen's Hospital for Children in January, 1909, when a girl aged 11 years. Some twelve months previously she had developed some round or oval white patches on her chest and shoulders and these had extended and increased in number, spreading also on to her neck and limbs, during the succeeding months. Her mother denied that the appearance of the white patches had been preceded by any hyper-pigmentation, or that any increase of pigment had been followed by atrophy of pigment in the affected areas. When seen by me, the child appeared to be healthy, and only complained of the disfigurement caused by the leucodermic patches. These were very numerous and extensive; many of them were irregular in outline, milky-white in colour, with a convex edge, and surrounded by a concave pigmented border. The patches were not elevated and to the touch no difference from the surrounding healthy skin could be detected. No subjective symptoms were present; the irides were not affected.

In addition to the numerous leucodermic patches on the trunk and limbs, there was another disfigurement which was much more noticeable and of which the patient complained much more. Although there was no apparent change in the pigmentation of the skin of the scalp, there was over the right frontal region a considerable quantity of quite white hair, the depigmentation extending right away down to the roots, and this white hair contrasted sharply, however the hair was dressed, with the rest of the child's yellowish hair. The scalp over this area did

not appear to differ either to sight or touch in any way from the other parts of the scalp. There was nothing pointing to a syphilitic history; no history of any injury, burn, or nervous affection, and no hereditary predisposition.

Although at this age such cases are nearly always progressive, the leucodermic areas have in this child become much less marked and diminished in size, and the white hair has already acquired a considerable amount of pigment.



Case of leucodermia. (Photograph taken in 1909.)

Dr. BUNCH added that during the last twelve months the lesions seemed to have become almost arrested, the only change in that time having been that the round margins of the patches seemed to have become still more sharply defined. He did not know whether to credit the change to the treatment by progressively increasing doses of liquor arsenicalis. Cases of arsenical pigmentation were familiar to all, and some of the Fellows had seen a well-marked example of the kind at the meeting of the Dermatological Section on the previous day, but the pigmented patches seen in such cases differed considerably from the general gradual increase in pigment seen in this case. The pathology of these cases was never very clear and there was nothing in the history of this child to throw any light upon the causation of the disease.

A Case of Purulent Pericarditis.

By J. PORTER PARKINSON, M.D., and DOUGLAS DREW, F.R.C.S.

THIS boy, aged 4½ years, had an attack of pneumonia involving the bases of both lungs at the end of last October. The urine contained blood, pus, albumin and epithelial and blood casts with many pneumococci. The pneumonia ran an ordinary course, the temperature becoming normal by November 6th, and the signs in the right lung clearing up. The pulse-rate, however, remained the same and the heart sounds became muffled, but there was no friction or increase of the cardiac dullness. After a few days this latter sign appeared, till finally there was dullness up to the first rib and increase to the right. On November 13 there was swelling of the face and some oedema over the præcordium. The next day Mr. Drew excised 2 in. of the fifth costal cartilage, and evacuated 10 oz. of pus which contained pneumococci in pure culture. The recovery was uneventful, the signs of consolidation at the left base disappeared, and the urine became normal by November 22. There are now no signs of enlargement of the heart either clinically or by the X-rays.

This case is interesting on account of the exceptionally good result of treatment. The boy now shows no sign of cardiac embarrassment, and the heart moves to and fro when the patient is put on one or the other side. Exploration of the pericardium in my opinion ought not to be done with the needle, owing to the difficulty of diagnosis of fluid by physical signs. It seems better to open it deliberately, and in that way evacuate its content, than run the risk of puncturing the heart. In this case the diagnosis was fairly clear before operation, but that is frequently not the case.

DISCUSSION.

Dr. PARKINSON added that there were signs of consolidation at the left base as well as at the right. Those signs persisted until the pericardial fluid was removed. This impairment of the percussion note behind owing to pericardial fluid was sometimes a quite early sign, though it was well known to occur when the fluid was large in quantity. He objected to the practice of putting a needle into the pericardium to make or confirm the diagnosis of the presence of purulent pericarditis. In one case he had reason to feel pleased

that he did not explore, as when Mr. Drew cut down on it the heart was dilated and found to be universally adherent. If one turned the child from side to side and the impulse of the heart was found to have shifted its position, there was some mobility inside the sac; this sign was present in the patient shown.

Mr. DREW said he thought such cases were rare, as in thirteen years' experience at a children's hospital he had only operated on two. He was not referring to pericarditis as part of a general pyæmic condition, which cases were not amenable to surgery. It was, he believed, generally held that after pericarditis, whether purulent or otherwise, universal adhesion of the pericardium occurred. But after an attack of suppurative peritonitis there might not be more than a few web-like adhesions, and in many cases of pericarditis possibly the same thing might occur; adhesions need not necessarily follow. He agreed that exploring the pericardium with a needle was a dangerous procedure. With regard to drainage of pyo-pericardium, some years ago Mr. Steward showed a case at the Clinical Section in which he had operated posteriorly through an empyema, draining by the same route. That was the most favourable position, but it could not be done under ordinary circumstances. In the case now shown, though there was much pus, the only evidence of it was œdema over the front of the chest.

Mr. PHILIP TURNER said one of the chief points of interest to the surgeon in purulent pericarditis was the best method of draining the pericardium. A few months ago he was asked by Dr. Hertz to operate on a case of pyo-pericardium in a boy, aged 8 years. But it was, unfortunately, some days before the parents would consent to operation. He made an incision along the seventh costal cartilage, close to the sternum, removing an inch of it, separating the structures attached, as was done in excising a rib for empyema. The pericardium, which bulged into the wound, was incised; much pus escaped, probably 10 oz. He was then able to introduce a tube into the great longitudinal sinus behind the heart. When the pericardium was exposed by excising the fifth costal cartilage there was difficulty in draining this pouch, but the drainage in the present case must have been satisfactory, as it was also in Dr. Hertz's case. The patient lived altogether for four or five weeks, death being due to myocarditis, leading to cardiac failure. He had only operated upon one other case of purulent pericarditis. The child was very ill, its condition being almost hopeless, and it only survived the operation for a short time.

Dr. ERIC PRITCHARD thought it possible that the signs of consolidation might have been due to massive collapse owing to diaphragmatic incompetence, in the same way as signs were found after paralysis of the diaphragm, or after operations on the abdomen in which the diaphragm was held in a position of quiescence. The rapid way in which the consolidation cleared up was suggestive that the condition was due to diaphragmatic paralysis rather than to direct pressure of the pericardial sac.

Dr. HERTZ, referring to his case, which Mr. Turner had already remarked upon, said that the most striking symptom present was one not mentioned in the account of the present case, namely, very well marked tenderness over the whole area of cardiac dullness and also in the epigastrium. There was a bulging in the upper part of the epigastrium, as the diaphragm was pushed down by the collection of pus in the pericardium. They were so certain about the diagnosis that no exploration was made with a needle, but Mr. Turner operated as soon as permission was obtained. He agreed with what had been said about drainage, because, after death, which occurred four or five weeks after the operation, there was no pus present, and it was clear that death had been due to the associated myocarditis. Although there was much fibrin still left between the heart and parietal pericardium, there was no evidence of organized adhesions.

Mr. NORBURY remarked that puncturing the heart with a needle was sometimes done for resuscitation in cardiac failure under an anæsthetic. In a case at St. Thomas's Hospital a man was having his leg put up into plaster, under chloroform, when he stopped breathing and his heart stopped beating. After various efforts to restore him had failed, a needle was plunged between the ribs into the heart and moved about: the heart commenced to beat and the man did well.

The PRESIDENT (Dr. G. A. Sutherland) said Dr. Parkinson was to be congratulated on his diagnosis in the case. Apparently there was nothing so definite as in Dr. Hertz's case, but Dr. Parkinson mentioned œdema over the præcordium. He did not know whether Dr. Parkinson meant to make a distinction between purulent pericarditis and cases of pericarditis due to other causes. He himself did not think there was any special risk in puncturing the pericardium; he had done it in several cases, and usually in the axilla. It was perhaps a little alarming at the first experience, when one felt the heart scraping against the end of the needle. In purulent pericarditis there was great difficulty in diagnosis, and the patients were so very young—it being most common under 12 months of age—that as a rule the affection was not even suspected.

Dr. PARKINSON, in reply, said he was referring to purulent or serous collections. In his experience it had not been necessary to withdraw the fluid in a serous case; in the majority of cases it disappeared gradually under treatment. The signs of pneumonia on the right side disappeared ten days before those on the left side, and that was against the idea that it was due to a weakness of the diaphragm. He did not lay stress on the child having acute nephritis, but that added to the severity of the prognosis. When nephritis was present as a complication of pneumonia it usually cleared up absolutely shortly after the fever disappeared.

Chronic Jaundice and Splenomegaly.

By LEONARD GUTHRIE, M.D.

THE patient is a girl, aged 6 years. Eighteen months ago she became jaundiced. Icterus has varied in intensity from time to time, but has never disappeared entirely. The urine is sometimes dark in colour but usually pale, the stools are said to be always dark brown. She often complains of feeling sick, but seems well on the whole.

Past illnesses: Varicella and pertussis at 3 years, "rheumatic fever" at 5 years, and morbilli at 5½ years, followed by chorea. Has always been troubled by thread-worms.

Family history: Parents, and brother and sister, aged 10 and 13 years, are in good health. No miscarriages. No children have died.

Present condition: Fairly well nourished blonde. On admission to hospital, December 11, 1911, the skin, mucous membranes, and conjunctivæ were of bright canary-yellow colour. Bowels constipated, motions dark, containing much mucus, but no oxyurides. Urine pale, no bile pigment, acid, 1015, no albumin. Heart and lungs normal. Liver not felt, area of dullness normal. Spleen easily palpable, extending 2 in. below costal margin. Notch not felt.

Blood examination: Serum is deep yellow coloured. Reds, 4,190,000; whites, 10,200; hæmoglobin, 80 per cent. Differential count: Polymorphonuclears, 76 per cent.; large lymphocytes, 4·5 per cent.; small lymphocytes, 17 per cent.; eosinophiles, 2·5 per cent.

Remarks.—During three months' observation, the icterus has varied greatly in intensity but has never quite disappeared. The motions are always dark, and the urine has never contained bile pigment. The spleen is still enlarged. The area of hepatic dullness is normal. The probable diagnosis is: "Toxic hepatic cirrhosis with splenomegaly."

DISCUSSION.

Dr. GUTHRIE added that Dr. Perkins, the Pathologist at the Paddington Green Children's Hospital, examined the blood to ascertain the resistance of the red cells to saline solution, and found it slightly lowered. Hæmolysis took place under a 0·227 per cent. saline solution, whereas the control was 0·175 per cent. With regard to the diagnosis, in some respects the condition resembled acholuric jaundice—the acquired type. But against that must be set the blood count. In acholuric jaundice there were certain distinct cytological changes, including the presence of megaloblasts and normoblasts, and an alteration in the shape of the cells. It seemed to be due to some defect in the bone-marrow and in the

blood-forming organs, and the jaundice was due to the fragility of the red corpuscles. In acholuric jaundice urobilin was found in the urine, and that was a measure of the breaking up of the red cells. Examination here did not show urobilin, but that examination had only been made once, and possibly further examination might show its presence. The case might turn out to be one of cirrhosis. Ten years ago Dr. Parkes Weber showed a girl, aged 12 years, who had splenomegaly and recurrent jaundice, but no bilirubin in the urine. He believed she was admitted six years later to the German Hospital with ascites, from which she died, and she was found to have cirrhosis of the liver. Perhaps this was in store for the present patient.

Dr. F. PARKES WEBER agreed that this case was likely later on to develop symptoms of hepatic cirrhosis, and that it did not seem to be of the class of familial "chronic acholuric jaundice with splenomegaly and anæmia," that is to say, so-called familial "hæmolytic icterus." Possibly the jaundice would ultimately diminish, and the case might then come to resemble one of Banti's disease. He asked whether inquiry had been made as to the possible presence of a taint of congenital syphilis. Even if no ordinary evidence could be obtained, the Wassermann reaction should be tried. Some cases of congenital syphilis with splenomegaly came perhaps ultimately to resemble the symptom-complex of Banti's disease.

Dr. GUTHRIE, in reply, said he could not discover any signs or suggestive evidence of congenital syphilis in the child, and there was no family history pointing to it. He would have a Wassermann test done. In Dr. Parkes Weber's case the condition of the liver was not such as one would expect to find in a specific case. In November he (Dr. Guthrie) recorded the case of a girl who had many points of resemblance to the present patient. In her the Wassermann reaction and the von Pirquet reaction were positive, but she had none of the stigmata of congenital syphilis.[1]

Addendum (April 11, 1912).—Subsequent examination of the urine by Dr. H. Perkins showed presence of urobilin and urobilinogen by chemical and spectroscopic tests, but no bilirubin. The case may therefore be regarded as one of "acquired acholuric jaundice," without characteristic blood changes. Wassermann's reaction is negative.

Bilateral Deltoid Paralysis.

By G. A. SUTHERLAND, M.D.

FEMALE child, aged 1 year. The child appeared to be healthy until four months ago, when she had an illness of an indefinite character lasting for a fortnight. Since then there has been little movement about the shoulder-joints, the patient being able to move the forearms

[1] "Recurrent Jaundice, Pyrexia, Splenomegaly, Anæmia, and Pigmentation of the Skin of a Girl, aged 11 Years," *Practitioner*, 1911, lxxxvii, p. 791.

and hands freely. She is fat and flabby generally. The left deltoid muscle appears to be completely paralysed and the right retains very little power. Definite wasting cannot be determined owing to the amount of superjacent fat. The bones and joints in the neighbourhood appear to be normal. There is no rigidity or tenderness. All the other muscles of the extremities are very flabby, but there is no evidence of paralysis elsewhere.

Case of Cerebral Palsy.

By G. A. SUTHERLAND, M.D.

E. C., MALE, aged 1 year. Born at full time; first child; normal labour without instruments. Child weighed 9 lb. Breast-fed for six weeks, then cows' milk and barley water. Became a fat child. At the age of 7 months began to have screaming attacks, drawing up his legs and twitching of head and eyes. At the age of 10 months had a series of general convulsions, lasting for two days, about nine each day. Since then has had an occasional general convulsion. Child is very fat and flabby. Face and head look large viewed from the front, but there is much flattening in the antero-posterior diameter, producing a brachycephalic condition. Constant jerking movements of the head, trunk and extremities take place, spasmodic and purposeless. He takes no notice of what is going on, seldom cries, and never smiles. He is unable to sit up, or to support himself sitting up, or to balance his head. The pupils react to light, and there is no ocular paralysis or nystagmus. The fundi are normal. Vision is apparently present, but hearing seems absent.

Case of "Subacute Arthritis of Shoulder-joint" (due to an Organism of the Bacillus Enteritidis Type).

By LIONEL E. C. NORBURY, F.R.C.S.

T. L., MALE, aged 11 months, admitted to the Belgrave Hospital for Children on August 11, 1911, with a history of a fall and (?) injury to left shoulder four weeks previously. Shoulder noticed to be swollen one week later. Patient treated for "pleurisy" three weeks before coming to hospital.

On examination: Signs of resolving pneumonia at left base; no pyrexia, no cough. Slight swelling of left shoulder-joint. Treated as an out-patient for eight days. Swelling of joint gradually increased; painful; limitation of movement.

On admission: Temperature 99·4° F. Considerable effusion into shoulder-joint. Diagnosis of subacute arthritis, probably pneumococcal in origin.

Arthrotomy by anterior incision. Turbid fluid containing deposits of lymph evacuated. Irrigation of joint with normal saline solution. Suture; no drainage. Temperature fell to 96° F. on following day, rising to 100·8° F. and 101·4° F. on second day, after which it remained subnormal. Diarrhœa with green stools on second day after operation, and off and on for several days. Sutures removed on seventh day; wound almost healed, very slight discharge. Movements at joint good.

Patient examined on March 19, 1912: General condition good; movements at shoulder-joint quite free.

Report on fluid taken from joint at time of operation: "Gram-negative motile bacillus present, which does not liquefy gelatine, and ferments (acid and gas) glucose, galactose, maltose, mannite, dulcite, and sorbite, and does not ferment lactose, saccharose, raffinose, dextrin, and glycerine; indol is not produced. It therefore belongs to a large group of organisms of *Bacillus enteritidis* type, which includes *Bacillus enteritidis* (Gaertner), *Bacillus paratyphosus*, &c.; it does not include the organism described as Morgan's No. 1, which is frequently found in cases of diarrhœa in children."

DISCUSSION.

Mr. NORBURY said that the interest of the case was in the association of pneumonia, arthritis, and diarrhœa, all coming on one after the other. It would be interesting to know if the pneumonia was due to the same organism. It was presumed that the diarrhœa was, but that was not certain. Another point was as to the possibility of washing out these joints and sewing them up without drainage. Some cases healed up by first intention and no adhesions remained.

Mr. DOUGLAS DREW said he thought all cases of mild septic joints in young children were best dealt with by aspiration and washing out the joint. He had had many cases which healed without arthrotomy; but if arthrotomy was performed it was better to wash out the joint and close it without drainage. Even if drainage was necessary, and even when the infection was very virulent, the cases nearly always recovered with movable joints.

Mr. DUNCAN FITZWILLIAMS said this was the first case he had known in which that organism was present. It was possible that that organism, as well

as the pneumococcus, might have been circulating in the blood, and the pneumonic organism, which was of low vitality, may have died out and given place to the organism found at the operation.

Dr. CAUTLEY recalled one of the early cases in which a paratyphoid organism was found in the joint; he would supply the particulars to Mr. Norbury.

Dr. MIDELTON said he had put forward a theory that the joint cavities were used, in cases of of infection, as receptacles. He regarded abscesses as artificial cavities, but joints were ready-made cavities. He was interested in the suggestion that one bacillus killed another. He had long held that staphylococci and streptococci killed other organisms.

Mr. NORBURY, in reply, thanked Dr. Cautley for his reference to the paratyphoid case. He presumed treatment by aspiration should be done only where the pus was very thin. It would not do good if there was much lymph about; it would then be better to open the joint and wash it out.

Congenital Deformities in the Lower Limb.

By DUNCAN C. L. FITZWILLIAMS, F.R.C.S.

A BOY, aged 3 years, who exhibits in his left lower limb the following congenital deformities: Dislocation of the hip, dislocation of the knee, and such severe degree of talipes as almost to warrant the description of dislocation of the ankle and transverse tarsal joints. The head of the femur is well up on the dorsum ilii, almost as high as the anterior superior iliac spine. The femur is much shorter than its fellow, though it is impossible to measure accurately the amount of the difference, the left knee when sitting reaches to about the junction of the middle with the lower third of the right femur. At the knee the head of the tibia is displaced forwards and slightly outwards, so that the condyles of the femur can easily be felt in the popliteal space and the finger pushed into the intercondylar notch. The ligaments of the joint are very loose, so that the relative position of the bones can be altered at will; hyperextension is allowed to about an angle of 45°. There seems to be no patella. The foot is turned inwards and upwards to such an extent that the inner side of the foot lies along and in contact with the inner side of the leg, the toes pointing towards the knee. The os calcis appears to be quite on its side, as the underpart of the heel looks inwards. Both bones of the leg are present, but it is difficult to make out the condition of the internal malleolus. The musculature of the limb is poor, but the child can walk, the outer part of the head of the astragalus being put to the ground: over this area a large bursa has developed.

The child first came under my care about two years ago with a strangulated inguinal hernia on the right side, but little could be done for the limb at that time as the parents neglected and rejected offers of treatment. Later a charitably inclined person lured them away just when they had consented to allow something to be done. I therefore did not see them again till this afternoon when the child was brought up, worse in every way than when I saw her last. Nothing has been done and the growth of the limb has not kept pace with the growth of the rest of the body, so that it is relatively shorter than formerly.

I propose to replace the hip and try at the same time to straighten the foot by removal of the astragalus: later an arthrodesis of the knee will probably have to be done. In dealing with the foot I think, considering the shortness of the limb, that it will be a good plan to retain the foot in a condition of equinus.

Congenital Dyschezia.

By ARTHUR F. HERTZ, M.D.

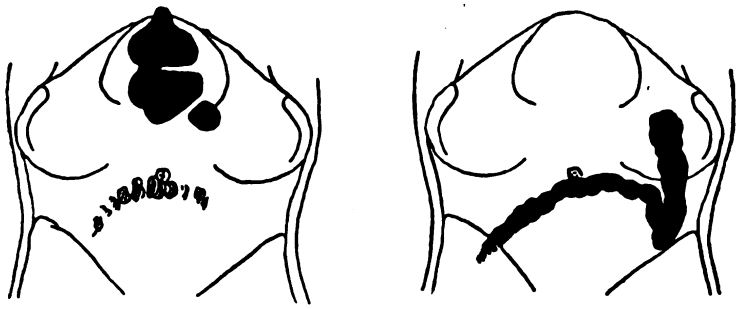
OWING to the acute angle formed at the pelvi-rectal flexure the passage of fæces along the intestines is obstructed at this point. Consequently the pelvic colon becomes filled with fæces from below upwards and the rectum remains empty until immediately before defecation. The entry of fæces into the rectum gives rise in new-born infants to reflex defecation, and in older children and adults to the sensation of fullness, which is the natural "call to defecation." The passage of fæces from the pelvic colon into the rectum is the result of active peristalsis in the former, brought about reflexly by various stimuli, the chief of which is the taking of food into the empty stomach. In my investigations with Mr. H. W. Barber and Mr. K. H. Digby we found that the rectum is insensitive to tactile and chemical stimulation, and that the call to defecation is a form of muscle-sense, depending upon the distension of the rectum, which occurs as soon as fæces pass beyond the pelvi-rectal flexure [1]. If a response is not at once made to the call to defecation the desire passes away. This is not due, as has been supposed, to the fæces being carried back into the pelvic colon by anti-peristalsis, but to the relaxation of tone, which occurs in the muscular coat of the rectum after it has been subjected to a certain degree of tension for a short period. The call to defæcation only returns after a further quantity of fæces has entered the rectum and produced a rise in the intrarectal pressure. As I first

pointed out in a communication to the Medical Section in February, 1908, all cases of constipation can be divided into two classes: in the first, which may be called *intestinal constipation*, the passage of fæces through the intestines is delayed, whilst defecation is normal; in the second class, for which I adopted the name *dyschezia*, there is no delay in the arrival of fæces in the pelvic colon, though their final expulsion is not adequately performed. It is extremely important to recognize these two classes of constipation, as their treatment is entirely different: diet, abdominal massage and aperients, which are appropriate for intestinal constipation, are quite useless in dyschezia, attention to the hygiene of the bowels and re-education of the defecation reflex by means of graduated enemata being the correct treatment.

I wish to-day to draw the attention of this Section to a class of dyschezia, which depends upon a congenital deficiency of the muscle-sense of the rectum. In a mild form it is not uncommon in infants, in whom the slight distension produced by the introduction of the finger or a piece of soap into the rectum results in an adequate stimulus. In the majority of cases the muscle-sense develops as the infant grows older, but congenital deficiency is occasionally the starting-point of dyschezia which lasts through life. This is particularly likely to occur if such infants are treated by aperients, which only hasten the passage of fæces through the small and large intestines, where no delay exists, and do not influence the rectum, which is the true seat of the complaint.

The condition can be recognized by making repeated digital examinations, when it is found that the rectum is constantly filled with fæces, even immediately after the bowels have been opened. In many cases the loss of sensibility of the rectum is further shown by the fact that the child does not cry or offer any resistance during rectal examination. Dyschezia should always be suspected in severe cases of constipation in infants and children, when ordinary methods of treatment by diet, aperients and abdominal massage have failed. Thus one of the worst cases I have seen was in a girl, aged 5 years, who was sent to me by a physician to one of the children's hospitals; he had found that treatment in the hospital for thirty-two weeks with laxatives, agar-agar, petroleum and electrical massage produced no improvement, but that glycerine and water enemata caused the bowels to open. In such cases it is often thought that when treatment suitable for intestinal constipation is given regularly, whilst enemata are given once or twice a week, the former prepares the way for the latter by hastening the arrival of the fæces in the rectum. As a matter of fact,

the special diet, drugs and massage are quite unnecessary, for the natural action of the bowels is generally sufficient to bring the fæces to the rectum, the enemata acting just as well without any additional treatment. In the case just referred to a slow but steady improvement has resulted from the daily use of glycerine enemata, graduated in the manner to be described presently, no other treatment being employed. In mild cases the bowels may open when aperients are given alone, as fluid fæces enter the anal canal, which may retain its tactile sensibility and so may be the starting-point of the defæcation reflex. Such treatment, however, is very undesirable, because the bowels only act when the stools are fluid, much water and nutrient material being consequently lost. In doubtful cases an X-ray examination is helpful, and it was by this means that I learned to recognize the condition. Between ½ oz. and 2 oz. of bismuth oxychloride, according to the age of



(*a*) Ten hours after bismuth meal.

(*b*) Thirty-four hours after bismuth meal; rectum full of bismuth-containing fæces, but no desire to defecate.

the child, are given in milk or porridge, the bowels having previously been emptied by means of enemata. Dr. H. Semon, in an unpublished investigation carried out at my suggestion, found that the rate of passage through the intestines in infants is about the same as that of adults, four hours being required to reach the cæcum, six the hepatic flexure, ten the splenic flexure, and twelve the pelvic colon. In congenital dyschezia the pelvic colon is reached in the normal time, and in twenty-four hours almost all the bismuth is collected in the distended rectum. This is well seen in the diagram, which is reproduced from tracings taken from the case of a girl, aged 8 years, who had suffered from extreme constipation from birth, and had recently been given an infusion of twenty to thirty senna pods every night without result.

The dyschezia soon leads to secondary retention of fæces in the pelvic colon and in severe cases in still higher parts of the large intestine, as, unless enemata are given, the rectum is never empty, and in spite of its

dilated condition there is insufficient room for all the retained fæces. The irritation caused by the retained fæces is likely to give rise to catarrhal colitis, and in the girl aged 8 years with congenital dyschezia, to whom I have already referred, retention occurred as far back as the cæcum, giving rise during the last four years to repeated attacks of typhlitis, with pain, tenderness, vomiting and pyrexia, which were at first diagnosed as appendicitis and were only recognized to be something different when five further attacks occurred after the removal of the appendix eighteen months ago. X-ray examination after the colon had been emptied showed that there was no delay in the passage of fæces as far as the rectum, but that severe dyschezia was present.

Treatment.—The child should take an ordinary diet, and neither aperients nor abdominal massage are required. When the stools are so hard that defecation is rendered painful and difficult, a little liquid paraffin should at first be given. The child should sit on a chamber for at least ten minutes every morning after breakfast and try to open his bowels, whether he feels the desire or not. If the attempt fails, as it probably will for some time, he should be given either a water or glycerine enema, after which he should repeat the attempt in the same position. In the majority of cases treatment by glycerine enemata is most effective: 1 oz. of glycerine is given the first day; the next day ½ dr. of the glycerine is replaced by water; the third day 1 dr., and so on, the glycerine being made more and more dilute until finally it is all replaced by water, which can then also be dispensed with. In some cases water enemata act better: 1 oz. is required for a new-born infant, 6 oz. for children a year old, and a pint for children aged 8 years. It should be introduced from a funnel at a pressure not greater than 18 in. through a tube inserted no further than just beyond the anal canal. The amount used is reduced by one-twentieth part at a time until no more is required. In many cases the substitution of glycerine by water and the diminution in the amount of water used must be very slow and may have to be prolonged over weeks or even months. I believe, however, that in all cases in children a cure eventually results, but if the condition is allowed to continue until adult life it may, in rare instances, be necessary to use enemata permanently. If the injection is given under low pressure and the glycerine is as dilute as possible and as little water used as possible, the enemata never lose their effect.

REFERENCE.

[1] Hertz, A. F. "Goulstonian Lectures on the Sensibility of the Alimentary Canal in Health and Disease," Oxford, 1911.

DISCUSSION.

The PRESIDENT (Dr. G. A. Sutherland) said members would agree with the two varieties of constipation which the author insisted on—namely, pure intestinal constipation, and the form to which he had given the name dyschezia —but he felt a little doubtful about the suitability of the term " congenital " as applied to dyschezia. He gathered from Dr. Hertz that the congenital condition was one of deficiency of the muscle-sense, and it occurred to him that there might be other causes for the condition besides a congenital defect. It was natural that in infants there should be little or no control of the rectum, or anything but a purely automatic reflex. But with regard to the factors which led to dyschezia, he would lay stress on the absence of proper training of the child in the performance of the act of defecation. In some cases diet had been a factor. And it was not unusual to find a child had some local irritation about the anus, such as a crack or fissure, or the presence of hard fæces. In such a case the child so much disliked making any evacuation that it would use every effort to avoid it, and that fostered the condition of dyschezia. Dr. Hertz referred to a relaxed condition of the sphincter ani, but there was another class of case with well-marked spasm of the sphincter, and that might be associated with dyschezia. One was accustomed to see intestinal constipation due to an atonic condition of the lower bowel, and one might easily assume a similar atonic condition of the rectum. One of the marked symptoms of rickets was an atonic condition affecting every part of the bowel. In recent years one had heard of imperfect co-ordination between the pylorus and the body of the stomach, and similarly there might be more or less dissociated co-ordinations between the rectal wall and the sphincter. He was not prepared entirely to accept Dr. Hertz's view that the muscle-sense was a necessary factor. Dr. Hertz said that a little glycerine and water would be sufficient to produce an evacuation of the rectum ; but he (the speaker) did not know why that should be if it were simply an absence of the muscle-sense, because there would be no distension of the rectal wall. Another difficulty he had was that simple hot-water enemata in cases of accumulations in the rectum in infants were not efficacious, though soap and water in similar quantity was satisfactory. If it were purely a matter of muscle-sense, why should plain water, which distended the bowel, be useless? The Section was indebted to Dr. Hertz for his interesting contribution.

Dr. G. W. JOHNSTONE expressed his agreement with the remarks of the President. Soap and water were practically essential in such cases. It was also very useful for accumulations in the lumen of the gut to apply the homely poultice. While he was in the Far East he had to see a case which was being treated for appendicitis. He palpated the abdomen, and found the transverse colon full of hard fæces. The patient was a student, a delicate Eurasian ; his musculature was weak, and no doubt his gut action was weak in association with it. In that case poulticing did all that was necessary.

Mr. NORBURY said a small child was sent to the hospital with a history of intestinal obstruction and an abdominal tumour. The diagnosis which had been made was "obstruction by a fibroid of the uterus." External examination, however, showed that the tumour was a mass of fæces. There was atony of the bowel; the child had not had a motion for a fortnight, yet it seemed quite well, and was not sick. Soap and water were very efficacious.

Dr. HERTZ, in reply, said he was much obliged for the criticisms which had been made, but most of them were due to a misunderstanding of his chief point. He did not say that every case of dyschezia in infants was congenital. In previous papers on the subject he had described many causes, including all those mentioned by the President, but to-day he wished to emphasize that there was in addition a type of dyschezia in infants which was of congenital origin. In all his cases the possible causes mentioned by the President had been excluded. The children were well trained with regard to their bowels, and other children in the family were not affected in the same way. He had never traced any connexion between dyschezia and the diet, because the call to defæcation was mechanical and not chemical in origin. The recollection of painful defecation in these children was ruled out as a cause, for the act was always painless, and indeed the anal canal was peculiarly insensitive when a digital examination was made. The question of muscle-sense in connexion with defecation had been fully investigated by Dr. H. W. Barber and himself in the case of adults. Investigations on intelligent men with the aid of the proctoscope and sigmoidoscope had shown that the rectal and intestinal mucous membrane was insensitive to touch and to chemical irritation—even to glycerine. But contact of glycerine with the anal mucous membrane produced a burning sensation and a desire to defecate. In all the individuals experimented on, simple mechanical distension of the rectum produced a desire to defecate. Thus it was clear that it was the distension of the rectum which caused the desire to defecate. The reason that the sensation disappeared when the fæces had been in contact with the rectum for some time was that the rectal musculature relaxed, as the intrarectal tension was found to fall at the same time. The glycerine in these cases did not act by distending the rectum, but it produced a reflex from the anal canal. He did not think the patient got well owing to the glycerine re-educating the muscle-sense so much as by its keeping the rectum empty. In fact, if the rectum was kept empty, the atony of the rectum, which was secondary to its constant distension with fæces, disappeared. It was, however, possible for a reflex to occur without any sensation being produced: thus soap might stimulate the mucous membrane of the rectum chemically and cause reflex contraction, just as hydrochloric acid did in the case of the stomach. In some of the cases he referred to he had found that water acted very well indeed, but in others glycerine acted best.

Section for the Study of Disease in Children.

April 26, 1912.

Dr. G. A. SUTHERLAND, President of the Section, in the Chair.

Bony Growth on the Skull.

By O. L. ADDISON, F.R.C.S.

I. F., FEMALE, aged 5 years. The swelling on the head was first noticed a fortnight after birth, and has gradually increased in size. There is a bony swelling the size of a pigeon's egg on the frontal bone just to the right of the middle line and continued as a ridge, gradually decreasing in size for 2 in. or more downwards and forwards to the temporal fossa. An X-ray photograph shows the swelling to consist of cancellous bone.

Mr. ADDISON added that the mother reported that the growth was enlarging, and if it were her wish he would remove it.

Exostosis of the Inner End of the Clavicle.

By O. L. ADDISON, F.R.C.S.

A. N., FEMALE, aged 11 years 9 months. The swelling, only recently noticed, is said to be getting larger. On the anterior surface of the inner end of the left clavicle is a bony swelling the size of a horse-bean. The sternomastoid muscle moves freely over the tumour. An X-ray photograph does not show the tumour, but a large cervical rib is well shown on the left side and a smaller one on the right.

DISCUSSION.

Mr. ADDISON added that the girl was brought up because of the prominence of the inner end of the clavicle, which seemed to be increasing in size; he thought it was a simple exostosis, similar to that which occurred in long bones, but the situation was unusual. Some members seemed to think there was a dislocation, but he did not agree. The cervical ribs did not cause any symptoms, and the condition was discovered when a skiagram was taken.

Mr. TUBBY said the case presented features of interest; it was similar to some cases he had seen at the Evelina Hospital for Children. At that time he considered they were cases of epiphysial enlargement, and he thought this case to be such. The projection forward which occurred was not true dislocation, but was due to subluxation of the sternal end of the clavicle from two causes. The first was relaxation of ligaments associated with hyperæmia and enlargement of epiphyses. The second was the fact of the presence in this case of a cervical rib on the same side, which helped to push the clavicle forward. He once saw an enlargement of the inner end of the clavicle in a medical man aged 52 years. By means of the X-rays it was possible to observe that the inner end of the bone was very much expanded, and he advised the patient to submit to an exploratory operation to ascertain what the cause of the expansion was. It was found to be due to a malignant growth of thyroid tissue. Subsequently death occurred from another cause but no growth whatever of the thyroid gland could be found; yet the enlargement of the clavicle was due to growth of thyroid-like tissue. With regard to the cervical ribs, he had operated on some seventeen cases of this abnormality, all but one in childhood; and this exception he expected to come for operation later. Such cases generally went on to adult life without symptoms, and then had tingling, numbness, and circulatory disturbance. A sufficiency of the rib must be removed to prevent any pressure on the nerves or obstruction of the arteries. The difficulties met with at the operation varied very much. In one of his cases the rib was situated between the nerves of the brachial plexus, and there was an aneurysmal dilatation of the artery above it. As a rule he did not attempt to remove the vertebral end of the rib because he feared trouble with the sympathetic nerve; and in the adult removal of the outer half sufficed to relieve the symptoms.

Dr. E. G. L. GOFFE suggested that the case was nothing more than dislocation of the clavicle; the condition, in his opinion, disappeared when the shoulders were brought forward.

Mr. ADDISON, in reply, said that he had not inquired into the family history. He was grateful to Mr. Tubby for his remarks, from which he understood the speaker's idea was that there was enlargement of the epiphysis, though he did not make it clear what the enlargement was due to. He did not agree that there was any general enlargement of the epiphysis; the swelling was confined to the anterior surface of the bone.

Pathological Specimen of Tumour on the Back.[1]

By O. L. ADDISON, F.R.C.S.

THE specimen is a hemispherical tumour 1½ in. in diameter. The convex surface is covered with skin. From the centre of this surface a small sinus, ½ in. in length, passes inwards, to open into a thin-walled cyst ¼ in. in diameter, containing a viscid opaque white fluid. Above this cyst is situated a larger multilocular thin-walled cyst ½ in. by ¾ in., containing the same kind of fluid. Below is an irregular plate of bone, roughly triangular in shape, 1⅛ in. in length and ⅜ in. broad at its widest part. The bone is thin at the centre, has thick, rounded margins, and is covered with a thin layer of cartilage. The whole is embedded in a mass of subcutaneous tissue.

Microscopical appearances: (*A*) The cyst wall shows (1) a thin lining of mucous membrane with a covering of columnar ciliated epithelium; (2) a submucous layer of loose connective tissue containing a few mucous glands; (3) two oval portions of hyaline cartilage; (4) an external fibrous layer. These appearances resemble those of the upper respiratory passages. (*B*) Smears from the opaque white fluid contents of the cyst show polymorphonuclear, polygonal, and columnar, ciliated, epithelial cells, with some deposit of fibrin.

Mr. ADDISON said he showed the case before the Society a year ago, the tumour having been present from birth. The bone in the centre of the specimen was attached or hinged on to the middle line by the cervical vertebræ, but there was no synovial cavity. The microscope showed that some of the cysts in the centre of the tumour were lined by ciliated columnar epithelium, and there were also in the cyst walls areas of cartilage and some mucous glands.

[1] The case was shown at the meeting of the Section on April 28, 1911. See *Proceedings*, 1911, iv, p. 165.

Destruction of the Uvula in Vincent's Angina.

By J. D. Rolleston, M.D.

Girl, aged 6 years, showing loss of uvula and anterior pillars, and portion of soft palate and tonsils. Free margin of soft palate presents depressed pale area of scar tissue. Voice nasal. No difficulty in swallowing.

Admitted to Grove Hospital on January 31, 1912, certified to be suffering from diphtheria on the seventh day of disease. Deposit on left tonsil: 8,000 units of antitoxin given.

February 1: Ulceration of left tonsil and left side of uvula. A few organisms resembling diphtheria bacilli in culture; numerous cocci. February 4: Ulceration of tonsil and uvula more marked. Vincent's organisms in smear.

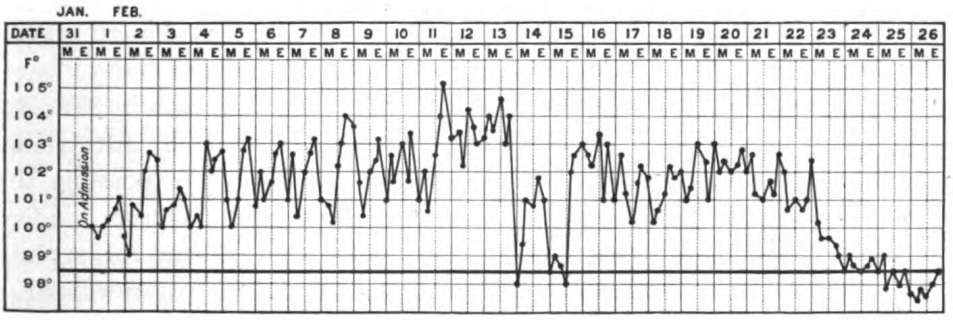
In spite of various local measures successively adopted—viz., syringing with solution of potassium chlorate and lavender, application of methylene blue powder, and painting with tincture of iodine—the ulceration advanced and was accompanied by much fœtor, dysphagia, prostration, and insomnia. From February 2 to February 14 the temperature was always above 102° F., and on February 11 was 105·2° F. (*see* chart). On February 14 the uvula was entirely destroyed. The larynx was not affected. On February 23 local and general improvement occurred and cicatrization rapidly took place. Vincent's organisms were still present in the throat smears on February 22, but none were found on March 2.

The voice long remained very indistinct and nasal, but gradually became clearer. From March 1 to March 9 there was some regurgitation, but none has been noticed since. Wassermann's reaction on March 16 (Dr. Cartwright Wood) was positive, but became negative on March 30, without anti-syphilitic treatment. Beyond a trace of albumin in the urine from February 11 to February 19, no complication occurred. The knee- and ankle-jerks remained active, and there was no sign of diphtheritic paralysis.

Discharged in good health on April 5. There is a slight degree of congenital ptosis of the left upper lid, but there is no family or personal history of syphilis.

The features of interest in the present case are: (1) The exceptional severity of the attack; (2) the behaviour of Wassermann's reaction. Vincent's angina is usually a mild affection, and readily yields to local

treatment, such as painting with tincture of iodine or applications of methylene blue powder. Local treatment in the present case proved unavailing, and improvement first seemed to begin after a good night's rest obtained by a dose of trional. The uvula is frequently involved in Vincent's angina. Thus in thirty-two cases observed by myself it was affected in twenty, but the damage was never considerable and complete regeneration of tissue always occurred. I can find only five other cases in literature in which the uvula was completely destroyed (Auché [2], Baron [3], Bruçe [4], Niedner [9], Achard and Flandin [1]). In Auché and Niedner's cases, as in my own, diphtheria bacilli were present, but their pathogenicity was not tested. I may mention, however, that diphtheria bacilli have been found in gangrenous conditions in the mouth and throat, and in such cases are usually of diminished virulence and incapable of producing the characteristic signs and



Temperature chart in case of Vincent's angina.

appearances of true diphtheria (Freymuth and Petruschky [6], Passini and Leiner [10], Sailer [14], Walsh [18]). In the present case the aggravation of the local and general condition in spite of antitoxin renders it improbable that the diphtheria bacilli present played any considerable part in the morbid process.

The term Vincent's angina has been given to the present case on account of the predominance of the fusiform bacilli and spirilla in the throat smears, but it may also be called a case of primary gangrenous angina. The great destruction of tissue, the penetrating fœtor which was much more offensive than that usually observed in Vincent's angina, the resistance to local treatment, and the grave disturbance of the general condition, certainly justify such a description. At one time death, which is the usual issue in gangrenous angina, seemed probable either from septic absorption or from involvement of the neck vessels with sudden and fatal hæmorrhage.

On the other hand, though an attempt is usually made to distinguish Vincent's angina from gangrene of the throat, there is little doubt, in my opinion, that the two conditions are closely associated. Roque [12], indeed, regards Vincent's angina as a variety of gangrene of the pharynx. In gangrenous angina, as Buday [5] and Vezprémi [17] have shown, the fusiform bacilli and spirilla of Vincent predominate, while the numerous other organisms with which they may be associated play only a subordinate part.

The presence of a positive Wassermann's reaction in Vincent's angina, apart from concomitant syphilis, has been recorded by other observers (Gerber [7], Much [8], Saverio [15]). In Much's case, examination of the blood during the febrile period gave a strongly positive reaction, but a fortnight later, when the angina was cured, the reaction was negative. On the other hand, the reaction is not invariably positive in Vincent's angina uncomplicated by syphilis, as three such cases reported by Sobernheim [16] and two by Saverio all gave a negative reaction.

In view of the successful results obtained with salvarsan in Vincent's angina by direct application (Achard and Flandin) or by intravenous or intramuscular injection (Gerber, Rumpel [13]), it is possible that salvarsan might have been of benefit in this case. Gerber, indeed, regards it as hardly less specific for Vincent's angina than for syphilis. In most cases, however, such heroic treatment as an intramuscular or intravenous injection is unnecessary, as the ordinary case of Vincent's angina readily yields to local treatment.

REFERENCES.

[1] Achard et Flandin. *Bull. et mém. Soc. méd. Hôp. de Par.*, 1911, xxxi, p. 504.
[2] Auché. *Gaz. hebd. d. Sci. méd. de Bordeaux*, 1904, xxiv, p. 555.
[3] Baron, C. *Arch. f. Kinderheilk.*, Stuttg., 1903, xxxv, p. 165.
[4] Bruce, H. W. *Lancet*, 1904, ii, p. 137.
[5] Buday, K. *Ziegler's Beitr. z. path. Anat.*, Jena, 1905, xxxviii, p. 255.
[6] Freymuth und Petruschky. *Deutsch. med. Wochenschr.*, 1898, xxiv, p. 232.
[7] Gerber. *Deutsch. med. Wochenschr.*, 1910, xxxvi, p. 2385; *Arch. f. Laryng.*, Berl., 1911, xxiv, p. 367.
[8] Much, H. *Med. Klinik.*, Berl., 1908, iv, p. 1119.
[9] Niedner. *Berl. klin. Wochenschr.*, 1908, xlv, p. 1380.
[10] Passini, F., und Leiner, C. *Wien. klin. Wochenschr.*, 1899, xii, p. 743.
[11] Rolleston, J. D. *Brit. Journ. Child. Dis.*, 1910, vii, p. 299.
[12] Roque. "Traité de Médecine et de Thérapie, Brouardel et Gilbert," 1906, xv, p. 131.
[13] Rumpel, T. *Deutsch. med. Wochenschr.*, 1910, xxxvi, p. 2286.
[14] Sailer, J. *Amer. Journ. Med. Sci.*, Philad., 1902, cxxiii, p. 59.
[15] Saverio. *Arch. ital. di otol., rinol. et laryng.*, 1910, xxi, p. 177.
[16] Sobernheim, W. *Arch. f. Laryng.*, 1909, xxi, p. 501.
[17] Vezprémi. *Centralbl. f. Bakt.*, Jena, 1907, Orig., xlv, p. 31.
[18] Walsh. *Trans. Path. Soc. Philad.*, 1900-1, n.s., iv, p. 179.

DISCUSSION.

Dr. BUNCH remarked that to him the interesting point was that the case gave a positive reaction to the Wassermann test, and that it became negative later without anti-syphilitic treatment. Dr. Rolleston did not describe in detail the Wassermann method employed; and he therefore asked whether it was the true Wassermann reaction, or whether it was a modification of it. It was now well known that in other diseases fixation of the complement could take place, and he had a paper in his possession in which it was stated that the fluid from hydatid disease gave a positive serum reaction. With regard to the successful effects of salvarsan in Vincent's angina, he asked if Dr. Rolleston knew the relationship between the spirillum found in Vincent's angina and the spirillum of syphilis.

Dr. E. G. L. GOFFE mentioned a case which came under his notice in the summer of 1911, that of a boy aged 10½ years, who was admitted to the North-Eastern Hospital, certified to be suffering from diphtheria. Spirilla and fusiform bacilli were found in smears from the throat. On five successive days cultures were made from the nose and throat, but no organisms of diphtheria nor any resembling them were found. A good deal of slough separated from the throat. The case progressed, sloughing became more marked, and the temperature rose at times to 104° F. On the tenth day of the disease it had fallen to 99° F., but the patient became gradually worse; there was general toxæmia, and death occurred on the seventeenth day of the disease, and fourteen days after admission. Before the patient died the whole of the uvula and most of the soft palate had sloughed away. Post mortem the sloughing was found to extend into all the adjacent tissues of the throat; the posterior pharyngeal wall, part of the tonsils, and the pillars of the fauces were involved. There was a little old pleurisy found in both pleuræ. Before death the condition produced a husky, croupy voice; and after death the ulceration was found to have extended into the larynx, involving the vocal cords. That case was interesting in view of the fact mentioned by Dr. Rolleston that not many cases of death from Vincent's angina had been recorded. He had seen cases recorded in which the whole of the uvula and part of the soft palate had been removed by the ulceration of Vincent's angina. The above case was the only one of death from Vincent's angina he remembered having seen.

Dr. F. PARKES WEBER wished to draw attention to the possibility of cases of destruction of the palate due to Vincent's angina coming into hospital some years later with another complaint, when the scarring and loss of substance at the fauces might easily be incorrectly accepted as evidence of past syphilis. He once saw a possible case of that kind. The history he obtained was that the patient, a man, aged 33 years, had formerly been in a fever hospital, where at first the disease had been supposed to be diphtheria, but afterwards ulceration of the fauces supervened with loss of substance, which did not heal until anti-syphilitic treatment (mercury and iodide of potassium) was adopted. Could

not the case after all have been one of "Vincent's angina," with loss of substance? As salvarsan was said by some people[1] to do good in Vincent's angina, possibly other anti-syphilitic treatment (mercury and iodide of potassium) might also sometimes be useful for the same complaint. On the other hand, the disease may have been true diphtheria followed by tertiary syphilitic ulceration; diphtheria bacilli were certainly present in the man's throat.

The PRESIDENT (Dr. G. A. Sutherland) asked if Dr. Rolleston thought the loss of the uvula and the gangrene were entirely due to the spirillum in this case; there arose the question of mixed infection.

Dr. J. D. ROLLESTON, in reply, said he thought there was probably a secondary infection. In the cultures which were taken were a number of cocci, whose exact nature he did not identify; he did not doubt they formed part of the morbid process. But the way was paved for them by the Vincent organisms. He could not give a satisfactory answer to either of the questions asked by Dr. Bunch. He could not give details of the exact Wassermann test, as it was not carried out by himself. He would ascertain from the bacteriologist what method he used. With regard to the relation of the *Spirochæta pallida* to the spirochæte of Vincent's angina, he did not know that that relationship had been worked out, but the two conditions often were associated. Vincent's angina often affected syphilitic persons owing to the *locus minoris resistentiæ* induced by the latter disease.

[*Addendum.*—Dr. Cartwright Wood has since kindly informed me that the method employed was the ordinary Wassermann reaction with the extract of syphilitic organs as antigen.—J. D. R.]

A Case of Morbus Cordis.

By F. J. POYNTON, M.D.

C. G., A BOY, aged 10 years, originally came to hospital on account of vague, aching pains "all over," with some shortness of breath on much exertion. No other symptoms of any kind. No history of frequent sore throats, rheumatic fever, chorea, or any grave chest illness. Only physical signs of any abnormal character are to be found in the heart, which is apparently displaced considerably to the right, the major portion of it lying to the right of the middle line. A systolic thrill and diastolic shock are palpable at the second right costal cartilage,

[1] On the employment of salvarsan for Vincent's angina, see Rumpel, *Münch. med. Wochenschr.*, 1910, lvii, p. 2288; Gerber, *ibid.*, p. 2385; Plaut, *ibid.*, 1911, lviii, p. 2768; and Sourdel's communication in the Société de Thérapeutique, Paris, November 8, 1911.

and here, too, upon auscultation are to be heard the only abnormal auscultatory physical signs—a rather shortened first sound followed by a systolic murmur conducted up into the neck. The second sound is accentuated and followed by a diastolic murmur conducted down the sternum to the left. The murmurs are particularly loud over the aortic region. The pulse is not "Corrigan" in type, and it is doubtful whether capillary pulsation is present in the lips. There are no other abnormal physical signs.

Skiagrams show that the heart, though to the right, is not transposed, and that there is no material enlargement of the left ventricle.

The case presents several interesting problems of diagnosis.

DISCUSSION.

Dr. POYNTON added that his suggestion was that the heart was probably congenitally misplaced. There was, he believed, a double lesion at the base, which might possibly be an acquired lesion, or it might be a congenital lesion, or possibly both congenital and acquired. He meant, for example, that the patient was born with a congenitally misplaced heart, that there was a congenital aortic lesion, and that there was some rheumatism superadded. He exhibited a skiagram showing the position of the heart.

Dr. CHARLES W. CHAPMAN said there was aortic regurgitation and mitral stenosis. He drew attention to the marked accentuation of the aortic second sound at the mid-sternal region, suggesting that found in arterio-sclerosis. Another point was that the child had lateral curvature with flattening in the dorsal region. He could not satisfy himself as to the cause of the displacement of the heart. Dr. Poynton's opinion that the cardiac condition was partly congenital and partly acquired appealed to him as the most probable explanation.

Dr. LANGMEAD considered that the ringing aortic second sound suggested a congenital lesion—i.e., that the aorta was enlarged and probably conveying most of the blood. He thought it possible that the heart was not displaced, but that the right side of the heart was enormously dilated and hypertrophied, and that there was a very small left ventricle. He thought that the blood was driven from the right ventricle into the aorta, either directly or through an imperfect septum ventriculorum, and that circulation was established through a patent ductus arteriosus.

The PRESIDENT said that Dr. Poynton would recognize the difficulty which members had in examining an unusual type of heart disease in the meeting room with all the surrounding noises. He did not himself hear some of the murmurs described by various speakers. He largely agreed with what Dr. Langmead said—namely, that it was probably a congenital lesion. He considered that there was dilatation of the right side of the heart rather than displacement of the organ.

Dr. PARKES WEBER remarked that he had succeeded in obtaining a capillary pulse on the boy's forehead, and he believed that that confirmed the evidence of aortic valve disease. He suggested that there was some congenital malposition of the heart and congenital aortic stenosis, with slight regurgitation. He hoped Dr. Poynton would lay special stress in the description on the loudness of the murmur over the aortic area.

Dr. POYNTON, in reply, said he thought the suggestions of Dr. Langmead and the President were excellent ones. His view still was that the heart was displaced, and that it was not all hypertrophy, though the right ventricle was enlarged. The skiagram did not show so much displacement as he thought clinically was the case. He would study the boy again, especially with regard to the condition of the back, which Dr. Chapman pointed out. He had been on the look-out for capillary pulsation, and it was thought to have been seen in the lips once or twice, but not on the forehead. That was in favour of it being an aortic lesion, and sometimes the murmur sounded exactly like that of a double aortic lesion. When, however, he first saw the patient he thought it was a congenital lesion of the type suggested by Dr. Langmead. An electrocardiogram had been taken of the case for him by Dr. Lewis, but it threw no light on the condition, except to show that it was not a case of transposition.

Rachitic Dwarf.

By F. J. POYNTON, M.D.

A BOY, aged 11 years 11 months. Birth: Full term; normal labour; "pigeon-chested at birth." Feeding: Breast for a fortnight, then Nestlé's milk up to one year. Early history: Much bronchitis as a baby; head sweated profusely. Walked at the age of 15 months. Grew till 4 years of age, but was quite small for his age, and was not breeched till that age. Has not grown much since. Three fits when teething; constipated as a baby. Scarlet fever at the age of 8 years; no other illness. Four sisters; neither they nor parents rickety.

Present condition: Height, 3 ft. 2 in. (should be 4 ft. 6 in.). Weight, 3 st. 3 lb. (should be 5 st. 6 lb.). Cranial circumference, 20¼ in.; skull square, not bossed. Facies not suggestive of achondroplasia. Curves of long bones exaggerated; spade-like hands and feet. Scoliosis. Beaded ribs; keeled sternum. Harrison's sulcus; angulus Ludovici prominent. Muscles very well developed. Heart and lungs normal. Liver and spleen not palpable.

DISCUSSION.

Dr. LANGMEAD did not consider the case to be essentially one of rickets. The child seemed to him to be more achondroplasic than rickety. The hands were especially characteristic of this disease. The "hypertrophic" form of achondroplasia was associated with enlargements at the epiphysial ends of bones very like those found in rickets.

Mr. W. J. MIDELTON asked whether steps had been taken to remedy the deformities in any way. He had recently had under care a patient in whom the deformity in the chest had been greatly modified by vibratory massage and Swedish exercises.

Dr. PARKES WEBER regarded the condition as one of achondroplasia, which was formerly known by the term "fœtal rickets." Both humeri and both femora were too short in comparison with the rest of the child. The spade-like hands were typical of achondroplasia, and the shape of the feet fitted in with that diagnosis. Curving of the long bones could also be associated with achondroplasia.

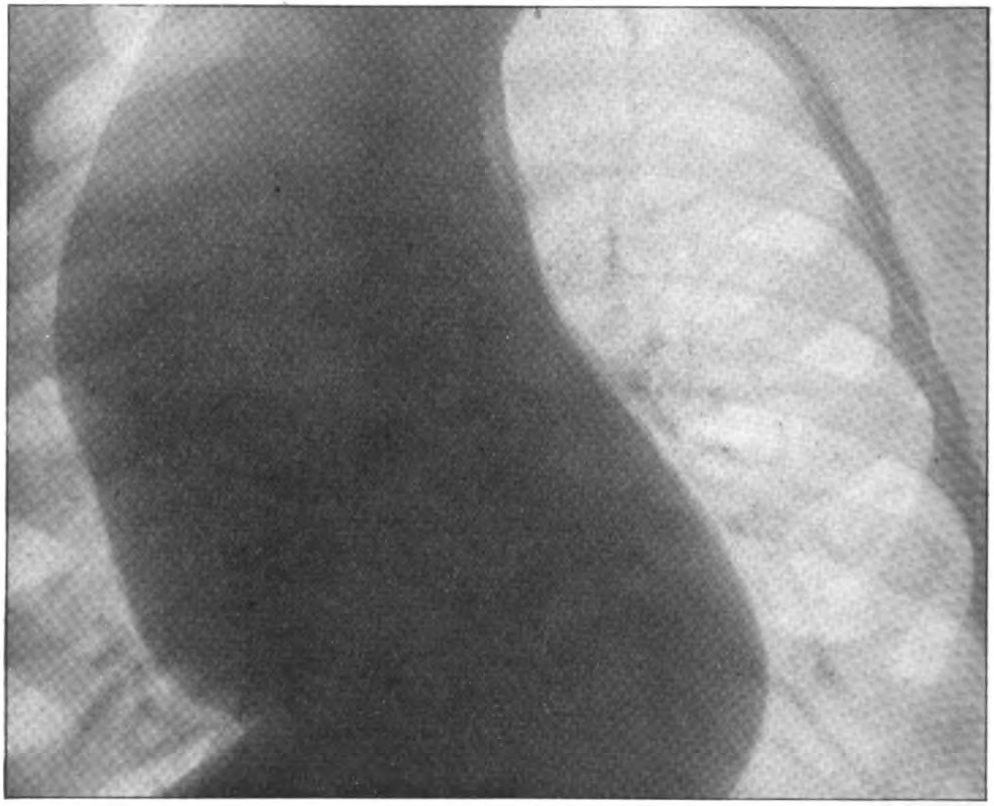
The PRESIDENT asked, with regard to the statement that the child was pigeon-breasted at birth, whether a child was ever born with a pigeon-chest like that. Mothers stated that their children were born with all sorts of conditions, such as asthma, and he thought imagination played a large part. He agreed with those who pointed out the resemblance of this case to one of achondroplasia, in favour of which was the bunching-up of the muscles, which suggested shortening of bones. Such prominence of muscles was not so marked in ordinary dwarfs as in subjects of achondroplasia. In the latter it was assumed that the muscles did not grow to exactly suit the length of the bones. The rachitic features in this case were not at present very striking, but Dr. Poynton gave a very definite history of rickets, so no doubt rickets had been a factor. There were features of both diseases in this case, but neither was fully developed.

Dr. POYNTON, in reply, said when he wrote an article on achondroplasia in Allbutt's and Rolleston's "System of Medicine," he published in that article a picture showing a combination of achondroplasia with rickets. He agreed that the present case was a very interesting one, and that there must be in it something more than rickets. He would like to hear suggestions for a better title. The head was not at all like that of an achondroplastic. The statement "pigeon-chested" at birth he put into inverted commas to indicate that it was merely the mother's unsupported statement. The changes in the bone suggested rickets, and there must be an element of that disease in the case. Perhaps it would be wiser to call the case one of "rickets with achondroplasia." The boy had only recently come under his care, and he would carry out the treatment suggested, and try to improve the condition of the chest.

Gumma of the Lung.

By D. Forsyth, M.D.

W. B., aged 10 years, is the sixth of eight children, his birth being preceded by two miscarriages. One of the children died jaundiced at six weeks, another is deformed by spinal caries. The patient himself



Skiagram of case of gumma of the lung.

had no medical history of any interest until two years ago, when he attended St. Thomas's Hospital for a swelling just above his left knee. This, after being X-rayed, was treated and disappeared, though afterwards the boy limped for a time. One year ago he developed interstitial keratitis in both eyes, the left cornea becoming permanently damaged. A couple of months ago, the keratitis recurring in the right eye, he

came under the care of Mr. McMullen at the Royal Westminster Ophthalmic Hospital, who, preparatory to injecting salvarsan, sent the child to me on account of the condition of his chest.

On examination the boy was pigeon-breasted, with the following physical signs, suggesting a solid mass in the right chest: Right chest —lateral expansion defective, percussion note impaired in first space, dull at second rib down to fourth space; resonance begins again under fifth rib, whence to liver dullness at seventh rib (nipple line) the note is resonant. Over the dull area the vesicular murmur as well as the vocal fremitus and vocal resonance are absent. Behind, the note is impaired opposite the first dorsal spine, becomes duller at the second, and resonant again at the fourth; breath sounds from the first to the eighth spine rather faint and high-pitched, though at the base they are heard better again; vocal fremitus is diminished and vocal resonance diminished and rather nasal on this side. The left lung and the heart are normal.

As the X-ray photograph by Dr. Ironside Bruce shows, the right chest contains, apparently about the hilum of the lung, a large, fairly sharply outlined mass occupying a position corresponding to the physical signs. This mass, though continuous with heart and aorta, does not pulsate nor in any way displace the heart.

The Wassermann reaction is positive.

Since coming under observation the boy has not lost weight, has had no cough and no sputum, but has had a slightly irregular temperature between 90° F. and 100° F.

DISCUSSION.

Dr. FORSYTH added that the chest had recently been re-examined by X-rays, and the shadow on the right side showed no material change. The condition had never produced and was not now producing symptoms of any sort; it was found by chance during the routine examination of the chest. The patient had been treated by salvarsan, followed by mercury and iodide of potassium, and these latter were being continued.

Dr. CHARLES W. CHAPMAN asked why the case was called one of gumma of the lung, as there were no lung symptoms, only mediastinal symptoms. He had had under his care during the last six months a lady with a similar area of dullness, which had disappeared under the administration of large doses of iodides.

Dr. FORSYTH pointed out that if the gumma were mediastinal, so large a mass would almost certainly have displaced the heart; but, since the heart was seen to be normally placed, the probability was that the gumma occupied the substance of the lung itself.

Mucous Gastritis in Infancy.

By Edmund Cautley, M.D.

In justification of the name "*mucous gastritis*" I may plead its analogy with "*mucous colitis.*" In both affections there is a profuse secretion of mucus. It is sometimes urged that mucous colitis is a nervous affection, not an inflammatory one. I do not suggest such an ætiological factor in the mucous gastritis of infancy. Sometimes the two disorders are coincident in the same infant, and in occasional patients the affection of the colon is more severe than that of the stomach, and the stools contain blood. Possibly a better name would be "subacute gastric catarrh," or "catarrhal gastritis." Pathologically there is a catarrh of the mucosa, characterized by excessive secretion of mucus. The mucosa is pale and flabby and more or less covered with mucus in the cadaver. I have brought the condition before this Section for criticism as to its nomenclature, ætiology, diagnosis, and treatment. I propose to limit my paper to the consideration of the disorder in infancy, especially in the first three months of life, for it is at this age it is most characteristic and there is serious liability to error in diagnosis. In older children it is common in mild forms, leading to various dyspeptic troubles. In infants after the first three months of life it is rarely serious, unless the child is premature or marasmic—that is, in a stage of development similar to that of a younger babe. It is not necessary for me to burden your patience with details of numerous cases. The description of a typical and severe attack will be sufficient for my purpose, but I must remind you that in a mild form the affection is of frequent occurrence.

Two years ago a female infant was sent to me from one of the suburbs as possibly a case of congenital hypertrophy of the pylorus. She was a second child and weighed 6 lb. at birth, but was said to have been a month premature. The child's weights at the end of the succeeding five weeks were 6, 6¼, 6½, 7 and 6¼ lb. respectively. For the first ten days of life she was nursed by the mother. After this the diet had consisted of peptogenic milk, until the last few days before I saw her. Progress was satisfactory for four weeks. Vomiting then began, preceded by pallor, and recurred after almost every feed. The bowels acted with the assistance of magnesia, and there had been

no special history of constipation. The attack was said to have been started by vaccination. At the age of 39 days, when I saw the child, she seemed quite bright, with a clean tongue and inoffensive breath. She vomited immediately after a feed of albulactin, the vomit containing a little mucus and having a slightly sour smell. On examination of the abdomen there was no visible peristalsis, no evidence of dilatation of the stomach, and no palpable pylorus. A diet of whey 2 dr. every quarter of an hour while awake was prescribed, the quantities to be increased to ½ oz. every half hour, 1 oz. every hour, and 2 oz. every two hours. On reaching the full feed of 2 oz. cream were to be added gradually. Cocaine, $\frac{1}{100}$ gr. hourly, was ordered.

Shortly afterwards the child's doctor informed me that progress was not satisfactory, so I advised that the diet should be changed to Allenbury No. 1 food and that the stomach should be washed out twice a day.

Three weeks after the first consultation she was brought to see me again. She had improved for a time on the Allenbury food. It then seemed to disagree, so the diet had been changed to milk diluted with four parts of water, and a small quantity of albulactin, 1 oz. being given every two hours. The vomiting still continued, unless the stomach was washed out twice daily. The stools contained a moderate amount of fæcal matter.

The child seemed more wasted, the skin of the abdomen being lax and shrivelled. She still weighed 6 lb., but had lost 5 oz., previously gained, in the last six days. Marked peristalsis of the stomach was visible. During the contraction of the stomach I thought I could feel the pylorus as a thickened cord. In view of the possibility of operative treatment being required, the patient was admitted into a nursing home and put on a diet of whey.

When seen next day the report was that there had been little vomiting, no visible gastric peristalsis, and a fairly normal stool. It was now ascertained that the vomiting was characterized by the presence of large masses of tenacious mucus, which were evacuated with great difficulty. It became more frequent, and sometimes gave rise to severe choking attacks. Frequently the vomit consisted entirely of mucus. This vomiting persisted for a considerable time. Moreover the stools occasionally contained mucus, and for some days large quantities were present in each evacuation.

After a few days' treatment by lavage the washing was discontinued as it appeared to me to be doing absolutely no good. The child then

began to improve slowly, the secretion of mucus becoming progressively less. Ten days after admission to the home she weighed 5 lb. 13 oz. In the next ten days she gained 6 oz., and 2½ oz. in another four days. During this period her food had been gradually increased, and she was discharged on a diet of cream, whey and lactose. Ten weeks later she weighed 9½ lb., and was digesting feeds of milk 3 oz., barley-water 2 oz. She occasionally vomited and brought up a little mucus.

The two striking features of this case were the enormous amount of mucus secreted and the resemblance of the condition in some respects to congenital hypertrophic stenosis of the pylorus. Thus the vomiting began in the fourth week of life, and had become progressively worse. In the sixth week peristalsis was visible, though it may have been present earlier. At this time the pylorus was palpable on one occasion, but there was obviously no complete obstruction. I thought there might be a mild degree of hypertrophy, and that the obstruction was due to secondary spasm, congested mucous membrane, or a plug of mucus. It was not until the child was in the nursing home that the excessive secretion of mucus was recognized. The vomiting was not projectile to the extent seen in typical pyloric hypertrophy, nor were several feeds retained before it occurred.

The *ætiology* of this disorder is to my mind fairly simple. These cases are comparatively rare in the breast-fed. For some reason, such as a chill or unsuitable diet, a catarrh of the gastric mucosa is set up and may become very severe. Occasionally it is due to too high a percentage of fat in the diet, and possibly it may be started by preservatives present in some creams. I feel assured that in certain instances it is due to an infective agent, notably those cases in which there is a coincident ileo-colitis or colitis. It is reasonable to suppose that malnutrition from any cause is a predisposing factor and that the disease may be a sequel of an acute gastritis. I have never seen a really severe case in a breast-fed infant. As I have already stated, the affection is most marked in the first three months of life, and is rarely severe in older infants unless they are small, premature, or marasmic. Probably the older and stronger infants, though secreting much mucus, do not vomit so readily, and pass it onward through the pylorus.

Any cause which leads to stasis of gastric contents is apt to induce the condition. Hence it may develop in the course of congenital hypertrophic stenosis of the pylorus.

The *diagnosis* is easy when the vomitus is seen. We must differ-

entiate it from other affections in which there is wasting, vomiting and constipation. I must recall to your notice valuable observations by Drs. R. Miller and W. H. Willcox[1] on "Some Gastric Conditions in Wasted Infants." These observers divided the cases clinically into three groups: (1) Atrophic dyspepsia, or pure marasmus; (2) hypertrophic pyloric stenosis; (3) pyloric spasm, without hypertrophy, or acid dyspepsia. This classification, to my mind, is too narrow and is incomplete. The following one is perhaps better.

WASTING IN INFANCY.

(1) Atrophic dyspepsia; ending in marasmus.
(2) Acid dyspepsia: (*a*) with pyloric spasm; (*b*) uncomplicated.
(3) Pyloric spasm.
(4) Mucous gastritis.
(5) Hypertrophy of the pylorus: (*a*) uncomplicated; (*b*) associated with gastric catarrh; (*c*) associated with pyloric spasm.

In simple marasmus Miller and Willcox found that there is no retention of food in the stomach and no mucin. The secretion of acid is diminished, and ferment activity is low. The tongue is furred, and there is a tendency to diarrhœa and vomiting. I must add that there is sometimes associated gastric catarrh and secretion of mucus.

In acid dyspepsia, or pyloric spasm, there is retention of stomach contents, no mucin, an increased acidity, and normal or decreased ferment activity (Miller and Willcox). Pyloric spasm, in my opinion, can occur independently of acid dyspepsia. It is certainly true that acid dyspepsia can occur without pyloric spasm, and the addition of spasm gives rise to confusing symptoms. Thus, the vomiting may be as explosive as in pyloric hypertrophy. It is apt to occur after each feed, and it is unusual for several feeds to be retained. Hence, dilatation of the stomach is slight or absent, and peristalsis is ill-marked and infrequent. Constipation is neither extreme nor persistent, and the child does not waste rapidly. The tongue tends to be clean. A pyloric tumour, if palpable, varies in size under examination.

I have mentioned these details rather fully as I think the affection is sometimes confused with mucous gastritis. J. Lovett Morse[2] describes

[1] *Lancet*, 1907, ii, p. 1670.

[2] *Amer. Journ. Dis. Child.*, 1911, i, pp. 366-75.

as pyloric spasm the case of a child, aged 6 weeks, which from the description is what I regard as mucous gastritis. There was much mucus in the gastric contents on lavage and much mucus in the stools. In the discussion on congenital pyloric stenosis, opened by me at Toronto in 1906, I mentioned the possibility that the pylorus "might become blocked by a plug of inspissated mucus, or by swollen mucous membrane, in gastric catarrh." And in the same year Hall recorded such a case, death resulting from gastro-enterostomy at 7 months of age. The child had persistent vomiting since birth and only weighed as much as when born. A plug of mucus, due to chronic gastritis, blocked the pylorus, and the intestines were empty. In the case I have detailed there is little doubt that the pyloric obstruction, giving rise to the temporary gastric peristalsis, was due to a similar plug of mucus or to congested gastric mucosa.

It is, however, in the group of cases described as hypertrophy of the pylorus that the greatest danger of error in diagnosis arises. This is obvious on consideration of the results obtained by Miller and Willcox on the examination of the gastric contents of these cases. I do not criticize their results, but I cannot agree with the conclusions they draw. They found that there is retention of food in the stomach, an excess of mucin, a marked increase in ferment activity, and that the acidity is variable and tends to be below normal. The tongue is generally very furred. And they further state that the acidity varies with the amount of gastritis present, and that the gastric abnormalities are modified by regular lavage. It seems clear that these writers are including under the symptomatology of hypertrophic stenosis of the pylorus those due to the gastritis which may, but is not necessarily, present as a complication. This is the condition I describe as mucous gastritis. It is not present in early stages, and may be absent throughout. Only recently, in a child operated on in the sixth week of life, lavage of the stomach showed that the gastric contents were extremely acid, and that there was no excess of mucus. At operation the typical condition of hypertrophic stenosis of the pylorus was present.

In addition the tongue may be clean throughout, or, at any rate, until secondary gastric catarrh ensues. The gastritis is a complication or a sequel, and the results of gastric examination must be ascribed to the gastritis and not regarded as indicative of pyloric hypertrophy. In mucous gastritis all these signs, except marked retention of stomach contents, may be present and without co-existent pyloric hypertrophy. Whether you call this affection mucous gastritis, catarrhal gastritis, or

subacute gastric catarrh, it must be recognized as an affection that is not infrequent in babies at the age when pyloric hypertrophy is common.

Mucous gastritis can be cured by purely medical treatment. The *prognosis* is good even in severe cases if the patient is treated carefully and patiently. No definite improvement can be expected in less than a week or two, and any attempt to increase the quantity or quality of the diet at all quickly is likely to lead to relapse. If, however, these cases are diagnosed as hypertrophy of the pylorus, the result will be an unduly favourable view of the prognosis in pyloric hypertrophy under medical treatment.

In the *treatment* of mucous gastritis I have not found lavage of very great value, except as a temporary expedient for a few days at a time. Sometimes it appears injurious. Nevertheless I recommend it as a measure worthy of trial in all cases in which there is much mucus secreted, using an alkaline lotion for the purpose either once or twice a day. In some cases the frequent administration of small doses of lime-water, bicarbonate of soda or citrate of soda is more beneficial,

The diet must be simple and easily digestible. Cow's milk is curdled very readily, increasing the vomiting and distress. I have obtained the best results from sweet whey powder, 1 dr. in 2 oz. of water providing a mixture analytically identical with freshly made whey. It is simpler to prepare than whey, and differs from it in some biological or chemical characters, for it does not so constantly produce the green stools passed by infants fed on whey. Horlick's malted milk and Allenbury No. 1 food have proved also useful. All these foods are lacking in the anti-scorbutic properties of fresh milk. In mild cases diluted peptonized milk can be tried. Asses' milk sometimes agrees. As soon as the worst symptoms have subsided and the secretion of mucus has diminished, small quantities of cream are gradually added to the diet. The diet of cream and whey is gradually replaced by peptonized milk, and then by milk and water or barley-water, or by milk and water with a small amount of Benger's food. Milk-sugar is preferable to cane-sugar. The latter is apt to increase the catarrh. Maltine is beneficial if the child likes it and is much constipated. Citrated milk can be tried when recovery is well advanced. Alcohol is contra-indicated, except in emergencies. Bismuth, especially the liquor bismuthi, is occasionally beneficial, but is more often disappointing. I attach far more importance to diet than to drugs, except in so far that alkalies help to dissolve the mucus and enable it to pass more easily through the pylorus.

Dr. MILLER said it was very difficult to reply at once to the criticisms made by Dr. Cautley in his paper on the work which he (the speaker) did with Dr. Willcox four or five years ago. He would explain what they were at that time trying to do, to show why their classification was not so complete as that which Dr. Cautley suggested. They tried to find out whether there was some congenital abnormality in the gastric juice of cases of hypertrophic pyloric stenosis. The phrase "inborn errors of metabolism" was then much used, and it occurred to them that there might be something of that sort present, and so they set to work to find out what the gastric contents were like. They therefore analysed the gastric contents of all cases in which the early symptoms suggested hypertrophic pyloric stenosis—i.e., those in which the differential diagnosis was difficult. These cases they found to show quite different analyses corresponding clinically to two groups, pyloric spasm (acid dyspepsia) and true hypertrophic pyloric stenosis. They then took some cases of marasmus as controls, in order to find out what might be regarded as the usual condition of the gastric juice in very wasted infants. One could wish that Dr. Cautley had given more cases of the condition which he called "mucous gastritis," because in the particular case which he detailed, in which gastric peristalsis was present, and in which a pyloric tumour was palpable, one would have thought that the diagnosis of hypertrophic pyloric stenosis would have been correct. It was in a female child, and in that sex the condition was usually milder than in male children. Dr. Cautley suggested that Dr. Willcox's and his own cases were not all true cases of hypertrophic pylorus. He believed all but one of them died, and post mortem they were as typical and marked cases as one could see. One would wish also that the gastric contents in Dr. Cautley's cases had been analysed, especially in view of his suggested classification of cases on these lines. But in only one case, so far as he noticed, were the gastric contents tested, and they were reported as being simply acid, which of course would have been the case anyhow. The total acidity did not appear to have been estimated. The condition of mucous gastritis, if it were admitted as a type of case, would mean that into the diagnosis of hypertrophic pyloric stenosis by gastric analysis an additional difficulty had been imported, for in their own cases they found that in no other type of case was mucin present except in hypertrophic pyloric stenosis, always remembering that they were dealing with chronic, and not acute cases. In the latter, mucin was often present, but in the chronic cases which they recorded they regarded the presence of mucin as a dangerous symptom and as strongly suggestive of hypertrophic pyloric stenosis. Negative results in this matter were of no value, and no stress should be laid upon them. He looked forward to studying the paper more carefully when it was available in print, but at present it appeared to him that Dr. Cautley's class of disease termed "mucous gastritis" could be explained by substituting for his mild type of case acute and subacute gastritis, and for his severe cases hypertrophic pyloric stenosis.

PROCEEDINGS

OF THE

ROYAL SOCIETY OF MEDICINE

VOLUME THE FIFTH

COMPRISING THE REPORT OF THE PROCEEDINGS FOR THE SESSION 1911-12

CLINICAL SECTION



LONDON
LONGMANS, GREEN & CO., PATERNOSTER ROW
1912

Clinical Section.

OFFICERS FOR THE SESSION 1911-12.

President—

Sir WILLIAM OSLER, Bt., M.D., F.R.S.

Vice-Presidents—

Sir WILLIAM WATSON CHEYNE, Bt., C.B., F.R.S.
W. BRUCE CLARKE, F.R.C.S.
ARCHIBALD E. GARROD, M.D., F.R.S.
SIDNEY P. PHILLIPS, M.D.
WALTER G. SPENCER, M.S.
SAMUEL WEST, M.D.

Other Members of Council—

JAMES CALVERT, M.D.
ALBERT CARLESS, M.S.
EDRED M. CORNER, M.C.
DOUGLAS DREW, F.R.C.S.
T. CRISP ENGLISH, F.R.C.S.
JOHN FAWCETT, M.D.
ROBERT HUTCHISON, M.D.
RAYMOND JOHNSON, B.S.
L. H. MCGAVIN, F.R.C.S.
JAMES MACKENZIE, M.D.
G. R. MURRAY, M.D.
W. GIFFORD NASH, F.R.C.S.
H. D. ROLLESTON, M.D.
H. BATTY SHAW, M.D.
HERBERT TILLEY, F.R.C.S.
H. J. WARING, M.S.
C. PERCIVAL WHITE, M.B.
W. ESSEX WYNTER, M.D.

Hon. Secretaries—

JAMES GALLOWAY, M.D. CHARLES H. FAGGE, M.S.

Representative on Library Committee—

LEONARD G. GUTHRIE, M.D.

Representative on Editorial Committee—

JAMES GALLOWAY, M.D.

CLINICAL SECTION.

CONTENTS.

October 13, 1911.

PAGE

Myeloid Sarcoma of the Tibia. Resection of Upper Third of Tibia, followed by Bolting of the Bones together with Living Fibula. By Sir FREDERIC EVE, F.R.C.S. 1

Severe Facial Neuralgia Associated with Myoma Cutis. By JAMES GALLOWAY, M.D. 7

Acute Encephalitis. By WILFRED HARRIS, M.D. 11

Spondylose Rhizomélique. By H. D. ROLLESTON, M.D., and W. P. TINDAL-ATKINSON 12

General Thyroid Malignancy. By R. C. ELMSLIE, M.S. 15

By P. LOCKHART MUMMERY, F.R.C.S.

(1) Excision of the Rectum for Cancer Six and a Half Years after Operation 18

(2) Excision of the Rectum for Cancer Seven Years after Operation ... 18

(3) Complete Excision of the Rectum for Cancer Two and a Half Years after Operation 19

Notes of a Case of Pseudo-hermaphroditism. By CHARLES WARD (Pietermaritzburg) 19

By H. BATTY SHAW, M.D.

(1) Case showing Unusual Area of Audibility of a Cardiac Murmur ... 21

(2) Recurrent Attacks of Dyspnœa (Asthma), accompanied on each occasion by a Faint Erythematous Annular Rash on the Limbs and Body 23

Hæmorrhagic Pericardial Effusion. By P. S. MARTIN 24

Arteriovenous Anastomosis for Gangrene due to Syphilitic Endarteritis. By H. MORRISTON DAVIES, F.R.C.S. 25

Clinical Observations on an Epidemic of Acute Poliomyelitis in Cornwall. By ALEXANDER GREGOR, M.D., and L. B. HOPPER 28

November 10, 1911.

PAGE

Enlarged Spleen with Recurring Hæmatemesis. By HERBERT P. HAWKINS, M.D. 43

Toxic Cirrhosis, with Spleno-portal Thrombosis and Ascites, treated by Femoral Drainage. By W. ESSEX WYNTER, M.D., and JOHN MURRAY, F.R.C.S. ... 45

A large Malignant Growth of the Cheek which has disappeared under Radium Treatment. By N. S. FINZI, M.B. 48

Hæmochromatosis with Diabetes. By REGINALD MILLER, M.D. 51

Ivory Exostosis of the Frontal Sinus. By W. SAMPSON HANDLEY, M.S. ... 52

Two Cases of Malignant Anæmia. By NORMAN DALTON, M.D. 55

The Rheumatic Diathesis and the Thyroid Gland. By LEONARD WILLIAMS, M.D. 58

Lipoma of Tongue. By V. WARREN LOW, F.R.C.S. 62

Further Experience in the Treatment of Cholera by Injections of Hypertonic Salines and Permanganates Internally among Europeans at Palermo. By LEONARD ROGERS, M.D., Major I.M.S. 62

December 8, 1911.

By HERBERT TILLEY, F.R.C.S.

(1) Four Cases illustrating Surgical Treatment of Chronic Empyema of the Nasal Accessory Sinuses 83

(2) Demonstration, by the Direct Method, of Papillomata of the Vocal Cords 86

Long-standing Dislocation of the Patella (? Traumatic) and Osteo-arthritis of the Knee-joint treated by Operation Eighteen Months ago. By MORRISTON DAVIES, F.R.C.S. 86

Acute Periostitis of Rib with Separation of Sequestra (Non-tuberculous, Non-traumatic). By HENRY CURTIS, F.R.C.S. 89

Healed Double Empyema. By E. M. CORNER, M.C., and L. GRANT, M.D. ... 90

By R. P. ROWLANDS, M.S.

(1) Anterior Gastro-jejunostomy; Perforated Jejunal Ulcer; Detachment of Jejunum; Excision of Ulcer; Gastro-duodenostomy ... 91

(2) Gastric Carcinoma; Partial Gastrectomy two years ago 92

By C. GORDON WATSON, F.R.C.S.

(1) Excision of Large Myeloid Sarcoma of the Ischium 93

(2) Painful Osteo-arthritic Hip 94

Two Cases of Rodent Ulcer treated by Fulguration. By P. TURNER, M.S., and C. IREDELL, M.D. 95

Case of Achondroplasia. By V. ZACHARY COPE, M.S. 97

Congenital Specific Stenosis of the Fauces and Pharynx. By W. G. SPENCER, M.S. 98

Ligation of the Innominate Artery for Subclavian Aneurysm. By C. A. BALLANCE, M.V.O., M.S. 99

Multiple Melanotic Sarcomata of the Skin, possibly Secondary to a Melanotic Sarcoma of the Skin, removed Eighteen Months ago. By V. WARREN LOW, F.R.C.S. 104

January 12, 1912.

PAGE

Primary Chancres of the Lip. By PHILIP TURNER, M.S. 107

Tumour below the Right Hypochondrium. ? Riedel's Lobe. By HERBERT FRENCH, M.D. 109

By F. PARKES WEBER, M.D.

(1) Recurrent Aphasia with High Blood-pressure 112

(2) Chronic Splenomegaly of Uncertain Origin, with Persistent Leucopenia 113

Three Cases of Alcohol Injection of the Gasserian Ganglion for Trigeminal Neuralgia. By WILFRED HARRIS, M.D. 115

Enterogenous Cyanosis. By HECTOR CHARLES CAMERON, M.D. 119

Pulmonary Stenosis in a Woman, aged 34. By H. E. SYMES-THOMPSON, M.D. ... 120

Epithelioma of Tongue; Operation; Subcutaneous Recurrences on Back Five Years later. By C. H. FAGGE, M.S. 121

Supernumerary Mamma in a Male. By CYRIL A. NITCH, M.S. 122

The Relative Value of Immediate and Delayed Laparotomy in Pneumococcal Peritonitis. By HECTOR CHARLES CAMERON, M.D. 123

February 9, 1912.

Excision of Epithelioma of Lower End of Pharynx. By F. J. STEWARD, M.S. ... 137

Anomalous Œdema. By FREDERICK LANGMEAD, M.D. 140

By F. PARKES WEBER, M.D.

(1) Diffuse Symmetrical Lipomatosis 142

(2) Osteitis Deformans (Paget's Bone Disease) with Chronic Eczema ... 143

Persistent Nystagmus associated with Periodical Vomiting. By GEORGE F. STEBBING, M.B. 144

Hirschprung's Disease: Congenital Dilatation of the Colon. By HECTOR MACKENZIE, M.D., and W. H. BATTLE, F.R.C.S. 146

Tabes Dorsalis with one Knee-jerk absent and the other brisk. By HERBERT FRENCH, M.D. 149

Neuro-fibroma of the Supra-orbital Nerve in the Orbit. By W. H. BATTLE, F.R.C.S. 152

Congenital Absence of the Left Half of the Diaphragm, simulating Pneumothorax. By WILFRED HARRIS, M.D., and W. H. CLAYTON-GREENE, F.R.C.S. 153

March 8, 1912.

Progressive Muscular Dystrophy. By JAMES GALLOWAY, M.D. 157

By HERBERT FRENCH, M.D.

(1) Paralysis of Right Vocal Cord, Obstruction to Superior Vena Cava, and Partial Obliteration of Right Radial Pulse from Mediastinal Fibrosis, probably Syphilitic 163

(2) A Case in which there were two separate large Thoracic Aneurysms ... 165

PAGE

Sarcoma of the Ilium treated by Coley's Fluid. By ALBERT CARLESS, M.S. ... 169

Mikulicz's Disease. By W. H. BATTLE, F.R.C.S. 171

By WILFRED TROTTER, M.S.

(1) Rupture of Extensor Tendon of Terminal Phalanx of Finger ... 174

(2) Rupture of Extensor Tendon of Terminal Phalanx of Finger. Treatment by Fixation, with Middle Joint Flexed; Cure 175

Hyperostosis Cranii. By E. B. WAGGETT, M.B., and EDWARD D. DAVIS ... 175

A Family with Membranous Discharge from the Nose. By A. M. GOSSAGE, M.D. 176

Note on a Case of Ruptured Intestine, with Especial Reference to the Mode of Production of the Lesion. By WILFRED TROTTER, M.S. 179

March 20, 1912.

Observations of the Movements of the Heart by means of Electrocardiograms. Introduced by W. EINTHOVEN, M.D. (Leyden)... 183

Dr. FLORENCE BUCHANAN (p. 185)—Dr. JAMES MACKENZIE (p. 188)—Dr. THOMAS LEWIS (p. 189).

May 31, 1912.

A Case of (?) Acromegaly. By RUPERT FARRANT, F.R.C.S. 193

Thoracic Aneurysm not connected with the Aorta. By FREDERICK LANGMEAD, M.D. 194

Relief following Bilateral Nephrotomy and Drainage for Acute Nephritis attended by Suppression of Urine and Uræmic Convulsions. By W. G. SPENCER, M.S. 197

By PAUL B. ROTH, F.R.C.S.

(1) Osteo-periostitis of Right Tibia (Congenital Syphilitic) 200

(2) Osteo-periostitis of Left Tibia (Congenital Syphilitic) 200

By JAMES GALLOWAY, M.D.

(1) Tonic Spasm of the Muscles, chiefly of the Extremities (? Myotonia) 201

(2) Sclerema Cutis (Adultorum) 203

Hæmorrhagic Disease in a Child (? Scorbutic). By G. A. SUTHERLAND, M.D. ... 205

Fibro-caseous Tuberculosis of Glands in Neck; Caries of Dorsal Spine. By ROBERT HUTCHISON, M.D. 207

Ununited Fracture of the Ulna treated by Bridging the Gap with a Slice sawn from the Tibia. By C. H. FAGGE, M.S. 207

By DUNCAN C. L. FITZWILLIAMS, F.R.C.S.

(1) Multiple Arthritis of Doubtful Origin 210

(2) Case showing the Result of Bilateral Division of the Ramus of the Jaw for Ankylosis of both Joints (?) Nineteen Months after Operation 211

(3) Sarcoma of the Jaw, probably Myeloma 212

Facio-scapulo-humeral Type of Muscular Dystrophy in Four Patients in Three Generations. By H. BATTY SHAW, M.D., and P. J. EDMUNDS, M.B. ... 212

PAGE

Aortic Regurgitation with Extreme Pulsation of the Aortic Arch; Diastolic Shock and Diastolic Thrill over the Heart. By F. PARKES WEBER, M.D. 214

Persistent Œdema of Right Hand and Forearm after Slight Tramautism. By F. PARKES WEBER, M.D., and G. DORNER, M.D. 215

Actinomycotic Sinuses in the Thigh, from one of which a Concretion, probably formed in the Appendix, was discharged. By RAYMOND JOHNSON, F.R.C.S. 216

Gonococcic Empyema. By HENRI GÉRAUD, M.D. (Paris), and H. J. JOHNSTON-LAVIS, M.D. 217

The Council think it right to state that the Society does not hold itself in any way responsible for the statements made or the views put forward in the various papers.

LONDON:
JOHN BALE, SONS AND DANIELSSON, LTD.,
OXFORD HOUSE,
GREAT TITCHFIELD STREET, OXFORD STREET, W.

Clinical Section.

October 13, 1911.

Sir A. PEARCE GOULD, K.C.V.O., in the Chair.

Myeloid Sarcoma of the Tibia. Resection of Upper Third of Tibia, followed by Bolting of the Bones together with living Fibula.

By Sir FREDERIC EVE, F.R.C.S.

E. B., AGED 18, was admitted to hospital on March 27, 1911.

History: In October, 1910, she slipped on a polished floor and the left leg bent outwards. After resting for six days she was able to walk with great difficulty, and has been in bed for the last seven weeks.

Condition on admission: There was a rounded swelling of the head of the left tibia; on its outer side it was soft and fluctuating. The knee was fixed at an angle of 90°, and there was very slight movement in it. Distinct lateral movement existed just below the level of the swelling. It seemed probable that a spontaneous fracture took place when she fell in October. A radiograph showed an endosteal sarcoma of the head of the tibia which had completely destroyed the end of the bone with the exception of the articular cartilage.

Operation (April 8, 1911): After confirming the diagnosis of myeloid sarcoma by an exploratory incision and immediate microscopic examination, the tumour was exposed by an incision on each side of knee connected by a transverse incision below the patella. The knee-joint was freely opened, the tumour separated from the popliteal

artery and vein and from the surrounding structures. The tibia was then sawn through well below the growth. The articular surface of the femur was sawn off. The growth had spread widely among the surrounding structure and was not enclosed in a capsule of bone.

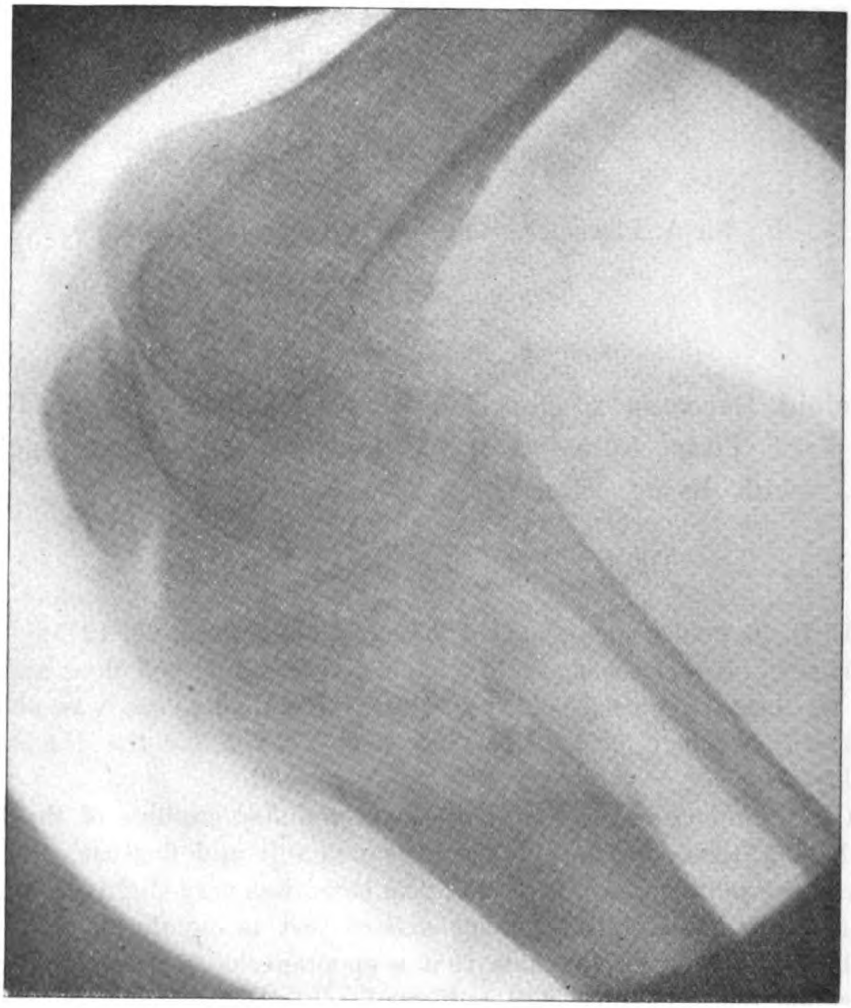


FIG. 1.

Myeloid sarcoma of the tibia (March 28, 1911).

In the course of dissection the external popliteal nerve, which was embedded in the tumour, was divided, but its ends were subsequently sutured. The fibula was exposed by a longitudinal incision and the muscles separated from it, care being taken to preserve the periosteum.

A portion of its upper end, 6 in. in length, was removed. A hole was then bored in a vertical direction in the extremities of the femur and tibia respectively, and into these holes the portion of the fibula was bolted in such a manner that the ends of the two bones were separated

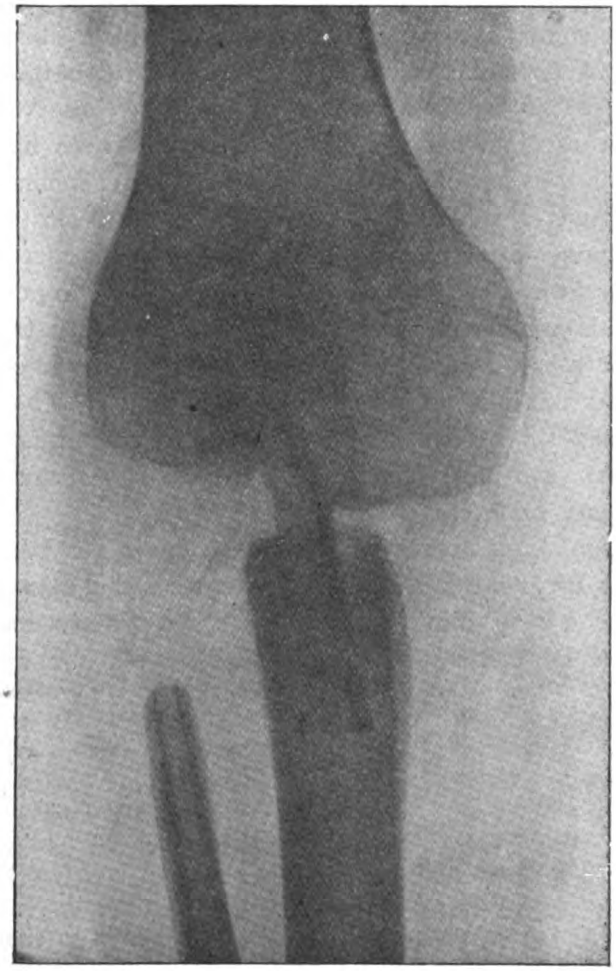


FIG. 2.

Skiagram taken nearly six months after the operation (September 26, 1911).

to the extent of over $1\frac{3}{4}$ in. The wound healed by first intention, and the union between the two bones of the leg has gradually become firmer, although the patient is not yet able to bear her weight on the foot.

Radiographs taken from time to time show that the transplanted fibula has gradually increased in circumference by the new formation of bone, and at the same time the interval between the divided extremities of the femur and tibia has become narrower, owing to growth of bone from their periosteum and cut surfaces. There is therefore good reason to hope that the limb will ultimately become a useful one.

Recently some interesting observations have been made by Axhausen[1] and Lobenhoffer[2] on the changes which take place after implantation of portions of living bone. The observations of the first named were chiefly made on animals, and these have been confirmed by the latter author as regards man. The conclusion at which they have arrived is that when bone covered with periosteum is transplanted the bone substance itself dies and only the periosteum lives. The new bone formation proceeds, therefore, from the periosteum, and ultimately leads to reparation of the entire bone. In transplantation operations, therefore, the periosteum must be preserved if the growth and permanent fixation of the graft are required. These views are entirely opposed to those pronounced by Sir William Macewen.

In my own case the radiographs show a new formation of bone beneath the periosteum of the graft.

In conclusion, I must express my obligation to Dr. W. Lobenhoffer, of Würzburg; for while I was considering, in the case related, how the length of the limb might to some extent be preserved after removal of the sarcoma, I became acquainted with his paper already quoted.

DISCUSSION.

Sir FREDERIC EVE added that in connexion with the case he cast about for a method of utilizing the fibula. His first intention had been to divide the fibula partially at its middle, and fix its upper end to the lower end of the femur; subsequently, after it had become united there, to have completely detached the lower part of the fibula and joined it to the tibia. While he was considering the question he became acquainted with the fact that Professor Enderlen[3] had used the fibula as a bolt to fix the bones together. It was obvious that if he had removed the upper third of the tibia, and united the tibia directly to the femur, the limb would have been so shortened that it

[1] **Axhausen, "Freie Osteoplastik,"** *Arch. f. klin. Chir.*, Berl., 1908, lxxxviii, p. 23.

[2] **"Beiträge zu der Lehre von der freien Osteoplastik,"** *Beitr. z. klin. Chir.*, Tübingen, 1910, lxx, p. 87.

[3] **Enderlen's case ultimately failed because the tumour, which was a periosteal sarcoma, recurred. The patient refused amputation in the first instance.**

would have been useless. By using the fibula as a bolt he gained nearly 2 in. in the length of the limb. Some interesting work had been done recently on the changes which took place in living bone when transplanted, especially by Axhausen, who had worked experimentally on animals. These experiments, together with the examination of the graft in one or two cases in which a bolt had been used in the human subject, went to show that when a portion of living bone was grafted in the way he had described in the synopsis, the bone substance died, but the periosteum and the endosteum lived, the new bone was produced mainly by the periosteum, but also to a small extent by the endosteum. Those facts were illustrated by skiagrams which he showed. The latest of these was taken nearly six months after the operation, and if that were compared with the one taken three weeks after the operation it would be seen that the later photograph showed a definite new formation of bone beneath the periosteum of the engrafted fibula. Further, the space which originally measured something like $1\frac{3}{4}$ in. between the cut ends of the femur and tibia was reduced very considerably by an outgrowth of bone from the upper and lower ends of the femur and tibia respectively. The case, although a very promising one, was not yet complete, indeed, he felt that an excuse was almost due from him for exhibiting it to the Section at that early stage. He had already had the patient under observation for six months, and it was known that such people had a tendency to drift away. There was still a certain amount of movement at the seat of junction of the two bones, but the patient was able to lift her leg from the couch. Even supposing that complete consolidation did not take place by the methods of bolting which he had employed, it was some justification for the procedure that it would be easy, subsequently, to turn up a portion of tibia and use it as a buttress by fixing it to the femur and the tibia.

It was a well-known fact that if a myeloid sarcoma was treated by excision, or by erasion, it very rarely recurred. It so happened that in the last week he had seen two of his cases which had been treated by these methods. One was a man with a very large myeloid sarcoma of the fibula which had disrupted the bone. Microscopical examination showed that the surrounding muscles were infiltrated with giant cells and round cells. He removed the upper half of the fibula and the surrounding tissues. At the present time, though it was eight years since the operation was done, the limb remained perfectly healthy. He also saw a young man from whose tibia he removed by erasion a myeloid sarcoma which was of the size of a tennis ball. The operation was done four and a half years ago, and the patient also remained perfectly well.

The CHAIRMAN (Sir A. Pearce Gould) said the case was a very interesting one, and raised a very important practical question, because all present would agree with Sir Frederic Eve that the proper treatment for myeloid sarcoma, whenever possible, was excision or erasion of the growth, not amputation of the limb. And the question must arise in cases which had advanced to such a large size, as in the present instance, before coming under the care of a surgeon, whether some means could be found to prevent undue shortening of

the limb. He hoped Sir Frederic would find it possible to exhibit the patient again later on. In the printed notes Sir Frederic said, "The patient is not yet able to bear her weight on her foot." He would like to know whether the girl could stand on the limb at all, because the whole value of a leg was determined by the ability to stand on it. Therefore, what members of the Section would be particularly anxious to see was the usefulness of the limb as a weight-bearing limb, whether it was likely to be better for the patient than an artificial limb. The case recalled to his memory a specimen of an ununited fracture of the middle of the shaft of the tibia in which the fibula was uninjured, and bore the whole weight of the limb. The fibula was much hypertrophied. It occurred to him that perhaps this fact might be utilized in such a case as that under discussion, and that the fibula might have been left intact and united to the femur, and an attempt made to obtain hypertrophy of the fibula, and the transmission of the weight of the body through that bone.

Mr. MORRISTON DAVIES remarked that a case which he operated upon at the beginning of the present year had some bearing on this one, although it did not answer the question raised by the President. It was a sarcoma of the upper end of the tibia, involving it up to the joint line. In that case he removed the upper 5 in. of tibia, and replaced it by 5 in. of tibia from a leg amputated on account of extreme wasting and flail-joint. He left the capsule and semilunar cartilages attached to the femur, and stitched the cut edge of the capsule and of the infrapatellar ligament to the articular surface of the new tibia. The new part of tibia was pegged down by a piece of fibula to the shaft of the original tibia. Amputation thirteen weeks later showed that in that case union did not take place between the two ends of the tibia, but the knee-joint was reconstituted in a most surprising way; the capsule was adherent all the way round, the infrapatellar ligament was firmly fixed to the graft, the semilunar cartilages were attached to the tibia, and even the crucial ligaments had acquired a fresh and satisfactory attachment to the graft. The muscles which had been divided from their insertions on the inner side of the tibia had become fixed to the new bone. Movement was possible through about 30°. The repair, therefore, of the articular part of the tibia and of the knee-joint was extraordinarily satisfactory. One probable reason for the absence of bony union of the shaft of the tibia was the death of the fibula used to peg the two bones together.

Sir FREDERIC EVE, in reply, said he could not answer the Chairman's question as to whether the girl was able to stand on the limb yet, as he had not tried it in that way. But she could lift the leg from the couch.

Severe Facial Neuralgia associated with Myoma Cutis.

By JAMES GALLOWAY, M.D.

THE patient, a male, aged 34, by occupation a clerk, suffers from neuralgia affecting the left cheek and radiating throughout the whole



FIG. 1.

Myoma cutis.

left facial region. He has suffered in this way for many years, but the pain increases and is sometimes so intense as to be almost unbearable, completely incapacitating him from following his occupation. The

neuralgia is always much worse on exposure and in cold weather; during the past warm summer he has experienced greater freedom from pain than for some years.

On examining the patient's left cheek the surface is seen to be studded with rounded flattened tumours, varying in size up to that of a large pea (fig. 1); in some spots they have almost coalesced so as to form elevated patches. The tumours have a yellowish-pink colour, with a slight suggestion of translucency. They are firm to touch, and are

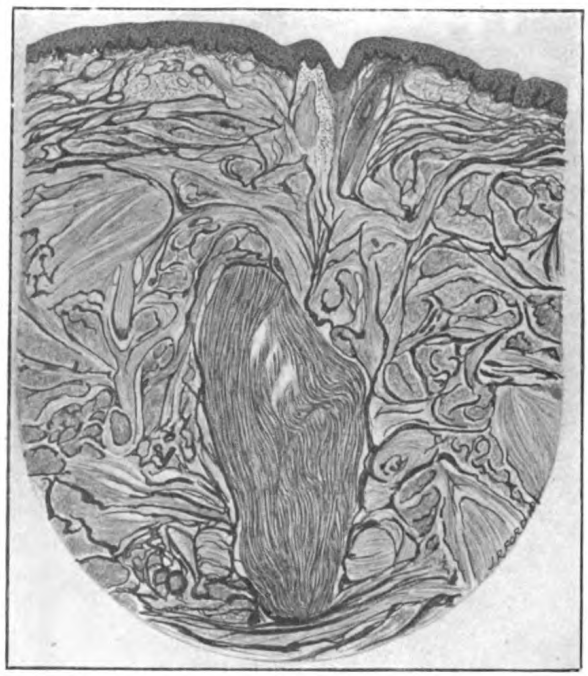


FIG. 2.

Section (× 20) through the tumour, removed from the skin of the front of the right thigh (stained by Van Gieson's method). Bands of unstriated muscle-fibre displace the normal connective tissue fibres of the skin. The largest bundle of plain muscle underlies one of the small hair-follicles which is seen in section under the epidermis.

usually slightly tender. The patient associates the pain with the presence of these tumours, and thinks that the neuralgia originates in them. The tumours have been noted by the patient for at least twenty years, and are increasing in number and size, rather than showing signs of diminution.

In addition to the tumours on the left cheek a few tumours of similar characters are seen on different parts of the trunk and extremities, sparsely scattered without grouped arrangement. One of these on the front of the right thigh was removed and examined microscopically (fig. 2). It is found to consist of irregularly rounded masses of plain muscle-fibre, which are largest in the neighbourhood of a hair-follicle; the largest mass of muscle seems to be in direct relation to the hair-follicle, and in this case, therefore, the presumption is that the new muscle growth has originated from the muscles of the hair-follicles—the arrectores pilorum.

The patient is brought forward to obtain suggestions for the treatment of his intense neuralgia, especially as to the advisability of using injections of alcohol into the main branches of the left fifth nerve.

DISCUSSION.

Dr. GALLOWAY added that this patient had been referred to him by a medical friend on account of the severe neuralgic pains from which he suffered affecting the left side of the face. The neuralgia had persisted for many years, but had increased latterly, and had now reached such a pitch that he looked forward to the coming winter with much apprehension, as the pain had always been most severe in cold weather. The pain incapacitated the patient from work, and he was prepared to undergo any treatment that held out a reasonable prospect of relief. The severity of the pain seemed to equal that of bad trigeminal neuralgia, but it differed in its nature inasmuch as there seemed to be some degree of superficial hyperæsthesia always present, and the patient seemed to think that the tumours of the left cheek were the starting point of the pain. In the descriptions given of these tumours superficial tenderness had frequently been noted, but no mention was made of the excessive neuralgic pain from which this patient suffered. Dr. Galloway inquired what should be done to remedy such neuralgia. He told the patient that the tumours had been excised in such cases, and that the removal of the tumour had the effect of relieving the pain. But in this case so extensive an excision would be necessary as to be almost out of the question, on account of the great disfigurement it would produce. It was noteworthy that the tumours on other parts of the body habitually covered gave rise to practically no discomfort. He inquired of Dr. Wilfred Harris, who had experience in the treatment of trigeminal neuralgia by the interstitial injection of alcohol into the course of the main branches of the fifth nerve, whether that method of treatment would hold out a good prospect of relief in the case of this patient.

Dr. WILFRED HARRIS said he had never met with a case of the kind in which neuralgia had been associated with the present condition of the skin of the face. The pain which the patient had been describing to him was not quite of the epileptiform type which was generally associated with trigeminal neuralgia. It was not a paroxysmal pain, lasting two or three minutes and then leaving the patient free until the next spasm came on, but it persisted until the face became quite warm again. The patient did not appear to be able to provoke the pain in the ordinary ways which were sufficient to bring on a spasm of ordinary trigeminal neuralgia—i.e., by such means as rubbing the face, blowing the nose, eating, &c. The patient, however, told him that there was one thing which started it and which reminded one of trigeminal neuralgia—namely, experiencing a sudden emotion, or striking a minor chord on the piano. With regard to the suggestion that the condition might be cured by injecting alcohol, he thought this possible; if so, he would suggest that the second division of the nerve should be chosen first. The pain was peripheral in its origin, and if one could cut off the path of impulse along the trunk of the nerve by any method which would arrest the pain, it was justifiable. In trigeminal neuralgia one might arrest the pain by the injection of the trunk of the nerve. If the injection was not intraneural, but perineuritic, the process would not cause destruction of the nerve, yet it might arrest the pain of trigeminal neuralgia completely for months. He did not think a perineuritic injection would have any effect in this case, but if one hit the nerve properly by the injection and caused deep anæsthesia he was sure the pain would be relieved.

Mr. P. LOCKHART MUMMERY suggested that another method besides injection was an alternative to excision, when the latter was out of the question, namely, the method which some surgeons practised in very bad cases of peri-anal pruritus—i.e., making an incision and undercutting the skin with the knife, and then sewing it back again. By that means one divided the superficial nerves as they passed from the skin, and this was very effective in bad cases of pruritus ani. He did not know why it should not be equally successful in such a case as the present one. The irritation and pain seemed to be in the skin, and if the superficial nerves were cut it seemed likely that it would enable the skin to return to its normal condition, for he believed that in such conditions there was a nutritional defect in the skin caused by disease of nerves, and if the nerve-endings could not continue their bad influence owing to having been cut, the condition often entirely disappeared.

Sir FREDERIC EVE suggested that the whole surface involved might be removed, and a Thiersch's skin-graft put on. The disfigurement following skin-grafting would be less than that now existing. In one or two cases of very extensive hairy mole on the side of the face he had used that method, and there was very great improvement in the appearance of the patient afterwards.

Acute Encephalitis.

By WILFRED HARRIS, M.D.

O. H., A GIRL, aged 13, living at Iver, near Uxbridge, was seized on May 21 with a shivering attack, followed by severe headache and vomiting, with pyrexia of 101·5° F. for one day only. She became delirious, and unconscious on the following evening, and did not speak again for thirteen weeks.

She was admitted into Iver Cottage Hospital on June 10, under Dr. Burton-Brown and Dr. Wood, who transferred her to my care at St. Mary's on July 18. There was no pyrexia while she was in the hospital at Iver, but the notes on admission there were: "Patient very restless and noisy; passes urine under her; will not help herself in any way." She was given liquid food, which she took fairly well, though her condition remained practically unchanged, when she was transferred to St. Mary's Hospital on July 18. She was then in a highly excited condition, screaming more or less continuously, and was with difficulty kept in bed. She resented all examination, though she took no notice of anything said to her, and uttered no word. She took her food well on the whole, though she would not help herself at all, and though there was not total incontinence, the nurses had the greatest difficulty in keeping her clean. She usually lay curled up in bed when she was not throwing her limbs about and screaming. Physical examination was negative. Kernig's sign was absent, the knee-jerks, plantar, and pupil reflexes were all active and normal. No rigidity or muscular weakness, eye movements and optic disks normal. No head retraction. Temperature normal, pulse varying from 80 to 90.

On July 25 the notes say: "Patient very restless, and cries without ceasing." Lumbar puncture showed the cerebrospinal fluid to be sterile, with no leucocytosis. August 2: Condition unchanged, taking food well, but will not feed herself. Strong faradism was on July 31 applied to the limbs, trunk and face, intermittently for several minutes, as her mental torpor was not improving, but without result. About August 20 she was first observed to take more notice of her surroundings, and to smile and speak. In a few more days she was able to answer questions intelligently, but in a high-pitched, squeaky voice, and she recognized some flowers sent her from home.

On September 5 I found that all her movements were extremely ataxic, rather more so on the left side, and she was totally unable to stand, though the muscular power of the limbs was fair. All the reflexes were still normal; she had now perfect control over her sphincters, and was able to talk quite sensibly, though still in a high-pitched voice as before. She could read and understand, though her power of accommodation was feeble. Since then she has slowly and steadily improved, and her voice is beginning to be more natural. She can move about a little, holding on to the furniture, but cannot stand alone. There is still ataxy of the arms, especially of the left, but there has never been any nystagmus.

Diagnosis: In the country she was at first considered to be suffering from "heat apoplexy." Her recent condition points to the cerebellum as being particularly involved, but the past history of the case indicates a much wider area of cerebral inflammation, though the pyramidal system seems to have escaped.

Prognosis: Eventual partial recovery.

Dr. WILFRED HARRIS added that in regard to the pyrexia which the child suffered from, he could not get the notes from the doctor at the time, but he had since heard that she had fever one day to the extent of 101·5° F., and that the temperature fell to the normal next day, when she became comatose. She had had no pyrexia since.

Spondylose Rhizomélique.

By H. D. ROLLESTON, M.D., and W. P. TINDAL-ATKINSON.

MALE, aged 49. His mother, aged 86, has been "rheumatic" for forty years, and one brother, aged 60, has a tendency to pains and stiffness of the joints, but is well now. He has not had any injury, enteric fever, syphilis (Wassermann negative), or any disease, except gonorrhœa eighteen years ago. Sudden swelling of the left middle finger came on three weeks after the onset of gonorrhœa and lasted one month, leaving permanent stiffness. Fourteen years ago pain and restriction of movement began in his hips; pain in the spine began eleven years ago. The disease has progressed until the spine and hand became immovably fixed five years ago; since then he has been subject to sudden attacks of pain beginning in the heels, extending to the neck and head, and

making his bed feel like a "bag of spikes." There has never been any recurrence of gonorrhœa or urethral discharge. The patient states that his condition was regarded as being gonococcic in origin when he was in Bartholomew's Hospital.

Present condition: There is rigidity of the whole spinal column, which shows slight kyphosis. There is flexion of the neck and of the



Skiagram of neck from left side.

head, which is fixed and turned to the right. There is no movement at the axo-atloid and occipito-atloid joints. When the patient is placed flat on the ground the head remains in the air, the occiput being 6 in. from the floor. The present height is 5 ft.; before the onset of his illness it was 5 ft. 5½ in. Movement of the jaw varies with the weather, and is accompanied by crackles. The teeth are carious. Movement of the right shoulder is much restricted in all directions; the elbow and

wrist move freely, but with pain. The left shoulder moves more freely than the right, but not perfectly; the elbow and wrist move freely and painlessly; the metacarpo-phalangeal joint of the middle finger is rigid. Both hips are flexed and hardly any movement is possible; movement of the knees is free, but painful. The left thigh and calf measure less in diameter than the right. The tibiæ show an antero-posterior curve. Movement of the chest is much restricted (expansion = 32¼ in. to 32½ in.). The lungs, heart, and other organs are normal. The maximum systolic blood-pressure is 110 mm. Hg. There is no anæmia; a differential count of the leucocytes (8,800 per c.mm.) shows 80 per cent of polymorphonuclears. Urine: 1015, no albumin, no albumose. Knee-jerks increased; plantar response flexor.

Dr. Allpress Simmons reports that "skiagrams of the neck show that the fibro-cartilaginous intervals between the bodies of the cervical vertebræ are narrower than normal, and that the cartilaginous intervals between the articular processes cannot be made out. The anterior common ligament is very obvious and opaque. These appearances suggest ankylosis. The same remarks apply to the upper dorsal vertebræ. In the lumbar region the inter-vertebral fibro-cartilages are narrow and opaque, but there is no definite bony ankylosis; the skiagram does not, however, exclude the possibility of ankylosis of the articular processes. The bony surfaces of the hip-joints are rough and interlocked, but there is no definite bony ankylosis. The shoulder-joints do not show any special change. All the bones are unduly translucent, probably from disuse."

DISCUSSION.

Dr. ROLLESTON invited discussion on the relation between gonorrhœa and the present trouble which had now rendered practically the whole of the spine rigid. Was the lesion gonococcic throughout, or was the case one in which gonorrhœa had so reduced the patient's resistance as to allow some other organism to settle down in the spine—a paragonococcic lesion; or was the lesion independent of the primary gonococcal infection? He also asked for suggestions as to treatment.

Mr. CARLESS asked whether Dr. Rolleston could say anything more as to the continuance of the gonorrhœa; he would like to know whether that disease appeared to have been quickly cured, or whether the patient had a chronic gleet for a long time. He remembered seeing a patient some time ago who gave the same kind of history. He came with what was thought to be rheumatism in most of the joints of his body, including those of the

spine, shoulders, and hips. It was shown, however, to be the result of gonorrhœa acquired seven or eight years before: here the patient had never been entirely clear of his gleet. Recollection of that patient made him curious to know how long the gonorrhœa had lasted in this case.

The CHAIRMAN said he could not give any therapeutic help to Dr. Rolleston, but he saw some years ago, with the late Mr. Thomas Bond, a young man who, after what he believed was his only attack of gonorrhœa, gradually acquired this form of osteo-arthritis or ankylosis, which affected all the joints of the body. When he saw the patient it was rather late in his trouble, as the joints between the atlas and axis, and the occipital bone and atlas, were becoming affected. He had then no movement in his temporo-mandibular joints, and no movement in any joint of the spine or limbs, so that he was helpless. Not long afterwards he heard of his death.

Dr. ROLLESTON, in reply, said the gonorrhœa had lasted four weeks only, and that there had never been any gleet or recurrence of the urethral discharge. The short duration of the gonorrhœa and the freedom of joints, such as the knees, usually affected in gonococcic arthritis, made him hesitate to accept a purely gonococcic origin for the extensive affection of the spine.

A Case of General Thyroid Malignancy.

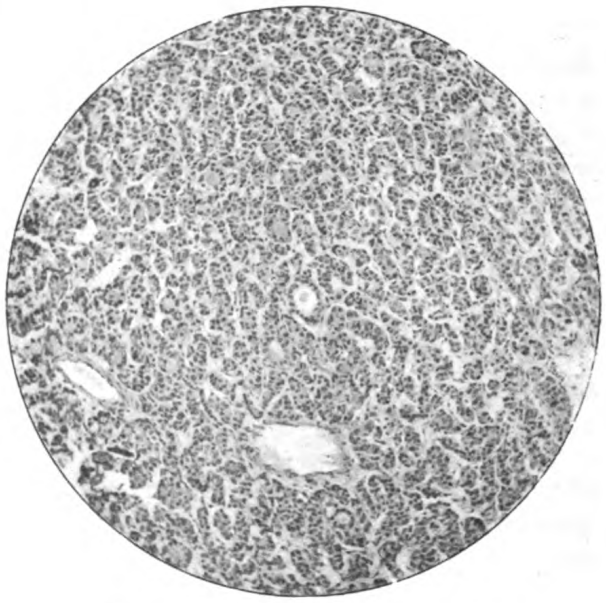
By R. C. ELMSLIE, M.S.

E. L., FEMALE, aged 61, was well until April, 1911, when she began to feel pain in the right wrist. This was treated as rheumatism until July 1, when a swelling appeared. At that time there was pain and swelling on the radial side of the right forearm, a short distance above the wrist. All the movements of the wrist, as well as pronation and supination, were limited. A skiagram showed an endosteal swelling of the lower part of the shaft of the radius. Wassermann's reaction was negative. The swelling was thought to be a sarcoma, and in July a portion of the affected part of the radius was excised. Microscopic examination of the growth showed that it consisted of thyroid tissue, not evidently carcinomatous, but resembling rather an adenoma mostly of the fœtal type of thyroid, but some of the alveoli containing colloid. The stroma was extremely vascular.

On re-examining the patient on July 29 it was found that the left lobe of the thyroid gland was enlarged, hard, but not fixed. The patient states that this enlargement had been present for years. The whole

skeleton was examined with X-rays on the screen in August, but no other endosteal growths were found.

In September the patient had a severe attack of dyspnœa lasting several days and resembling severe asthma; this she states has been a frequent occurrence with her in the autumn and winter. On this occasion, however, although she has improved, the dyspnœa, stridor, and hoarseness have persisted. The thyroid enlargement has increased a little in the last two months, and the veins of the root of the neck on the left side have become distended. The movements of the vocal cords are at present natural.



Thyroid metastasis in the radius (× 85).

DISCUSSION.

Sir FREDERIC EVE desired to mention a case of his own, of a nature bearing on the present one. A woman came with a large pulsatile tumour in the upper end of the femur. Her thyroid was carefully examined, but no enlargement of it was detected. He removed the thigh at the hip-joint. Some few months after the operation the thyroid became obviously enlarged. The operation was done three years ago, and he had seen the patient from time to time. There had been a progressive enlargement of her thyroid, but it was slow. In other respects she remained in perfect health.

Dr. F. G. CROOKSHANK said that Mr. Elmslie's remark to the effect that he did not know of a case of the kind being recorded in a man induced him to state that two or three years ago he saw, with a colleague, the case of a man, aged 60, in whom a swelling appeared near the wrist, in much the same position as in this patient. A surgeon recommended an operation for its removal. For some reason operation was delayed for a time, and in the meantime it was found that there was recent enlargement of the thyroid. In consequence, a further opinion was asked for; there was more delay, and, in the course of a month, ten or twelve more such swellings in various parts of the body were discovered or appeared. The patient died soon after, and the diagnosis made was "thyroid cancer." This seemed to have been a clear case of the kind in a male.

Dr. H. D. ROLLESTON raised the question of the occurrence, in some cases in which the bony growth was accompanied by little or no obvious change in the thyroid, of a secondary growth without any real primary growth in the thyroid. This was on the analogy of the secondary carcinomatous changes in the lymphatic glands in the groin of sweeps whose warty scrotums did not show any primary carcinoma, described some years ago by Sir H. T. Butlin.

Dr. BERNSTEIN said that five or six years ago he had an opportunity of investigating a case which was under the care of Mr. Tubby and Mr. Carling, that of a doctor, aged 50, who had, in his left clavicle, a definite hollowing out, and inside it a growth, which showed all the appearance of thyroid tissue, while the thyroid gland itself was apparently normal. A partial autopsy was made, but permission could not be obtained to examine the skull. There were no metastases in the other bones which were examined, but there was a distinct metastatic thyroid tumour, without any primary tumour in the thyroid, at all events of any size. This experience would answer the question put by Dr. Rolleston.[1]

Mr. ELMSLIE replied that he would like to feel that his case had the same chance as Sir Frederic Eve's case, but the condition of the voice and the occurrence of dyspnœa showed that there was some involvement of the trachea, so that she had very little chance of recovery. There were cases recorded in which a definite growth of the nature of carcinoma had been found in the thyroid; the cases of which he had looked up the records all showed enlargement of the thyroid gland, due sometimes to parenchymatous goitre, often with fibrous change or calcification, sometimes to adenoma, and sometimes to definite carcinoma.

[1] Dr. Hebb also reports a case of cancer in the thyroid isthmus in a male with general metastases, *Trans. Path. Soc. Lond.*, 1888, **xxxix**, p. 342.

A Case of Excision of the Rectum for Cancer Six and a Half Years after Operation.

By P. LOCKHART MUMMERY, F.R.C.S.

MALE, aged 37 (at time of operation). There was a very large growth involving the whole upper portion of the rectum, and partly fixed posteriorly. On examination it was doubtful if the growth could be removed, and another surgeon who saw the patient expressed the opinion that "it was inoperable."

Operation (May, 1905): A preliminary laparotomy was performed to ascertain whether secondary deposits were present, and a temporary colotomy opening was made in the upper end of the sigmoid. The abdominal wound was closed. The coccyx was then excised, and the whole rectum removed from below, the stump of the sigmoid being brought down and stitched to the skin at the anal margin. The patient made a good recovery, and was about in a month. A year later the colotomy spur was destroyed with an enterotome and the opening allowed to close. The new rectum acted well, and he now has excellent control in the normal manner, and is a very busy and active man.

Microscopically the growth was a rapidly growing adenocarcinoma.

Mr. LOCKHART MUMMERY added that the interest of the case was the age of the patient, 37 years only, and the fact that the growth was a very large one. It showed that a very good result could be obtained by the method of excision entirely from the perineum. Half the sigmoid was removed in this case, as well as the whole rectum.

A Case of Excision of the Rectum for Cancer Seven Years after Operation.

By P. LOCKHART MUMMERY, F.R.C.S.

MALE, aged 71. The posterior wall of the rectum was excised after resection of the coccyx, seven years ago, for a large growth about the size of a two shilling piece on the posterior wall. The wound in the bowel was sewn up and healed *per primam*. On section the growth was an adenoma, malignant at the base. The patient has perfect control and has had no trouble with the bowel.

Complete Excision of the Rectum for Cancer Two and a Half Years after Operation.

By P. LOCKHART MUMMERY, F.R.C.S.

THE patient was a man, aged 70, who was admitted to St. Mark's in February, 1909, with a growth in the middle of the rectum. At the operation the coccyx was removed, and the entire rectum and surrounding tissues freed after opening the peritoneal cavity from below and dividing the mesocolon. The mucosa was dissected out of the anal canal, and the bowel pulled down through the sphincters and cut off after closing the wound, the end of the bowel being stitched to the skin ¾ in. from the anal margin, and subsequently trimmed up. The parts removed included the whole rectum and about 4 in. of the sigmoid flexure. The present rectum is formed from the sigmoid. The patient has normal bowel sensation, and very good control except for liquid. He lives an ordinary life and is in excellent health.

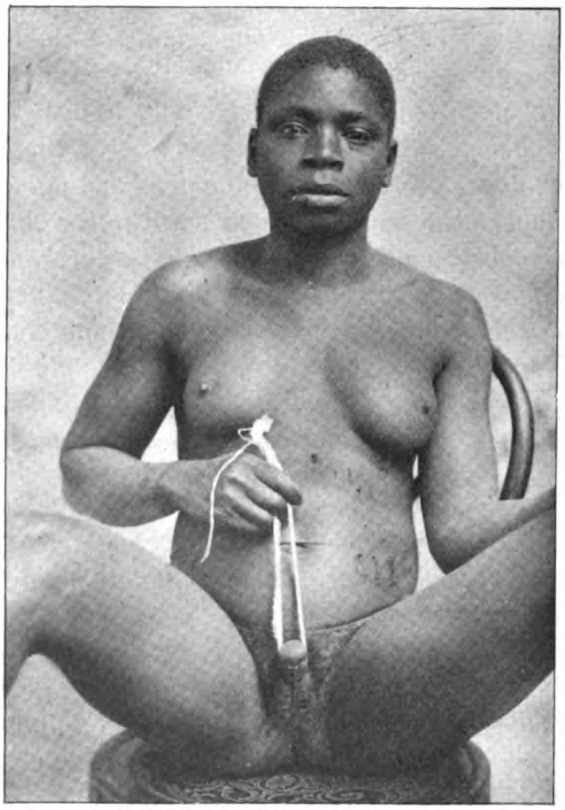
Microscopically the growth was an adenocarcinoma.

Notes of a Case of Pseudo-hermaphroditism.

By CHARLES WARD (Pietermaritzburg).

THIS native ("Sifuba"), aged about 18, who will be spoken of as a male in this report, arrived at this gaol on May 6, 1911. He had been sentenced to six months hard labour and a flogging of ten lashes for entering the bedroom of a white girl. As there was some doubt as to his sex, I have made a thorough examination of him and find as follows: He is 5 ft. 1½ in. in height, and weighs 126 lb. His general appearance is feminine, being plump and round, with small features, and having two well-developed breasts with normal nipples, and apparently containing a breast gland; he has no hair about the face, a little under each arm, and rather scanty about the pubes. His pelvis inclines rather to the female type, being wide; he has thirty teeth, no wisdoms below. His hands and feet are small, and his skin fine and supple. He presents a penis with glans, and of apparently normal structure, 1¾ in. long

and 3 in. round the glans ; it is imperforate, with short prepuce—the opening of the urethra being at the base (hypospadias). There is a flattened, empty scrotum, with the raphe not quite closed for an inch below the opening of the urethra. He has no difficulty in urinating. There are no testicles or other swelling in the inguinal canals. *Per rectum*, the sound in the bladder can easily be felt, and it is easily found



Pseudo-hermaphroditism.

that there is no uterus, ovaries, pubes, nor testicles; there is no vagina nor vulva. The secretion from the prostatic region shows no spermatozoa under examination. He states he has never passed blood, also that the penis "never alters." It seems to be a case of arrested development of the parts going to form the female organs, and later again arrest of the parts going to form the male organs. He comes under none of the scientific classifications of hermaphroditism. It will

perhaps make it plainer why I consider he should be graded as a male if I tabulate the points either way as follows:—

PRIMARY SEXUAL CHARACTERS.

Male.	*Female.*
Penis.	None.
(No testicles, vesicles, nor spermatozoa.)	(No uterus, ovaries, nor vagina).

SECONDARY SEXUAL CHARACTERS.

Male.	*Female.*
Negative.	Features, voice, limbs, breasts, general conformation.

I consider that it is a case of pseudo-hermaphroditism, and was probably going to be female; but the important point for us to consider is his future grading. Possessed of a penis, or, for the sake of argument an enormously enlarged clitoris, capable of being used as a penis, it would be risking too much to let him go forth as a female. In spite of his statement, I believe he *does* have erections, and the offence of which he was found guilty was hardly that of a neutral or eunuch.

According to his own statement he has always been considered a male and has worked as herd-boy and man-servant. He is strong and healthy, and quite capable of hard work, and, when released, it might be as well if some method of keeping him in touch with the authorities could be devised.

A Case showing unusual Area of Audibility of a Cardiac Murmur.

By H. BATTY SHAW, M.D.

M. H., AGED 38; has never been able to do any physical work. He has suffered from infancy from "something wrong with the heart." Frequently there is pain over the front of the right chest; is unable to run because of shortness of breath. Weakness and debility have been noticed more particularly since the age of about 9, when he had scarlet fever; there have been no other illnesses. One sister died of pulmonary tuberculosis: there is no cyanosis nor clubbing of the fingers. The heart's apex beat is in the fifth intercostal space in the left nipple line, and is a little forcible. There is no enlargement to percussion, nor is any abnormality to be seen by X-rays except that the heart is very long

from above downwards. Occasionally a systolic sound, quite localized, is to be heard at the apex of the heart, both on standing up and in recumbency. The pulmonary second sound is a little accentuated, and the aortic second sound can scarcely be heard. A systolic murmur can be heard at its maximum over the aortic cartilage, and this is audible over a considerable area in any direction. It is unaccompanied by a thrill and is louder on forced inspiration: *a murmur can be heard over the interscapular area, at a maximum to the inner side of the left scapula.* It is increased in intensity during inspiration and presumably owes its origin to the same cause as the murmur heard near the aortic cartilage. The tension of the pulse is raised and measures constantly about 160 mm. of mercury. The urine shows a well-marked cloud of albumin. The pulse-rate is 70.

DISCUSSION.

Dr. SHAW handed round skiagrams of the patient, revealing a shadow suggestive of an aneurysm of the aorta, at the junction of the arch and the descending portion. He would be glad to hear from those who had knowledge of coarctation of the aorta as to whether, in their opinion, that condition was present in this patient. If the condition was due to an aneurysm it must be very unusual, as the symptoms dated from such an early period of life.

Dr. H. D. ROLLESTON had had one case of coarctation of the aorta under his care which he failed to recognize as such. It was in a man, aged 30, who had signs of aortic regurgitation; the case was published by the late Dr. Lee Dickinson and Dr. W. J. Fenton.[1] Judging by that case and general reading, he would not be inclined to regard the present case as one of coarctation of the aorta.

Dr. F. PARKES WEBER said the case was not of the type of coarctation of the aorta characterized by great dilatation of the subcutaneous arteries. That was a very well marked type of clinical case, though an exceedingly rare one. He had seen two instances of it: one shown before the Clinical Section by Dr. Gossage,[2] the other, under the care of Dr. James Mackenzie, at Mount Vernon Hospital some time ago. The present case certainly did not belong to that type.

[1] Dickinson and Fenton, *Lancet*, Lond., 1900, ii, p. 1196.

[2] *Proceedings*, 1909, ii, p. 210.

A Case of Recurrent Attacks of Dyspnœa (Asthma) accompanied on each occasion by a faint Erythematous Annular Rash on the Limbs and Body.

By H. BATTY SHAW, M.D.

L. H., A SERVANT, aged 23. She is slender. For five years she has had attacks of asthma accompanied by the occurrence of bronchitic signs in the lungs and apparently provoked by the existence of nasal polypi: the condition is exaggerated by full meals. In each attack of asthma an annular rash appears on the body and limbs. The rash is faintly erythematous and causes considerable itching; it lasts during the attack, disappearing entirely when it passes off. The last rash was observed this morning, when she had an attack of asthma. Both asthma and rash have now almost disappeared. Relief is given to both conditions by the use of potassium iodide. Neither the rash nor the asthma ever occurs alone. The rash has no association with the use of the various drugs employed for the relief of asthma, and was noticed in the attacks before the patient sought any relief from drugs; urticaria is not a feature of the skin eruption.

Dr. SHAW said that it had been observed that his case of the occurrence of a rash with each attack of asthma was equalled in interest by the more common cases of alternation of attacks of asthma with various rashes such as eczema and psoriasis. At the present moment there was, however, reason to bring forward such a case because of the interest aroused by the study of the changes produced experimentally by the parenteral introduction of various proteids derived from the animal and vegetable kingdom, a subject which, however, could not be discussed in detail on such an occasion. But the disturbances brought about in the function of respiration as a result of the hypersensitiveness or anaphylaxis initiated when such proteids were introduced by the parenteral route, recalled some of the features of what was recognized clinically as asthma, and on the other hand the rash noticed in this case recalled the erythematous rashes familiar in the "serum disease," a condition which had been shown to depend on the introduction into the body of the foreign proteids of horse serum. This patient suffered from polypi of the nose, removal of which relieved the condition; it was a speculation not without interest as to whether the attacks of asthma in the case were, as well as the concurrent rash, an expression of an auto-intoxication from the nasal polypi.

Hæmorrhagic Pericardial Effusion.

By P. S. MARTIN.

H. C., MALE, aged 28. Admitted to University College Hospital with an extensive pericardial effusion, and collapse of the left lung. The history was that of indigestion for eight weeks with one outbreak, for three days, of severe vomiting, which was followed by persistent dyspnœa.

The pericardium was opened under local anæsthesia, and 2½ pints of almost pure blood were removed. The effusion gradually recurred, and four weeks later there was a return of the dyspepsia. Again the pericardium was opened, and 3 pints of similar fluid withdrawn. The effusion has since been steadily re-accumulating, but up to the present date, three weeks after the second operation, has given rise to no return of the acute symptoms. Throughout the illness there has been no fever, with the exception of a few days after the first operation.

There is no history of rheumatism, but on admission the heart was in a state of auricular fibrillation, suggesting myocardial inflammation. This ceased soon after the first withdrawal of fluid and has not recurred. There is no evidence of tubercle elsewhere in the body ; the pericardial fluid was sterile, but an injection of tuberculin for diagnostic purposes has caused a definite rise of temperature. Neither by X-ray nor by ordinary physical examination could any sign of aneurysm or new growth be detected.

Pericardial friction was heard only before and shortly after the first operation, and it has now ceased. The patient is in fair general health.

DISCUSSION.

Dr. MARTIN added that the chief interest attaching to the case was as to the cause of the condition. The effusion had recurred twice after being removed, and it had now again reached its original degree. He did not think it possible to state the exact cause. There was no evidence, either clinically or by means of X-rays, that the condition was due to aneurysm or to new growth. The only positive evidence obtained was that the patient had given a very marked reaction to the injection of tuberculin, which was given for

diagnostic purposes. The reaction was both local and general. The effusion was now considerable again, and he did not know whether draining it would be of any great or permanent benefit.

Dr. JAMES RAE asked how the amount of effusion in this case compared with that in other cases in which there was effusion into the pericardium. He could remember a case of tuberculous pericarditis in which there were two and a half or three pints of effusion, accompanied by severe vomiting, which he explained as due to pressure on the sympathetic nervous system. There was no blood in the effusion, but there was some pyrexia, which later on quietened down. With regard to hæmo-pericardium, he had seen it stated that the rapid effusion of blood to the extent of 10 oz. into the pericardial sac was fatal. He would be glad to hear the opinion of others on this point. A case of historical interest was that of George II, in whom there was rupture of the right ventricle, and after death almost a pint of effused blood was in the sac. In addition to the rupture there was discovered a dissecting aneurysm of the aorta.

Arteriovenous Anastomosis for Gangrene due to Syphilitic Endarteritis.

By H. MORRISTON DAVIES, F.R.C.S.

R. F., MALE, aged 46, had noticed pain and discoloration of the middle toe of the left foot since March, 1911. After admission under Mr. Pollard on June 21, he had an attack of delirium tremens. There was dry gangrene of the middle toe of the left foot with a line of demarcation round the base. The foot was cold and painful. No pulsation in the tibial arteries; pulsation occasionally felt in the popliteal and well marked in the femoral artery. No gangrene of the right foot, but no pulsation to be felt in the tibial arteries. General conditions otherwise fair; heart slightly hypertrophied; trace of albumin in the urine.

July 3: Amputation of gangrenous toe under gas; wound left open and plugged.

Patient came under my care on August 1, and there was then some discoloration of the great toe and dorsum of foot on the left side. On August 13 a patch of gangrene appeared on the inner side of the fourth and outer side of the second toe. Purple discoloration of the whole of the great toe, over the head of the fifth metatarsal, and over the dorsum of the foot. The whole foot was cold and painful. Amputation wound

still unhealed. No pulsation in the left popliteal artery. Trace of albumin still present in the urine. On August 15 I did an end-to-end anastomosis of the superficial femoral artery and vein at the upper end of Hunter's canal. Mattress sutures tied over a loop of catgut were used, and the cut edges of the vessels everted. The sutures were of extremely fine silk and were soaked in oil. There was no leakage from the junction. The proximal end of the vein and the distal end of the artery were ligatured. Wound closed and healed by first intention. Anæsthesia by stovaine, supplemented later by ether. Duration of operation, one and a quarter hours.

On the following day the foot was quite warm and painless, and since the operation there has never been any œdema. The discoloration disappeared and the gangrenous patches came away leaving healthy skin behind. Now, eight weeks after the anastomosis, the foot is warm, there is no pain, discomfort, or œdema, and the patient has been walking on it for the last four weeks.

DISCUSSION.

Mr. DAVIES said he only wished to add one or two points with regard to the technique. What he regarded as important in these cases was the absence of tension on the sutured vessels. In many of the cases in which an end-to-end anastomosis had been done, each end of the artery was anastomosed to each end of the vein, but as there could be no flow coming back along the distal part of the artery to the proximal part of the vein, these need only be ligatured. In dividing the artery and vein it was well to divide the artery a centimetre lower than the vein, so that when the two ends were brought together no tension was exerted upon the stitches. As far as he had been able to trace on looking through the literature, there had been only twenty-eight cases in which the procedure was done for actual gangrene and about forty-five for threatening gangrene. In only four of the twenty-eight could one say there had been anything like success, while in ten there was partial success—i.e., improvement for a week or two. Of the four cases, one was done in 1903 by San Martin, of Satierstegni, but in that case a Syme's amputation was done at the same time. Mr. Ballance's case, which was shown before the Society, was the second case, and that patient died in five months from an abdominal lesion. The third case operated on by Glasstein was one in which there was gangrene of one toe, and twelve weeks afterwards the improvement following on the anastomosis was still maintained. In his own case, which could be included among those which were successful, the patient had been walking about for four weeks, and it was eight weeks since the anastomosis was done.

The coldness had disappeared, the purple discoloration which was on the dorsum of the foot and over the great and little toes, and the gangrene which was present on the adjacent side of the second and fourth toes, had cleared up, and there was no longer any pain. The patient had had syphilis, and was a heavy drinker.

The CHAIRMAN desired to congratulate Mr. Morriston Davies on the excellent result he had obtained. It would be interesting to hear whether any other Fellow had a similar case to relate.

Mr. NORBURY said he recently saw a case of a similar kind, that of a man who was operated upon about six months ago at St. Thomas's Hospital. He had senile gangrene of two or three of his toes, and an attempt was made to stop the process by means of arteriovenous anastomosis. An end-to-end anastomosis was done, in the way which Mr. Morriston Davies described, and although the trouble cleared up to some extent and the process seemed to stop for two or three months, after that time an amputation was necessary, and lower down than would probably have been required if amputation had been done in the first instance. The amputation was done just above the ankle, but before that it was thought it would be necessary above the knee. He saw the patient six months after the operation—i.e., recently, and there was no sign of gangrene of the stump. An attempt was made to slide the vein over the artery in the first instance, but there was too much tension, and so an end-to-end operation had to be performed.

Clinical Observations on an Epidemic of Acute Poliomyelitis in Cornwall.

By ALEXANDER GREGOR, M.D., and L. B. HOPPER.

THAT acute anterior poliomyelitis has appeared in this country in a serious epidemic form is conclusively proved. There was a series of cases in Bristol from June, 1909, to January, 1910, as reported by Parker in the *British Medical Journal* of March 18, 1911. The disease during the present year has become epidemic in the counties of Devon and Cornwall. In the former county the disease has been more severe than in Cornwall, there having been about ninety known cases in Devon, while in Cornwall the number of ascertained cases to the present date is forty-two.

The first and the majority of the cases coming under our own observation occurred in Penryn. This town has a population of about 3,000 inhabitants and is situated at the head of one of the tidal estuaries of the River Fal—viz., the Penryn river—about two miles above Falmouth. It is a very ancient town and the inhabitants mostly working-class people. The majority of the houses are old, huddled together, and in many cases are in a very insanitary condition. The sewage is water-carried and is discharged into the creeks in a crude condition, and is left exposed on the ebb tide. Little attempt is made by the sanitary authorities to remedy matters, the streets being badly scavenged and the town generally dirty. The water supply is of doubtful quality.

The first case occurred on January 23. The next case was noted on May 27 (Case XII), and in connexion with it, it is interesting to note that a horse belonging to the patient's father had what is locally known as "poke-neck." It is said to have been paralysed in the neck and forequarters. It fell down in the stable and was unable to rise. When taken out, with assistance, it again fell. It was shot without having been seen by the veterinary surgeon. This happened a week before the child was taken ill. Case V occurred on June 8. This boy's father had been in the house of Case XII.

Of the nine cases in Penryn, five were in West Street, which is narrow street on the main road, with a considerable amount of

motor traffic. The road has been tar-sprayed for the first time this summer, so that the dust nuisance has been reduced to a minimum. The occupants generally are poor and not over cleanly in their habits. Though we have been unable to trace contact in all those cases, there is strong presumptive evidence that such did take place, as the street is the only playground of the children. No doubt, too, there is much visiting among each other's houses; and also one of the cases occurred in a grocer's shop. Two cases occurred in the village of Mylor, also situated at the head of another tidal estuary of the Fal. There was distinct contact of one of these cases with another case in Truro. One occurred in Budock and two in Falmouth, four in Camborne and two in St. Austell. No two cases occurred in the same house, with the exception of the St. Austell cases.

It is interesting to note that the children in Penryn bathe regularly in the sewage-contaminated creeks. Of those attacked, no single patient had at any time bathed or waded in this polluted spot. This is quite contrary to the experience of the Massachusetts epidemic, where 62 out of the 150 were proved to have been swimming or wading in polluted waters.

Another interesting observation during the course of the Penryn epidemic was the number of cases of herpes zoster occurring in the practice of one of us (Hopper). The majority of the persons affected were old, and mostly women. Various parts of the body were affected by the eruption—viz.: (1) Six had lesions over the neck, shoulder-blades, deltoid and pectoral muscles; (2) four over the intercostal muscles; (3) three over the recti and buttocks; (4) three on the lower limbs (total of sixteen). In most of them constipation was a marked feature, and in many of the cases there was severe pain and general malaise. In no other space of time have so many cases occurred in this district, and therefore the question naturally arises, is the same organism at work in both diseases?

The *ages* of the patients ranged from ten months to fifty years. The rejection of the term "infantile paralysis" is now therefore fully justified, seeing that persons of all ages are subject to the disease. With one exception, however, those who have lost the use of their limbs, or have their limbs permanently impaired, are under the age of eight. All cases of the encephaloid type have been fatal.

Sex.—Fourteen males, seven females.

Mortality.—There were five deaths in the eighteen cases recorded, giving a mortality of 25·5 per cent.

Symptoms.—The most outstanding symptom, and one which occurred in every case, was derangement of the intestinal tract: there was either constipation or diarrhœa, or both. In all cases the motions were exceptionally large, and very foul-smelling. In contradistinction to this, except in five cases, three of which had epistaxis and two slightly reddened fauces, there was no affection of the naso-pharyngeal mucous membrane. Two cases had slight enlargement of the cervical glands. One case was diagnosed as summer diarrhœa. This is certainly against Flexner's theory that the virus enters the body through the air-passages, and would rather point to the alimentary canal as the point of entrance. This view is strengthened by the fact that there were six cases in which retention of urine was observed and one had incontinence. One case had to be catheterized for eight weeks. Great irritability and pain were marked features, and in some cases the latter was most distressing, the patient crying out for hours; the least movement of the paralysed limbs increased this. A rash was present in six cases which were seen through all the stages of the disease. In every case it was typically urticarial, a fact which again supports the theory that the infection is intestinal. The temperature never rose above 103° F., generally it was about 100° F. in the observed cases; but it must be borne in mind that some of the cases were not seen till the initial stage had passed and the paralysis had developed.

Only one case recovered completely, all the others will be disabled more or less for life.

We have arranged the cases into groups in a more or less arbitrary manner. There is little distinction between the encephaloid type and the others, which in reality only differ in degree and often pass the one into the other, depending apparently on the virulence of the organism, the resisting power of the patient, and the part of the nervous system attacked.

Group I.—Encephaloid Type.

Case I.—E. K., female, aged 30. No occupation. Patient suffered from habitual constipation with consequent digestive troubles, and was inclined to be hysterical. First taken ill on July 11, and then complained of headache over the right parietal region. This continued till she was first seen on July 18, when the following was noted. Temperature 99 F., pulse weak, but regular. Pupils equal, but dilated; reaction normal; reflexes exaggerated; very constipated.

July 19 and 20: No change. Marked restlessness and no sleep. Temperature normal.

July 21: Menstruating. Headache less. Temperature 100° F.

July 22: Got out of bed, fell down, and was found in dazed condition. Passed fæces and urine in bed. Pain in all joints. Reflexes exaggerated; pupils normal. Temperature 102° F.

July 23: At intervals patient very drowsy; at other times hysterical, clapping hands, shouting and crying; right arm spastic. Headache severe; retraction of head and neck, with great pain on movement. Knee-jerks much exaggerated. Urticarial rash over abdomen. Will not recognize anyone. Temperature 102·4° F.

July 24: Patient very irritable; headache very severe. "Tache cérébrale" well marked. Rash still present. Kernig's sign negative. Right arm rigid.

July 25: Patient seems better to-day. Passed a huge, very foul-smelling motion as the result of calomel and enema; tongue clearer. Temperature 100° F., pulse 90. By evening drowsy and difficult to arouse.

July 26: Drowsy, with intervals of delirium. Urine and fæces passed in bed. All limbs rigid, also head and neck. Tache cérébrale still well marked.

July 27: Becoming comatose. Limbs still rigid.

July 28: Lies motionless with all limbs flaccid, but with the exception of the right arm they all move on stimulation. Squint well marked. Swallowing now becoming difficult. Left pupil more dilated than right. Urticarial patches still on abdomen, which is much distended.

July 29: Squint still present, but not so marked. Limbs all move voluntarily; some rigidity of left arm. Swallowing still difficult. Comatose, but can be roused. Fæces, urine, breath, and sweat extremely foul-smelling.

July 30: Squint gone; other conditions the same.

July 31: Drowsy, with intervals of consciousness.

August 1: Died comatose; sensation was complete throughout.

Case II.—G. B., male, aged 3 months (Dr. Tonking's case, Camborne). Taken ill on August 27, with pain in head and vomiting; seen by Dr. Tonking on August 28 at 11 a.m. Pain in throat; salivation due to being unable to swallow. No Kernig. Neither plantar nor knee-reflexes obtainable. Died at 10 p.m. from paralysis of respiration.

The diagnosis of the two following cases (Cases III and IV) may be disputed, but the result of the examination of the cerebrospinal fluid is consistent with anterior poliomyelitis; hence they are included.

Case III.—A. J., male, aged 25 (Dr. Lanyon). A perfectly healthy man up till the time of his fatal illness; no history of tubercle. On September 22 he woke with a severe frontal headache and retched a good deal; lay in bed till 11 a.m.; got up and went a short voyage in the Bay. On the way home he felt sick and vomited (not sea-sickness). Arrived at the dock at 5 p.m. Too ill to walk home, but walked up the flight of steps to a cab; walked from cab to bedroom unaided. Seen by medical man at 5.30 p.m. Pulse 42.

Lying curled up in a very lethargic state; feet cold; did not speak, but put his tongue out when told; vomited a brown grumous fluid. No reflexes or other symptoms noted. Became comatose in a short time and died at 8 p.m. the same evening.

Post-mortem on the following day: All organs perfectly healthy except the brain. Meninges much congested. Lateral ventricles distended with fluid, 5 or 6 oz.

Preliminary Report from the Pathological Department of St. Bartholomew's Hospital.—The cerebrospinal fluid is turbid; it smells offensively; albumin, 0·25 per cent.; Fehling not reduced; centrifugalized deposit; cells; bacteria and small Gram-negative bacillus swarming in this sample of cerebrospinal fluid. These were found to be *Bacillus coli* and staphylococci due to contamination.

Case IV.—O. N. B., male (Dr. Tonking). Taken ill August 28. Temperature 100° F. to 101° F. Vomiting.

August 30: Very irritable. Tache cérébrale well marked. Kernig present on both sides. Fundi normal. Paralysis of external recti (squint); no other paralysis.

September 2: Semi-comatose: gradually deepening stupor.

September 3: Coma; died.

Report from the Pathological Department of St. Bartholomew's Hospital.—The cerebrospinal fluid is clear. The albumin content is 0·1 per cent. Fehling's solution is reduced by the cerebrospinal fluid. The centrifugalized deposit shows very few leucocytes. All that were seen were lymphocytes. The centrifugalized deposit shows numerous clumps of mycelium of a mould, and also a large number of Gram-negative motile bacilli, and a few of staphylococcus. Numerous fibres of the deposit were stained for tubercle, but no tubercle bacilli were seen.

Comments.—Cultures are being made, and a report will be sent later. The case is clearly not one of cerebrospinal meningitis, for the cells present are lymphocytes, and these are but few in number. The albumin content is only very slightly raised above the normal amount. It is identical with the quantity found in cerebrospinal fluid of poliomyelitis cases recently examined from the same district. This cerebrospinal fluid is probably from a case of poliomyelitis, but it is not possible to be sure that the case is not tuberculous meningitis in an early stage. In that case, however, I should have expected to find more lymphocytes, rather more albumin, and the tubercle bacillus. The moulds of bacteria present are almost sure to be contaminations.—M. H. GORDON.

GROUP II.—GRADUAL ASCENDING AND DESCENDING TYPE.

Case V.—E. C. C., male, aged 2½. Taken ill on June 8 and first seen on June 10, when it was impossible to arrive at a diagnosis. The following conditions were noted: Very constipated, with coated tongue, no nasal discharge,

no sore throat, herpes, convulsions, twitchings, hyperæsthesia, or delirium. Passed no urine for thirty-six hours, and then for twenty-four hours. Epistaxis on three occasions; Kernig's sign present, also tache cérébrale well marked. Urticarial eruption. Temperature ranged from 101° F. to 102·4° F.

On June 14—i.e., on the sixth day of the illness—paralysis appeared in both legs. Complained of a great deal of pain in the legs and in the lumbar region and quite unable to raise himself. Plantar and patellar reflexes absent, testicular and abdominal present. No loss of sensation.

For a period of three weeks there was no recovery of any group of muscles. Then the weakness of the erector spinæ passed off and the child could pull himself up in his crib and kneel on the right knee. Then the extensors of the right thigh began to regain power. Seven days later the right gastrocnemius began to recover, and each day after this fresh groups began to show returning power. In the left leg, however, no change was observed till five weeks had elapsed. Here, too, the extensors made the start, followed by the gastrocnemius, and finally the tibialis anticus.

On September 11 the peronei were still paralysed.

Case VI.—W. E. M., male, aged 1 year 4 months. Patient taken ill on July 18 and first seen July 26. The illness commenced by the child being fretful and irritable, with a slight attack of fœtid diarrhœa. There was an urticarial rash on the abdomen for four days. No vomiting, sore throat, or glands. Slight retraction of the head and neck was noticed. On the second day of his illness the left side of the face was paralysed and drawn to the right. This disappeared in five days. On the fourth day the left arm was totally paralysed, and two days later the left leg. When first seen the temperature was normal. The child was constipated, with coated tongue. Kernig's sign and tache cérébrale present. Reflexes present. The left arm and leg were paralysed, and great pain was elicited on moving those limbs.

The subsequent history of the case was as follows: The power in the flexors of the upper arm returned first, the flexors of the fingers following. The remaining groups gradually recovered, though passive movement caused pain for five or six weeks. In the leg the thigh muscles, first the flexors, then the extensors, recovered, followed by the gastrocnemius and the tibialis anticus.

On September 5 the child could use the arm perfectly and could walk. He dragged the left leg, which was somewhat everted.

Note.—Immediately prior to the illness the mother, with the child, visited a house where there was a case of poliomyelitis. The child was handled by the person who was nursing this case.

Case VII.—G. B., male, aged 8.

August 21: Had usual bath at 7.30 and was apparently well.

August 22: Felt tired before getting up. Later on got up and went short train journey. Complained of headache and wanted to lie down. Taken home and put to bed; eyes suffused. Temperature 103° F. Herpes showing on lips. No Kernig, no tache cérébrale, no rigidity.

August 23: Herpes increased. "Anxious" on feet and rigid on standing. Stiffness and pain in back of neck. Kernig's sign well marked, producing pain in hamstrings. Tache cérébrale in five seconds. Right knee-jerk absent, left faint.

August 24: Temperature 103° F., pulse 132. Tache cérébrale, two seconds. Pupils dilated, headache, constipation, slight epistaxis. Both knee-jerks and plantar reflexes absent. Abdominal and cremasteric reflexes sluggish; head retracted; right leg completely paralysed, left leg affected. Very restless and irritable during the night. Severe epistaxis.

August 25: Temperature 101° F., pulse 140. Patient very restless and irritable. Bowels moved slightly, very offensive. Attacks of breathlessness, breathing becoming Cheyne-Stokes-like during the night. Speech staccato. Left leg totally paralysed.

August 26: Temperature normal, pulse 92. Slightly delirious. Bowels moved, very offensive. Difficulty in commencing the act of micturition. A great deal of pain in head, knees, and legs.

August 27: Intervals of severe pain. Urticarial rash on buttocks. Both arms affected, especially the right deltoid.

August 28 and 29: Still in great pain. Huge offensive motions.

August 30: Less restless. Pain most marked in the hamstrings.

August 31: Pain less; distinct improvement in movement of left leg: can slightly flex the toes.

September 4: Power returning to right arm. Left leg rigid.

September 5: Both legs can now be straightened without pain; left foot still rigid.

September 6, 7, and 8: Still having huge, constipated motions, very offensive.

September 9: Can now flex toes of both feet. Extensors of left thigh gaining power.

September 11: Patient now quite free from pain. Feeling of needles and pins in lower limbs.

September 22: Right arm almost normal. Flexors of right foot slightly active: flexors of left foot and extensors of left thigh have a certain amount of power.

Case VIII.—S., male, aged 1 year 11 months. First taken ill in July. He was out of sorts for several weeks. During this time there were no convulsions, but there was vomiting and diarrhœa. The mother then noticed the child was weak in his back: he could not sit as usual. Then both legs became paralysed, and a little later the right arm, followed by wryneck on the right side. Recovery took place in the following order: first the neck, then the back, and lastly the legs.

On September 23 the whole of the right arm remained paralysed, with the exception of slight movement of flexors of fingers.

Case IX.—L. M. R., female, aged 4. (Dr. Tonking.) Already suffering from pertussis. Ten days before death vomited, and next day one arm was paralysed, and on the day following the other arm was involved. The whoop disappeared a few days before her death owing to involvement of the respiratory centre. Death was due to paralysis of the respiratory muscles.

Case X.—Male, aged 3. (Dr. Burton, St. Austell.) When first seen had history of diarrhœa off and on for two weeks. No vomiting. Said to have "gone off his legs." On examination: Temperature subnormal, pulse 140, tongue coated, slight enlargement of cervical glands, and fauces reddened. There was a distinct squint, which mother thought he had had for some time. Pupils equal and reacting. Movement of legs in bed seemed good, but patient unable to stand; no pain. Tache cérébrale and Kernig's sign present; knee-jerks absent; output of urine small. Patient would go for whole day without passing water. He remained the same for two or three days, then a slight retraction of the muscles of the back of the neck was noticed, with pain on movement. The boy lay curled up and kept the thighs flexed on the abdomen, and the legs on the thighs. He resented interference and complained of pain in the legs, abdomen and back. There is now (October 1) slight wasting of deltoids, and extensors of legs and thighs, with feet-drop. The boy is gradually improving, can sit up, but cannot stand.

Case XI.—The notes of this case have been kindly sent by Dr. Tonking, of Camborne. "About a month ago I saw a child, a male, aged 6 months, with vomiting and diarrhœa. I attended him about a week. The daily temperature was 100° F. I left the child apparently cured of the summer diarrhœa. Two weeks later the mother sent for me as the child could not move his leg. I find flaccidity of both legs and absence of reflexes. This was obviously a case of poliomyelitis of the spinal type overlooked during an epidemic of summer diarrhœa."

Group III.—Sudden Type.

Case XII.—D., male, aged 6. Child was first taken ill on May 27, when he was observed falling about in the house. He went out, when he again fell, and was carried home. When seen, temperature 101° F.; no vomiting, convulsions, epistaxis, sore throat, or rash. Constipated, with coated yellow tongue. Retention of urine thirty-six and twenty-four hours. Complained of headache, stiffness, and pain of muscles of back, and in the ankle- and knee-joints. Very irritable and constantly calling out. Hyperæsthesia over back and legs. All four limbs totally paralysed.

May 30: Some slight flexion of fingers of right hand noted. Temperature 100° F.

May 31: Temperature normal. Biceps and deltoid of right arm have slight movement. Great pain at night and very restless.

June 5: Can move toes on right side, also flexors of right thigh. Can lift his right arm to his mouth.

June 13: Can flex fingers of left hand. No pain now at night.

On June 23 admitted to hospital, where he was treated with massage.

On September 14 the condition was: Left arm completely paralysed with the exception of the flexors of thumb, ring and index fingers. Left leg completely paralysed. Right leg muscles of thigh normal; all muscles below knee with the exception of the plantar muscles gone.

Case XIII.—F. F. M., female, aged 2 years, 3 months. First taken ill on August 3, and seen on August 7. Complained of headache and vomited during those three days. When seen, temperature 100·2° F., constipated, very irritable, and crying constantly, no epistaxis, no throat trouble, herpes, rash, or retraction, Kernig's sign and tache cérébrale absent. Reflexes absent. Both legs and left arm totally paralysed and very painful. The paralysis came on very suddenly in this case. The child was at the wash-basin and fell while crossing the floor for a towel. When picked up she was paralysed in both legs and the left arm was found paralysed the next day. All the muscles of the legs were paralysed, and improvement was noticed in the extensor and plantar muscles and later in the flexors. On September 12 the child could stand by the aid of a chair, the peronei still being functionless. All the muscles of the upper arm were affected and the flexors of the forearm. These latter regained function on August 10, and on September 12 the condition was the same.

Case XIV.—Male, aged 10 months. First taken ill about July 6. Vomiting, one convulsion, extreme drowsiness, dilatation of both pupils. Diarrhœa followed by constipation. Head and neck retracted. Right arm paralysed. Other symptoms not obtained.

On September 14 child had complete loss of power in the right arm, with the exception of slight flexion in the fingers, where contraction is beginning.

Case XV.—J. C., male, aged 12 months. Taken ill on June 29 and not seen till a month later. Was ill for four days, when it was noticed that he was unable to raise his left arm. No history could be elicited of convulsions, drowsiness, delirium, epistaxis, sore throat, or retraction of head and neck. There was irritability of temper for the first ten days, constipation, and an urticarial rash.

When first seen on July 29 he had all the movements of the left arm except those connected with the deltoid, which were very much atrophied.

On September 14 this muscle had almost disappeared.

Case XVI.—A. C. Y., male, aged 1½. First taken ill on June 9, and first seen on June 16. The child was fretful and irritable, and on June 13 had retention of urine for thirty-six hours. Paralysis of both legs was noticed. When seen on June 16 no Kernig's sign or tache cérébrale. Plantar and knee-

reflexes absent. Extreme tenderness in legs and lumbar part of spine, and both lower limbs paralysed. Sent to hospital and returned in the first week of September.

On September 6 no improvement in any muscles of left leg. Some slight extension of right thigh.

Case XVII.—R. L. V., male, aged 1 year 10 months. First taken ill on June 8. Headache, very constipated, very irritable, then drowsy, tongue coated and dry. Retention of urine. On the third day total paralysis of the right leg appeared, with paresis of the erector spinæ. This was complete for ten days. Then the flexors of the toes became active, followed by the extensors of the thigh and gastrocnemius, and gradually all except the peronei. Contracture of the flexors of the toes occurred, but this has been overcome.

On September 15 the child was healthy and could walk, though the peronei are still affected.

Case XVIII.—R. I., male, aged 13 months. Taken ill on August 23, and first seen on August 24. Temperature 100° F. Constipated. Urticarial spots on abdomen. He was playing with other children when it was noticed that he could not walk about as usual. On examination it was found that the peronei of the right leg were paralysed. No other group was involved.

September 12: The child can walk, but there is distinct eversion of the right foot.

Case XIX.—X. Y., female, aged 50. This patient suffered from epilepsy in early life and had polypi removed from the uterus. For many years she remained free. When the climacteric began, at the age of 44, she had occasional fits. On January 23, on getting out of bed she fell down, cutting her forehead, and was picked up in a dazed condition. On being put to bed it was found that the right leg was totally paralysed and the left leg partially. Reflexes lost. Paralysis of rectum and bladder, which lasted for eight weeks. Limbs painful and great irritability. Reflexes lost. On September 25 could stand and walk with assistance. Knee-jerks present, plantar-reflexes still absent. There is a good deal of wasting in all the muscles, especially those of the left calf. Right foot droops considerably.

Case XX.—Female, aged 7. (Dr. Burton's case, seen at the same time as Case X, and in the same house.) Had diarrhœa for several days. In afternoon complained of pain, but went to school. At 7 p.m., when seen, child was shivering and looked ill. Temperature 102° F., pulse 108. Headache; pain in back and legs; cervical glands enlarged; could hardly walk across the room; knee-jerks were doubtful, right present, perhaps slightly exaggerated, left not obtained; Kernig's sign present. Next day both legs completely paralysed. Temperature remained at 102° F. Knee-jerks absent; great pain on movement.

SYNOPSIS OF

Group	Case	Date	Name	Age	Sex	Temperature	Headache	Vomiting	Convulsions	Irritability	Condition of throat and nose
		1911		Years		° F.					
I	I	July 11	E.K.	30	F.	99-102	Yes	No	Twitchings	Yes	Negative
I	II	Aug. 27	G.B.C.	3	M.	—	Yes	Yes	—	—	—
I	III	Sept. 22	A.T.	25	M.	—	Yes	Yes	—	—	—
I	IV	Aug. 28	O.N.B.	—	M.	100-101	—	Yes	—	Yes	—
II	V	June 8	E.C.C.	2½	M.	100-102·4	No	No	No	Yes	Epistaxis
II	VI	July 18	W.E.M.	1 4/12	M.	Normal	No	No	No	Yes	Epistaxis
II	VII	Aug. 22	G.B.	8	M.	103	Yes	Yes	No	Yes	Epistaxis
II	VIII	July	S.	1 1/12	M.	—	—	Yes	No	—	Negative
II	IX	—	L.M.R.	4	F.	—	—	Yes	—	—	Negative
II	X	Sept.	—	3	M.	Subnormal	No	No	No	No	Slight reddening of fauces
II	XI	Sept.	—	6/12	M.	100	—	Yes	—	—	—
III	XII	May 27	D.	6	M.	101	Yes	No	No	Yes	Negative
III	XIII	Aug. 3	F.F.M.	2 3/12	F.	100·2	Yes	Yes	No	Yes	Negative
III	XIV	July 6	M.	10/12	F.	—	Yes	Yes	Yes	Yes	Negative
III	XV	June 29	J.C.	1	M.	—	—	No	No	Yes	Negative
III	XVI	June 9	A.C.Y.	1½	M.	Normal	—	No	No	Yes	Negative
III	XVII	June 8	R.L.V.	1 10/12	M.	—	Yes	No	Yes	Yes	Negative
III	XVIII	Aug. 3	R.I.	1 1/12	M.	100	No	No	No	Yes	Negative
III	XIX	Jan. 23	X.Y.	50	F.	—	Yes	—	Yes	Yes	Negative
III	XX	Sept.	—	7	F.	102	Yes	No	No	Yes	Slight reddening of fauces
IV	XXI	June 18	T.	16	F.	Normal	No	No	No	No	Negative

SYMPTOMS.

Rash	Constipation, diarrhœa	Retention of urine	Kernig's sign	Tache cérébrale	Reflexes	Limbs affected	Muscles affected	Result
Urticarial	Constipation	Yes	No	Yes	Exaggerated	—	—	Fatal
—	—	—	—	—	Absent	—	Respiratory	Fatal
—	—	—	—	—	—	—	—	Fatal
—	—	—	Yes	Yes	—	—	External recti	Fatal
Urticarial	Constipation	Yes	Yes	Yes	Absent	Both legs	All	Peronei affected—one leg
Urticarial	Diarrhœa and constipation	No	Yes	Yes	Absent	Left arm and leg; face	All	Peronei affected
Urticarial	Constipation	Yes	Yes	Yes	Absent	Both legs and right arm	All leg muscles and deltoid	All leg muscles
No	Diarrhœa	No	—	—	Absent	Both legs and right arm; wryneck, right side	All	Arm except flexors of fingers
No		—	—	—	Exaggerated	Both arms and muscles of respiration	All	Fatal
No	Diarrhœa	Yes	Yes	Yes	Absent	Both legs and arms and face	External recti; deltoids; extensors of thighs and legs	—
—	Diarrhœa	—	—	—	Absent	Both legs	All	—
Urticarial	Diarrhœa and constipation	No	Yes	Yes	Absent	Both arms and legs	All	No recovery
No	Constipation	No	No	No	Absent	Both legs and left arm	All	All except peronei of one leg and one arm
No	Diarrhœa and constipation	No	—	—	Absent	Right arm	All	Complete paralysis and contraction of fingers
Urticarial	Constipation	No	No	—	Present	Left arm	Deltoid	No recovery
No	Diarrhœa	No	No	No	Absent	Both legs	All	No recovery
No	Constipation	Yes	—	—	Absent	Right leg	All	Recovered, all except peronei
No	Diarrhœa	No	—	—	Present	Right leg	Peronei	No recovery
No	Constipation	Yes	—	—	Absent	Both legs	All muscles	Partial recovery
—	Diarrhœa	Incontinence	Yes	Yes	Absent	Both legs and arms	All leg and deltoids	—
No	No	No	—	—	Present	Left arm	Deltoid and biceps	Recovered

The following day there was retraction of head and neck, well marked; also tache cérébrale. The slightest movement of the head caused great pain, and the spine was somewhat bowed. Lumbar puncture was performed the same night, 2 dr. to 3 dr. of fluid being withdrawn, clear, and came away drop by drop. This proved to be sterile. The next day the child seemed somewhat relieved. For two or three days, however, the child was very irritable, diarrhœa was present, and fæces and urine were passed involuntarily. There was slight delirium at night. Lumbar puncture was again performed, 6 dr. to 7 dr. of fluid being withdrawn, this time under marked pressure. Dr. Gordon reported on this: Albumin, 6 per cent.; sugar, *nil;* films and cultures sterile; lymphocytes but no leucocytes present.

The lumbar puncture was followed by marked improvement in the child's condition. Pain lessened and the child much more comfortable.

The child is now left with both legs paralysed, with wasting, especially of the vasti and the anterior tibial muscles. Dropped foot on both sides and no movement of either leg. The deltoids are slightly affected.

Group IV.—Transient Type.

Case XXI.—T., female, aged 16. First taken ill on June 18 and seen on June 20. She complained she had loss of power in the left arm. No temperature, rash, or headache. In short, there was no constitutional disturbance. There was complete loss of power in the left deltoid and biceps, accompanied with pain.

June 26: Power of movement returning, but still painful.

June 30: More power and less pain.

July 6: Good movement and some slight pain. Returned to work.

DISCUSSION.

Dr. Gregor, in answer to the Chairman, said that most of the small places in which the cases occurred were dirty and the drainage and sanitary arrangements were defective. That did not apply to Falmouth, where the conditions were diametrically opposite.

Dr. F. E. Batten said the authors had dealt with the matter chiefly from the clinical aspect, in relating the cases which they had encountered. He wished first to offer some criticism in regard to those cases. Undoubtedly, the authors had recorded cases of poliomyelitis, but they had recorded some cases, especially in the "encephaloid" group, which were open to criticism. In one case Dr. Gregor sent up the cerebrospinal fluid to Dr. Gordon, who had reported it as fœtid and contaminated with micro-organisms: such a fluid could have been of no use from the diagnostic point of view. It was, indeed, doubtful whether the case was one of poliomyelitis and, considering that the fluid was

turbid, it seemed much more likely that it was a case of ordinary meningitis. This was a mistake which was likely to be made in dealing with such epidemics; people knew that an epidemic of poliomyelitis was occurring, and there was a tendency to put down as poliomyelitis or polioencephalitis every cerebral case in hich there was any doubt as to diagnosis. Many of the cases of ordinary meningococcal and other forms of meningitis which constantly occur in this country were attributed to polioencephalitis. The distinction could be made with the greatest ease, for lumbar puncture enabled one to make a definite diagnosis between meningococcal meningitis and polioencephalitis or poliomyelitis. It would have been important, if possible, to have obtained post-mortem examinations in the other cases which Dr. Gregor called encephaloid disease, but in private practice it was very difficult to get permission for such examinations, and perhaps still more difficult to obtain a reliable microscopical examination of the cerebral tissue. The literature contained extraordinarily few cases of polioencephalitis which had been competently examined microscopically. One of the most interesting points in the paper had reference to sewage-contaminated water and the proximity of the cases to rivers and other water. It was curious how poliomyelitis did occur in seaport towns and in towns near rivers, and how the disease would pass down a river, so that there were cases at different points along its banks. This had been shown to be so in America, and it was also rather a striking feature in this country. The authors of the present paper appeared not to have been able to trace any contact with water; the children affected had not been bathing. In the year 1910 there were numerous epidemics throughout England: an extensive one in Cumberland, embracing Carlisle, Barrow-in-Furness and Maryport, and places along the coast; there was another in Dorsetshire, at Cerne Abbas and Weymouth, and another epidemic in Melton Mowbray, of which no details could be obtained, because it was still in the hands of the Local Government Board. Wickman had recorded an outbreak of the disease which affected fifty persons out of a population of 600; the cases could be traced to the school. It would be interesting to know if the children affected were of the school age, and it would be important to know whether they attended school. Many of the children might be below school age, but, on the other hand, they might have brothers and sisters who were attending school, and with them they might have been in contact; an investigation of the school question would have been very interesting, and any information on that point would be valuable. The authors referred to the question of herpes occurring at the same time as the poliomyelitis. Garrow had called attention to the same point in the epidemic in the Cumberland district in 1910. Garrow investigated the Cumberland district, and he found there were many cases of herpes in adults at the same time as poliomyelitis occurred in children. The association of herpes with poliomyelitis was a very interesting question. Had these two conditions any connexion with one another? He thought that must remain for subsequent proof. Head regarded them as one and the same condition. It had not been proved that the toxin which caused poliomyelitis would also

produce herpes, nor had the virus been obtained from posterior root ganglia The opportunities of investigating the posterior root ganglia were very few, and everybody knew how rare it was to obtain a post-mortem examination on a case of poliomyelitis. To transmit the disease to the monkey was the only method at the present time of actually verifying the nature of the disease. Instances of animal paralysis, such as horses being paralysed in their limbs, were not infrequent, but it was very difficult to associate the two conditions, and since horses were insusceptible to the virus of poliomyelitis it was difficult to see how they could be a source of the infection to man. It seemed that no other animal except the monkey and the ape was susceptible to the virus of poliomyelitis. Attempts had been made to show that rabbits were susceptible to the virus, but the experiments were open to question. The mortality was very high, but that might have been because there were abortive cases in which the fever was transient and the paralytic symptoms slight, which had not come under the notice of the authors. The term "acute poliomyelitis" was generally accepted, and under it should come all the cases which the authors had so well described in the paper.

Dr. GREGOR, in reply, said all the children affected were either at school, or had brothers and sisters at school.

Clinical Section.

November 10, 1911.

Sir WILLIAM OSLER, Bt., F.R.S., President of the Section, in the Chair.

Case of Enlarged Spleen with Recurring Hæmatemesis.

By HERBERT P. HAWKINS, M.D.

GIRL, aged 9. She is said to have been pale and delicate from her birth. At the age of 4 she suddenly vomited half a pint of blood, without pain and without any previous symptoms pointing to gastric disease. She was admitted into Belgrave Hospital, and apparently soon recovered.

At the age of 6 she had a second hæmatemesis, equally sudden, and she is said to have vomited a pint of blood. This was followed by melæna for several days. It is not certain, but it is probable, that this attack was accompanied by some pain in the epigastrium. She recovered in about six weeks.

Early in 1910, at the age of 8, she began to have frequent attacks of pain "in the stomach and around the heart." Crops of small purpuric spots appeared on the chest, and the mother states that she was easily bruised. On one occasion there was epistaxis. She was becoming pale and weak, and on admission into St. Thomas's Hospital in April of that year melæna was observed on several days in succession. The spleen projected 2½ in. below the costal margin and it was indurated. Red cells were 2,425,000, with a colour index of 0·7. Leucocytes were 3,700. Three normoblasts and five megaloblasts were seen. Polychromatophilia was marked. Coagulation time was 2·5 seconds. There was no other sign of disease beyond hæmic murmurs. After a month of arsenic, red cells were 3,437,000, with a colour index of 0·6.

She remained fairly well, though always pale, till July 27, 1911, when she suddenly vomited a pint of blood. For six weeks before this she had complained from time to time of abdominal pain just above the umbilicus.

A few hours later there was further hæmatemesis, and melæna followed for several days. The spleen was enlarged and hard as before. Red cells were 2,200,000, colour index 0·5, leucocytes 2,980, with no change in relative proportions; two normoblasts, but no megaloblasts seen. For fourteen days there was pyrexia, maxima being 101° F. to 102° F. Under the use of arsenic the condition of the blood has slowly improved; red cell count is now 4,328,000, colour index 0·8, leucocytes 3,600. She is fairly well; she takes food freely without any discomfort, but she is not gaining weight. The liver is just palpable, but it is within the limits of health. The pulse is seldom under 100, and the urine occasionally contains a trace of albumin. The spleen moves freely with respiration, and the question is whether the condition of the blood and the history of the illness justify its excision.

DISCUSSION.

Dr. HAWKINS added that each hæmorrhage was followed by a rather profound anæmia, and there had always been found a condition of leucopænia. He was first struck by the fact that the anæmia did not respond to treatment as rapidly as would have been the case if it had been due simply to loss of blood. Before he saw the patient she had been treated as a case of Henoch's purpura. The spleen was 2½ in. to 3 in. below the ribs. The blood not having responded as expected to the administration of iron, she was placed under arsenic, and since then she had reached a condition of almost complete health. There was still some shortage of leucocytes. She was now gaining in weight, but the spleen still showed a tendency to get larger. He did not think there was any evidence of cirrhosis in the liver. The girl was now probably as well as she was likely to be, and the spleen moved freely during respiration, and if that could be taken as an evidence of the absence of adhesions, it was worth something. Moreover, the spleen was not very large. He supposed that the idea underlying excision of spleen would be the question of preventing subsequent development of cirrhosis of the liver and ascites; and now, if ever, seemed the time to carry it out.

Dr. F. PARKES WEBER remarked that it was important in such cases to know whether there had been recurrent attacks of jaundice. He thought that a history of attacks of jaundice made one suspect, in any case of the kind, that there was already cirrhosis of the liver, in fact that the hepatic cirrhosis might have been as much primary as the enlargement of the spleen was supposed to be. He believed there was a class of cases in which one found enlargement of the spleen, apparently primary, but associated with recurrent attacks of jaundice, and in which, in reality, there had been cirrhosis of the liver from the commencement. He thought that in Dr. Hawkins's case there was no history of an attack of jaundice at all, and that was a point in favour of recommending the operation of splenectomy.

Case of Toxic Cirrhosis, with Spleno-portal Thrombosis and Ascites, treated by Femoral Drainage.

By W. Essex Wynter, M.D., and John Murray, F.R.C.S.

A. B., female, aged 44. Eight years back, during pregnancy, while moving some furniture, patient felt something give way in the left hypochondrium. Subsequent to delivery a lump was felt in this situation, and, three months afterwards, there was enlargement of the abdomen: three years ago, in similar circumstances, the abdomen was again distended. For three months before admission this distension had been increasing so as to cause difficulty in walking and breathing, with pain when lying on the left side, and œdema of legs. Diarrhœa had been constant. There was well-marked ascites, and the spleen descended to the level of the umbilicus. The diaphragm was pushed up, displacing the heart and causing some collapse of the bases of the lungs. The liver was small.

March 10, 1911: Twenty-one pints of serum removed by paracentesis. It had a specific gravity of 1018 and contained 40 gr. per litre of coagulable protein and traces of mucin, but no reducing substance or cells.

March 15, 1911: Blood count—Erythrocytes, 5,110,000; leucocytes, 16,000; hæmoglobin, 80 per cent.

March 24, 1911: Paracentesis, 28 pints.

March 28, 1911: Removed to surgical ward.

Mr. Murray opened the femoral ring through a semilunar incision in Scarpa's triangle. A small opening was made above Poupart's ligament to enable the peritoneum to be pushed through. This was then divided and stitched to the sides of the ring. A considerable amount of fluid escaped, but the abdomen was not completely emptied. The incisions were closely sutured, and healed in a few days. On account of the rapid formation of fluid causing tension in the femoral wound, paracentesis was twice performed in the surgical ward—May 5, 1911, 23 pints 16 oz.; May 23, 10 pints 12 oz. She was discharged on May 26.

June 12, 1911: Blood count—Erythrocytes, 5,640,000; leucocytes, 12,500; hæmoglobin, 80 per cent.; lymphocytes, 27·6; hyaline and transitional, 1·8; polymorphonuclears, 70·0; eosinophiles, 0·6.

July 7, 1911: On one occasion the ascites and œdema of legs disappeared rapidly while taking "anasarcin."

July 21, 1911: A slight leakage occurred through breaking down of the cicatrix in thigh, and this was treated by pad and bandage, healing by 27th, when she left hospital.

September 1—30, 1911: Owing to distension of the upper femoral region by gravitation of fluid, the scar again gave way, leaking slightly; and, through apprehension of possible peritoneal sepsis, the patient was readmitted and fitted with an elastic support. While lying up, the ascites returned and 16 pints were removed on September 29, 1911, before leaving. This was the first tapping since May 23 (four months).

Addendum.

E. R., aged 54, male, was exhibited to the Society on December 13, 1907,[1] as an example of ascites due to cirrhosis hepatis, cured by permanent drainage through the femoral ring. This man, during the four years which have elapsed since the operation, has had no further ascites, and though a hernia has developed in the scar over the femoral opening, no inconvenience has been caused by it.

J. S., aged 50, male, was operated upon for ascites associated with cirrhosis in 1908; an additional precaution against premature closure of the femoral opening being the insertion of a decalcified bone tube. He has been at work for three years, feeling quite well. At the present time there is some free fluid in the peritoneal cavity, and a dilated tortuous saphena vein indicates some obstruction at the site of operation.

Both men continue to drink beer.

DISCUSSION.

Dr. Wynter added that this was one of the cases in which ascites had been to some extent relieved by drainage through the femoral ring. Four months had elapsed without tapping, from the time of the original operation until just now. But there was some trouble in the course of recovery owing to the cicatrix giving way. Prolonged decubitus favoured the formation of an endothelial lining which prevented the complete effusion of the fluid into the tissues. While bringing the female case, he thought it a good opportunity to show the two other cases, one of which was exhibited to the Section four years ago as a successful example of femoral drainage. The patient (E. R.) had not been tapped again. The second patient (J. S.) was under care in April, 1908; he had been at work since, and away from observation until he was asked to

[1] *Proceedings*, 1908, i, p. 49.

come up. Both were cases of rather acute ascites, rapidly refilling with fluid after tapping, which he thought could be claimed to have been cured by opening up the femoral ring and allowing the fluid to flow into the thigh. His opinion was that the fluid did not go on escaping in that way, but that the relief of tension allowed of a collateral circulation developing in the abdominal wall, which permanently relieved the ascites. At all events, after that procedure these two men had remained well for four and three years respectively, without inconvenience. One of them had some fluid in his abdomen, but not sufficient to call for tapping. He hoped it might subside, or that the patient would get on without interference. As both the men had continued their habits, he expected there would be very little improvement in the condition of the liver.

Dr. DE HAVILLAND HALL said he was glad to hear Dr. Wynter's remarks on the question of femoral drainage. Encouraged by the success of Dr. Wynter's first case, he asked Mr. Spencer to see a patient with him, and omentopexy was performed, with some benefit. Mr. Spencer had carried out that procedure for him in hospital many times, and, in his opinion, with considerable benefit. One or two of the cases might be regarded as cured. After the omentopexy, as the ascites continued to recur, he thought of Dr. Essex Wynter's suggestion of femoral drainage, and asked Mr. Spencer to see the patient again six months later. Mr. Spencer drained the patient by passing a decalcified bone tube from the peritoneal cavity through the femoral ring into the tissues of both thighs. There was a free escape of fluid, but the result was not encouraging; indeed, he could scarcely say that any benefit was derived from it at all. The lady had since been tapped thirty-four times, and yet she continued in excellent health, and averred that she was better than a year ago. So possibly the operation had prolonged her life; certainly she had been made more comfortable by the omentopexy, and possibly the drainage through the femoral canal had done some good. He had seen an account in a French paper of a possible explanation of the effect of this drainage. A physician suggested the advantage of withdrawing some of the fluid from the peritoneal cavity, and then withdrawing the point of the needle until it came within the subcutaneous tissue, and allowing some of the fluid to escape into that tissue. By that means diuresis was started, and the patient's kidneys acted in the most successful manner. So it was possible that, apart from the direct benefit produced by the loss of fluid through the femoral canal, it might act by stimulating the kidneys and thus stimulating absorption.

Mr. SAMPSON HANDLEY said he was specially interested in that subject, because Dr. Wynter asked him to do the operation on his first case. It had remained a successful case. It appeared to him that in certain cases there were two reasons for the failure of the proceeding. In some cases it would fail because of some mechanical accident, such as a piece of omentum passing down into the newly made opening, and blocking it up. In others he believed it failed because such a torrent of fluid was poured out that no drainage

system could cope with it. He had tried to obviate the objection that the opening might be blocked up by applying the same principle of capillary drainage along silk threads, which he had used for the brawny arm of breast cancer. In at least one case this had been successful. He believed the patient was still living; he had not seen her for two years. In that case he made an abdominal incision, and using very thick silk, he sewed up the peritoneal and muscular layers and thrust the ends of the silk, which had been left long, into the subcutaneous tissues in all directions; subsequently the skin was sutured separately. In that way the silk appeared to act as a channel for the conduction of the ascitic fluid into the subcutaneous tissue of the abdomen. This was shown by the appearance of an area of œdema around the abdominal incision. Where femoral drainage failed, the method he had just described seemed worthy of consideration.

Dr. DE HAVILLAND HALL, remarking on Mr. Handley's observations, said that in his patient the plan of drainage by silk was also adopted very freely, threads being passed up into the axilla and into the mammary region in three directions, but without benefit, as far as he could see.

Dr. JAMES GALLOWAY desired to ask Mr. Handley whether it might be possible to obtain more successful results in such cases of femoral drainage of ascites by utilizing the peritoneal covering so as to form a channel from the peritoneal cavity into the subcutaneous tissues. One of the main difficulties in obtaining a successful result was the tendency for the artificial opening to get blocked by cicatricial tissue. By utilizing the inguinal canal, or perhaps by everting the peritoneum, drainage might be established for a longer period.

Mr. HANDLEY, replying to Dr. Galloway, said that in the original operation he divided the femoral canal, and sewed the two ends to Poupart's ligament.

Dr. ESSEX WYNTER, in reply, said he made the diagnosis of spleno-portal thrombosis because of the very acute way in which the symptoms had come on, and the very great enlargement of the spleen; it was only an inference.

A large Malignant Growth of the Cheek which has disappeared under Radium Treatment.

By N. S. FINZI, M.B.

MRS. X., aged 48. Has been previously shown at the Laryngological Section in June. Since the age of 7 had a soft and painless swelling growing from the mucous membrane of the cheek. In May, 1910, this became painful, and a month or so afterwards enlarged and continued to increase in size until it was removed widely in November, 1910. All seemed satisfactory until December 19, when there was a sudden rapid swelling in the cheek. This increased rapidly and was

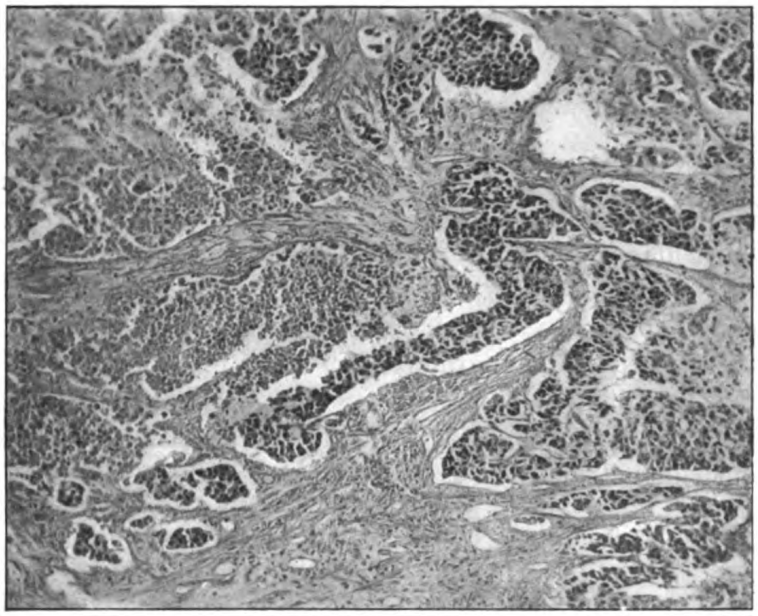


FIG. 1.

Microphotograph of section of growth (low power) from the pre-auricular gland.

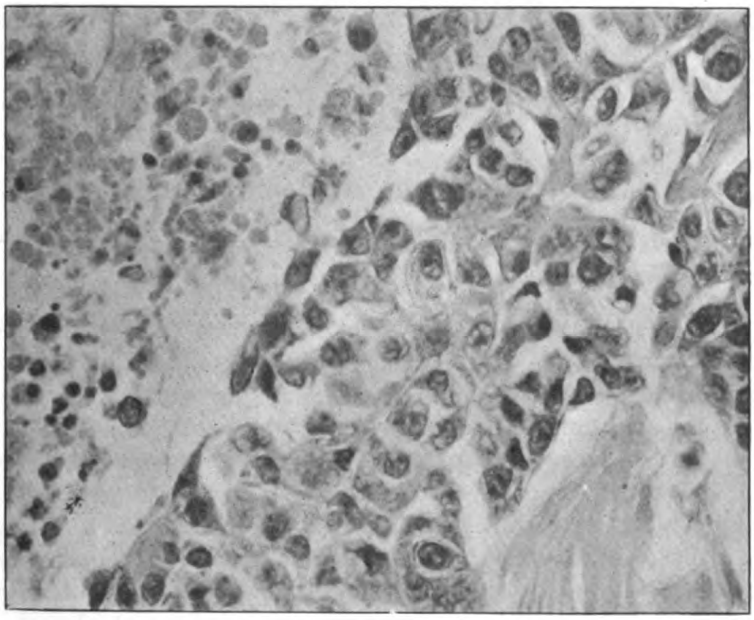


FIG. 2.

Microphotograph of section of growth (high power) from the pre-auricular gland.

incised a few days later and a drop or two of thin pus obtained; nevertheless the growth continued to increase rapidly.

On January 3, 1911, there was an enormous swelling in the cheek about the size of a Jaffa orange, slightly movable, but with considerable œdema round it. At one spot it seemed on the point of bursting externally, as there was a small fluctuating area covered by extremely thin skin. No definite glands were felt, but the œdema, especially in the pre-auricular region, may have obscured these.

January 3—7, 1911: Radium bromide, 105 mg., in two tubes, one surrounded by platinum 1½ mm. thick, and the other by lead the same thickness, inserted into the tumour for sixty-nine hours, and on lint outside the skin in two situations for twelve hours each.

February 2: The tumour had shrunk considerably but there was a pre-auricular gland to be felt. This was excised and 100 mg. of radium bromide with a filter of 2·5 mm. platinum was inserted into the wound for nine hours.

February 23: There were still a few nodules in the cheek; 205 mg. of radium bromide with a filter of 2·5 mm. platinum applied on lint externally for thirty hours, and internally for nine hours.

May 11: Prophylactic treatment with 205 mg. radium bromide thirty-four hours in all.

Patient now seems quite well and the scar has gradually and continuously softened.

It may be remarked that there was never any sign of sloughing of the growth after the treatment, but it simply gradually absorbed, leaving in its place a depression and a scar.

Sections are shown of the original growth and of the gland which was removed (figs. 1 and 2). The nature of the growth is either that of a glandular carcinoma or of an endothelioma.

Dr. FINZI added that within a fortnight the patient had developed a growth the size of a large orange. There was much œdema, and he could not tell whether the submaxillary glands were enlarged or not. A fortnight after commencing the treatment the growth in the cheek had almost disappeared. There was a pre-auricular gland to be felt, and it was thought best to remove that gland, and insert the radium into the wound. Three weeks later there were a few nodules left in the cheek. They were treated, and a fortnight later they had all disappeared. The patient was then unable to open her mouth more than 1½ cm. She had had two prophylactic applications of radium, and there had been considerable softening of the scar. The interesting point in the case was the extreme rapidity of the recurrent growth, and the equally rapid rate at which it disappeared under radium treatment.

Case of Hæmochromatosis with Diabetes.

By REGINALD MILLER, M.D.

A MAN, aged 38, has noticed darkening of the skin for eighteen years, but remained in good health until five months ago, when he began to waste and to suffer from thirst. The pigmentation began on the left shoulder, where the patient carried his bag of tools. It is now universal and is best marked on the face (which is leaden-hued), forearms and legs. A recent scar on the skin is deeply pigmented. No part of the skin is normal in colour. In addition to the bronzing there are innumerable small darker spots, resembling freckles. The mucosa of the mouth, the cartilages of the ears, and the sclerotics are unaffected. The areolæ of the nipples are little if at all affected. The scalp is pigmented but not so deeply as the face. The hair of the head is normal in amount but has begun to turn grey. Axillary hair has disappeared; pubic hair is present. The urine contains much sugar (6 to 10 per cent.) and acetone; from 100 oz. to 150 oz. are passed daily. The glycosuria is little controlled by diet. The blood count shows—Red cells, 4,700,000; white, 7,900; hæmoglobin, 92 per cent. The mononuclear cells are proportionately increased. The liver is enlarged to the umbilical level, firm, and not tender. There are no symptoms of hepatic cirrhosis, and the patient has only suffered from very occasional attacks of mild indigestion throughout the course of the illness.

A microscopical section of the skin removed from the forearm was shown.

DISCUSSION.

Dr. MILLER added that the patient was at first passing 350 grm. of sugar daily, and a large amount of diacetic acid. Under treatment he was still passing 230 grm. of sugar a day, and at the end of last week he feared that the patient was passing into a condition of coma. The liver was enlarged, extending to the umbilicus, but the patient had had very few dyspeptic symptoms, and he did not know that one could say he had had any symptoms referable to the condition of the liver itself. There was a suggestion of clubbing of the fingers. The skin section had been prepared, and Dr. Spilsbury reported to him that iron was to be found in the deepest layer of the epidermis, in the connective tissue round the sweat-glands, and in the walls of the blood-vessels. But, so far, iron had not been found deposited in the cells of the sweat-glands themselves, as had been reported in other cases.

Dr. JAMES GALLOWAY remarked on the prolonged period of pigmentation in this case preceding the development of abdominal symptoms and of glycosuria. In the cases of hæmochromatosis under his observation, when the pigmentation of the skin developed there had been no difficulty in recognizing the enlargement of the liver or the spleen, and in some cases other abdominal disease. In Dr. Miller's case, however, the pigmentation seemed to have existed for many years before any symptom of abdominal disturbance was noted, the glycosuria being of only recent occurrence.

Dr. GARROD said it would be interesting if Dr. Miller would carry out the more recently described tests of functional activity of the pancreas in this case, such as Schmidt's test, casein digestion, and Cammidge's reaction.

Ivory Exostosis of the Frontal Sinus.

By W. SAMPSON HANDLEY, M.S.

THE patient, a man, aged 35, had noticed a lump growing on the inner side of his right eye for about three years. It had not given him

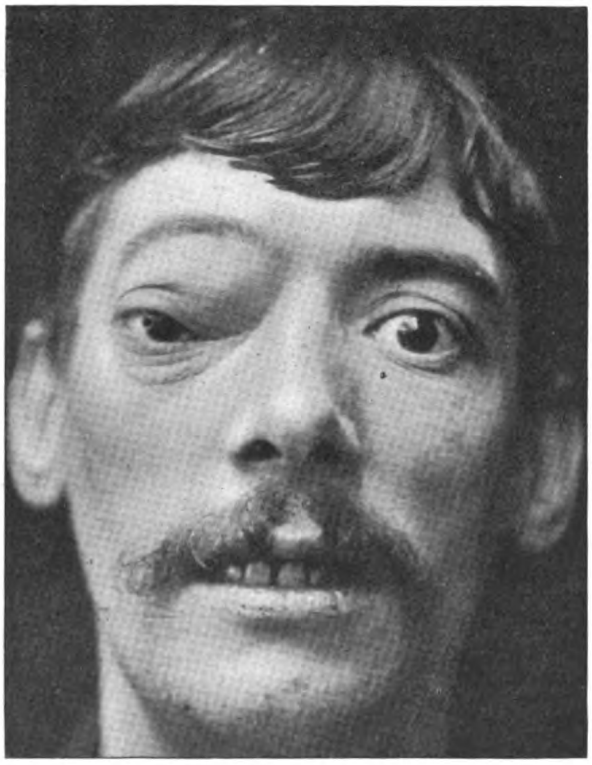


FIG. 1.
Ivory exostosis of the frontal sinus.

any pain. At the operation an incision was made along the inner half of the eyebrow, inclining downwards towards the inner canthus. The surface of the exostosis was exposed : it was not covered by a layer of bone corresponding to the anterior wall of the frontal sinus, this layer having apparently been absorbed. The exostosis was continuous with the frontal bone at the inner wall of the sinus, and here its base was divided with a chisel. With some difficulty it was then loosened from its bed

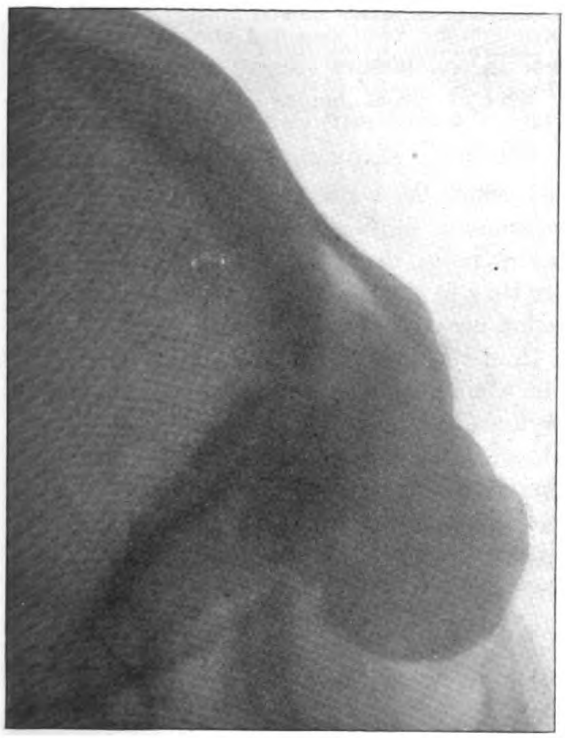


FIG. 2.

Skiagram showing attachment of the tumour.

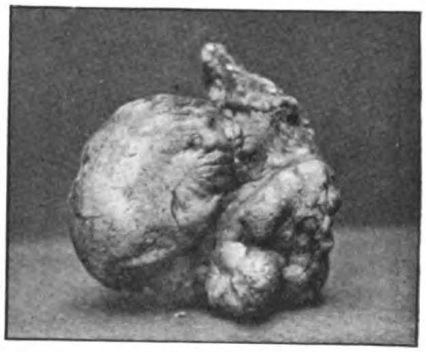


FIG. 3.

Exostosis removed.

by the chisel, aided by rocking with lion forceps, and removed. There was free bleeding, and air bubbled up from the bottom of the cavity, showing that it communicated with the nose. The cavity was packed with iodoform and allowed to heal by granulation.

It is interesting to note that the patient had suffered from bilateral nasal polypi for some years prior to the development of the osteoma. Five years ago the patient had three "black eyes" on the right side

in quick succession, and he thinks there has been some swelling ever since. The osteoma weighs 290 gr. It is lobulated, and upon section it shows the appearance of imperfect concentric lamellation. Its superficial portions are hard and dense, while internally and towards its base of attachment it consists of softer bone.

DISCUSSION.

Mr. HANDLEY said he had recorded the case because such cases were very rare. The only point of interest in the history seemed to be that the patient had had nasal polypi for several years before he developed the exostosis.

Mr. JEFFERSON FAULDER asked what Mr. Handley regarded as the indications for the removal of the tumour in this case. Did the tumour produce symptoms? According to the notes, there was no pain. Secondly, were the X-rays used, and if so, were they of any help as showing the extent of the tumour? He asked the question because of the well-known difficulty in the removal of some exostoses from the base of the skull. He brought down a tumour which he removed in a similar case last year, that of a girl, aged 20. She had very definite symptoms. The eye affected was nearly blind, vision having become reduced in it to $\frac{3}{60}$, and there was considerable displacement of the eyeball. She also had nasal obstruction from part of the exostosis. In getting the tumour out he experienced enormous difficulty. The chisel had no effect on the tumour, and it was necessary to remove it, as Mr. Handley did in this case, from the base. Part of the swelling obstructed the nose, and part of it pressed on the optic nerve, causing the partial blindness. In his case the skiagram was of the greatest possible service, and showed that it projected back to the optic foramen.

Mr. HANDLEY replied that there were no symptoms; there was only the deformity. In both specimens there was a constriction across the middle of the specimen, and that might be caused by the supraorbital ridge of the frontal bone resisting absorption more than the thinner bone above and below.

Two Cases of Malignant Anæmia.

By NORMAN DALTON, M.D.

CASE I.—HÆMOLYTIC TYPE.

J. M., AGED 59, was admitted to King's College Hospital on September 28, 1907. The history presented no special features, but there was a possibility of congenital syphilis and she had had two miscarriages. She had been constipated for many years. There had been progressive asthenia for two years, with emaciation and morning vomiting. Some epigastric pain, relieved by vomiting. No reason to suspect alcoholism. No hæmatemesis. On admission the complexion was of a dusky yellow, and she had all the signs and symptoms of profound anæmia. Heart normal, except for anæmic bruits, lungs normal, urine normal, and never exceptionally high-coloured. The temperature was never very high, but has always been between 99° F. and 100° F. at night, and the pulse was always proportional to the temperature. The abdomen was full and there was a lump of doubtful nature in the epigastrium.

October 5, 1907: Blood count—Red cells, 864,000; hæmoglobin, 34 per cent.; colour index, 1·2; leucocytes, 1,500; polynuclears, 34·8; lymphocytes, 64·8. The red disks were large, and a megaloblast and two normoblasts were seen. The high colour index and the abnormal corpuscles were characteristic of the true pernicious anæmia, showing hæmolysis and an unsuccessful attempt at the formation of new red disks. On October 7, however, the result of a test meal showed the complete absence of free HCl from the gastric juice and the presence of lactic acid in the stomach.

The case seemed to be one of those rare cases of gastric cancer in which hæmolysis occurs, thus simulating the true pernicious anæmia. By October 7 she seemed to be dying, was extremely prostrate and delirious, with a glazed tongue. She was now put on bismuth mixture and gradually improved. By October 29 the red cells had gone up to 1,028,000, the colour index being 2. Vomiting ceased. She was able to leave the hospital on November 22, 1907, greatly improved. No lump could now be felt in the epigastrium.

She was readmitted on May 4, 1908. She was extremely prostrate,

with vomiting, œdema of the feet, retinal hæmorrhages, &c. The epigastric lump could again be felt. She was put on liq. arsenicalis, ♏iv, t.d.s., but got worse, the vomiting being constant and delirium setting in. Red disks, 624,000; colour index, 1·5; great leucopænia with relative excess of lymphocytes, megaloblasts, normoblasts. She was put on bismuth mixture instead of arsenic, and improved. On June 10 the red disks were 1,764,000; colour index, 1·8; no nucleated red cells. The spleen had never been found enlarged. She was well enough to leave the hospital on June 13, and for two years she was able to get about at home and to do some housework.

At Christmas, 1910, the asthenia and vomiting recurred, and she was readmitted to the hospital on July 17, 1911. Red cells, 976,000, but the colour index was now just below normal. Abnormal red corpuscles were found. The abdomen was now full with a little ascites, and the liver and ? the spleen became enlarged. She was very ill for a few days—viz., vomiting, glazed tongue, delirium, &c.—but improved under bismuth. By August 29 the red disks had gone up to 2,092,000, the colour index being 1, and no abnormal red cells were found. On September 21 the red disks were 3,280,000, and she is now really very well.

The points about the case are that the toxins which cause the hæmolysis appear to come from the stomach, because when she is bad the vomiting is such a prominent symptom, and bismuth brings her round, while arsenic makes her worse. When, however, the gastric symptoms cease she can take arsenic with advantage. It would seem as if the arsenic could not exert a favourable influence until the gastric irritation had been subdued. The original idea of cancer of the stomach must be abandoned after four years. She gives a Wassermann reaction.

Case II.—Aplastic Type.

S. R., aged 21; living under favourable circumstances on high ground. Suffered from progressive asthenia for four years; latterly extremely pale and weak; constant attacks of vomiting. On admission to King's College Hospital, August 13, 1910, all the symptoms and physical signs of profound anæmia were present. Complexion pale, not yellow. Loud systolic murmur all over heart, triple sounds over the pulmonary valve; the last were probably due to a reduplicated pulmonary systolic bruit, but at times they sounded like frictions.

Tongue normal; no septic teeth or other sepsis. Spleen moderately enlarged, and often painful and tender. Liver not enlarged. Wassermann reaction absent. She was well covered without being fat, and her weight did not alter materially.

Blood Counts.

August 15, 1910: Red cells, 1,345,000; colour index, 0·7; white, 1,200; polymorphonuclears, 54; lymphocytes, 46.

September 5: Red, 1,456,000; colour index, 0·7; white, 2,600; polymorphonuclears, 70; lymphocytes, 28.

October 12: Red, 1,048,000; colour index, 0·7; white, 6,000; polymorphonuclears, 63; lymphocytes, 34.

October 25: Red, 2,376,000; colour index, 0·6; white, 5,800; polymorphonuclears, 70; lymphocytes, 28; eosinophiles, 2 per cent.

November 18: Red, 904,000; colour index, 0·9; white, 7,600; polymorphonuclears, 52; lymphocytes, 48.

The extreme oligocythæmia, the low colour index, and the relative lymphocythæmia are typical of aplastic anæmia. At no time were any abnormal red disks found. The patient fluctuated greatly. About every ten days there was a bout of fever, remittent, but not reaching a great height. It lasted four or five days, and was accompanied by vomiting. She was very prostrate at these times, and I thought that she would die. In the intervals the red disks increased in number. Note that the hæmoglobin did not increase as rapidly as the number of corpuscles, nor did it fall as rapidly as they did; the highest colour index being present when the corpuscles were fewest. In such a case the bone-marrow of the sternum is found to be white, atrophied, and fatty. The loudness of the bruits and the attacks of splenic pain suggested infective endocarditis with splenic embolism, but blood cultures proved sterile; and in a case of this kind in which I did a post-mortem, although endocarditis was present, it was of the simple kind. No treatment appeared to have any favourable effect, though arsenic and iron were given. The improvement which set in after November was spontaneous.

December 5: Red, 1,260,000; colour index, 0·9; white, 3,600; polymorphonuclears, 57; lymphocytes, 43.

She gained comparative strength rapidly, and when she left the hospital the blood count was, January 10, 1911: Red, 2,028,000; colour index, 1·5; white, 4,000; polymorphonuclears, 58; lymphocytes, 41.

The high colour index in this count is curious, and it has not been maintained.

She came to King's College Hospital in May, 1911. Blood count, May 22: red, 2,550,000; colour index, 0·6; white, 3,200; polymorphonuclears, 53; lymphocytes, 38.

At present (November 4, 1911) the blood count is practically the same as in May. This blood count now would pass as a rather severe case of ordinary chlorosis, but the extreme oligocythæmia and the recurrent fever with vomiting differentiate the two conditions. The improvement up to date is remarkable, as most cases of aplastic anæmia are fatal in a year or so. She has been at home all the year, and has occasionally been able to walk as much as five miles. But nearly every week she gets a "bad attack," probably like the bouts of fever and prostration she suffered from in hospital, but she does not vomit now. The heart and spleen are the same as when she was in hospital. Since she left the hospital two teeth have decayed, but the mouth is quite aseptic. Menstruation has returned.

Dr. DALTON regretted that the second patient was too ill to attend. In regard to the first patient, there were two points of interest. The first was that she always got worse when given arsenic, and always got better when the stomach was treated with ordinary bismuth and mucilage. That occurred on the three occasions she was in the hospital. The other point was that it was now found that the Wassermann reaction was positive. Syphilitic anæmia was known not to be of that type; there were not high colour indices or abnormal corpuscles. He was therefore inclined to say there was some modification of the syphilitic toxin, probably due to liver or stomach disease, which had caused that toxin to produce hæmolysis.

The Rheumatic Diathesis and the Thyroid Gland.

By LEONARD WILLIAMS, M.D.

THIS patient's case was reported in the *Lancet*, May 1, 1909,[1] in an article entitled "Adenoids, Nocturnal Enuresis, and the Thyroid Gland," as follows:—

"The patient, E. P., a girl, aged 11, was first seen on February 2, 1909. She had wetted her bed every night since birth. For the last

[1] *Lancet*, 1909, i, pp. 1245-47.

few years she had, in consequence, slept on straw. She was dull and apathetic, but not stupid. She was thirsty, but her appetite was poor. The patient was not undersized, and her weight was 5 st. 1 lb. The tonsils were somewhat enlarged, but there were no adenoids or nasal obstruction. The temperature was 99° F. in the mouth. One and a half grains of thyroid extract, three times a day, were ordered. February 9: The patient had missed wetting her bed for the last three nights—the first in her life. The same dose of thyroid extract was ordered. February 16: On three successive nights the bed had not been wetted. The dose of thyroid extract was increased to 2½ gr. three times a day. February 23: No enuresis had occurred during the week. Thyroid extract was ordered as before. March 2: No enuresis. March 9: No enuresis. March 12: No enuresis."

On March 12, although there had been no enuresis, the patient complained of pain in the left arm, and on examination the elbow proved to be swollen and tender.

She did not come to the hospital again until March 26, and was consequently without medicine for a week. The bed had been wetted once, the night before her return. The dose of thyroid was reduced, and a mixture was ordered containing calcium iodide and arsenic. On April 2 there had been no enuresis; the thyroid was stopped, but the mixture continued. On April 16 the bed had been wetted four times in fourteen days; the thyroid was repeated. April 23: Enuresis had occurred once. April 30: No enuresis.

The patient remained quite free from enuresis from April 30 till August 11. She was coming regularly to the hospital during that time, and was taking 1½ gr. of thyroid extract. The notes show that during that period there had been complaints of pains in the joints, especially the elbows and knees.

From August 11 till October 6 she remained away from the hospital. Having been given a supply sufficient for a month only, she had thus been without treatment for more than three weeks. On reporting herself at the end of that period she stated that she had wetted her bed three times during the previous week and once during the current week. From this date (October 6, 1909), under regular thyroid medication, there was, up to March 8, 1910, no enuresis. She then disappeared from the out-patients' room. During this time there had been several complaints of joint troubles.

Early in January, 1911, I found her in the wards, under the care of Dr. Langdon Brown, who had been treating her for a typical attack

of acute articular rheumatism, which had affected her mitral valve. There had been no urinary difficulties in the course of the attack, but the enuresis returned during convalescence. It was controlled, though tardily, by thyroid medication, continuing with considerable remissions until April 25.

On May 26, 1911, she came to the hospital with a definite attack of left-sided hemichorea, which rapidly yielded to large doses of arsenic. In August she began menstruating; the periods have been regular and painless, but unduly profuse. November 7: Involuntary movements returned last week.

The mother is interesting as a case of pseudo-obesity, who is unable to tolerate the smallest dose of thyroid.

DISCUSSION.

Dr. LEONARD WILLIAMS said he showed the case more in order to elicit opinions than to dogmatize about it. Some medical critic, at the meeting at Belfast, suggested that the cases which he (Dr. Leonard Williams) reported as being improved by the exhibition of thyroid extract existed only on paper. Here, at any rate, was one case which had a real existence. The subsequent history was interesting. The patient had an attack of rheumatic fever and two attacks of chorea. He suggested that, using the term in its mathematical sense, the rheumatic diathesis was a "function" of the thyroid gland. He had held that view for some time, and, curiously enough, that very morning he received a letter from Dr. Anderson, of Bodmin, in which the writer said there was a family at present giving him much anxiety. The mother had myxœdema, which started after the menopause. The son, aged 35, had Graves's disease, and both daughters, aged respectively 42 and 39, had got it. The latter was nearly well, but she looked as if she might very easily relapse. Her mitral valve was affected by an attack of acute rheumatism some years ago. Every member of the family had had acute rheumatism, and one girl died of ulcerative endocarditis when a child. He asked whether rheumatism played any part in the causation of the Graves's disease, as all, or nearly all the cases which it had been his misfortune to see had had a history of rheumatism. It was generally thought that Graves's disease and myxœdema were mutually exclusive, in the sense that one was supposed to be due to too much thyroid, and the other to too little. There were, however, mixed forms, which showed some of the signs of myxœdema and some of the signs of exophthalmic goitre. It was curious that he should receive this letter after having suggested that the rheumatic diathesis—i.e., acute rheumatism, chorea, subcutaneous nodules, tonsillitis, &c., which were recognized as allied to one another—as due to some dereliction of duty on the part of the thyroid gland.

The letter suggested that exophthalmic goitre, which was supposed to be due to excess of thyroid gland, had also some connexion with rheumatism. He was not prepared to express any definite opinion about the matter, except that it was worthy of investigation. Another question which had arisen, and was being discussed in the *Lancet*, was the effect of salicylates upon people who were supposed to be sub-thyroidic. Dr. Waller suggested that salicylates did not agree with people who were sub-thyroidic; and some of the accidents attributed to aspirin were said to be due to a want of thyroid in the persons to whom they were administered. But some had suggested that aspirin and salicylates did much good in exophthalmic goitre, and others suggested that the salicylates did much good in chorea. These facts could not be dismissed as merely coincidental, and he thought it possible, and even probable, that the ultimate ætiology of the rheumatic diathesis might be found in some as yet undiscovered and undescribed difficulties connected with the thyroid gland.

Dr. MILLER said the first point suggested by Dr. Williams was as to what was meant by "the rheumatic diathesis." It was well known that most children who were going to have rheumatism suffered from many kinds of functional nervous conditions, of which enuresis was only one. This was the condition which he had suggested should be called "latent chorea." In it enuresis, lienteric diarrhœa, somnambulism, tics, and headache were found. He asked if thyroid extract had been proved to be of any value in treating those other conditions also. Personally, he did not think they were manifestations of the rheumatic diathesis, but rather exhibitions of the earliest symptoms of rheumatism. In other words, a patient did not get rheumatism because she was nervous, but she was nervous because she already had rheumatism. With regard to the question of enlargement of the thyroid gland in chorea, he believed the figures were, that about 10 per cent. of cases of chorea showed slight enlargement of the thyroid. It would be remembered how common such enlargement was in girls of from 10 to 14 years of age. He did not know whether this enlargement of thyroid in 10 per cent. of cases was of any importance. He had seen a case, which was under the care of his colleague, Dr. Sutherland, in which there was definite chorea on the top of Graves's disease. He thought such a combination was infinitely uncommon. The child had tremor, goitre, and exophthalmos, and, in addition, mitral disease and choreic movements. She died, and post mortem, in addition to the condition of thyroid, there was also rheumatic endocarditis—i.e., endocarditis of the type seen in rheumatic cases—showing, therefore, that there was the possible infective cause for the chorea. Dr. Carey Coombs, who had done so much work on the histology of rheumatism, examined the thyroid as well as other organs, and could trace a rheumatic affection of the mitral valves and myocardium, but there were none of the characteristic changes of rheumatism histologically in the thyroid.

Lipoma of Tongue.

By V. Warren Low, F.R.C.S.

C. T., aged 69, has noticed the swelling on the right side of his tongue for eight years. Gradual onset and slow increase in size. No pain, and only inconvenience owing to the size of the organ. Now lobulated tumour on right lateral aspect of tongue extending for two-thirds of its length. Mucous membrane over the tumour smooth, and through it can be distinctly seen the underlying lobules of fat. There is a smaller and similar tumour on the left side of the organ. There is no infiltration of the tongue, which can be readily and easily protruded.

Mr. Warren Low added that lipomata of the tongue were not common, and when they were met with they were symmetrical or multiple; this case was no exception to the rule. The second lipoma here was not so advanced as the other, but it was quite distinct. It did not cause much inconvenience, and he did not expect the patient would allow anything to be done to it. In answer to a question by Dr. de Havilland Hall, he said the patient would not confess to being alcoholic.

Further Experience in the Treatment of Cholera by Injections of Hypertonic Salines and Permanganates Internally among Europeans at Palermo.

By Leonard Rogers, M.D., Major I.M.S.

It is impossible, as it is unnecessary, in such a paper as this, to give a complete account of all the details of the system of treating cholera which I have worked out during the last few years in Calcutta, as they are fully recorded in my recently published work on the disease,[1] while a summary appeared in a paper in the *British Medical Journal*.[2] It will, however, be advisable briefly to recapitulate the principles on which it is based so as to allow of what I have to say regarding my

[1] "Cholera and its Treatment," 1911.
[2] *Brit. Med. Journ.*, 1910, ii, pp. 835-39.

recent very instructive experience of cholera among Europeans at Palermo being more readily followed.

The Blood-changes in Cholera as a Scientific Basis for its Treatment.—In the first place my researches[1] have shown that in the collapse stage of cholera, from one-half to two-thirds, or even more, of the fluid of the serum has been lost to the system, while in the most severe cases a still greater proportion of the chlorides may be lost, and the blood may even be hæmolysed. These observations indicate the

Table I.—Blood Changes and the Effects of Hypertonic Saline Injections in Cholera.

	Normal.	*Average of 7. Fatal Cases.*	*Average of 12 Cases Recovering after Transfusion.*	*Average of 5 Mild Cases not Transfused.*
Percentage of corpuscles	*45*	*71*	*65*	*56*
Percentage of serum	*55*	*29*	*37*	*44*
Loss of serum	—	*64 per cent.*	*52 per cent.*	*35 per cent.*
Pints of saline injected	—	*3.7*	*3.6*	—
Serum after injection	*6*	*61 per cent.*	*61 per cent.*	—
Blood pressure before injection	*110*	*0–50 m m.*	*51 m m.*	*71 m m.*
Blood pressure after injection	—	*97*	*107*	—
Chlorides before injection	*0.8*	*0.79*	*0.90*	*0.92*
Chlorides after injection	—	*0.95*	*1.07*	—

necessity of rapidly replacing the lost salts as well as the lost fluid, and of raising the salts in the blood somewhat above the normal point, so that the osmotic currents will tend to cause fluid to run into the blood rather than out of it, and thus the diarrhœa will be checked, instead of increased, as it is by the use of normal salines. For this purpose I use the following hypertonic saline solution :—

Sodium chloride	120gr.
Potassium chloride	6 ,,
Calcium chloride	4 ,,
Water	1 pint

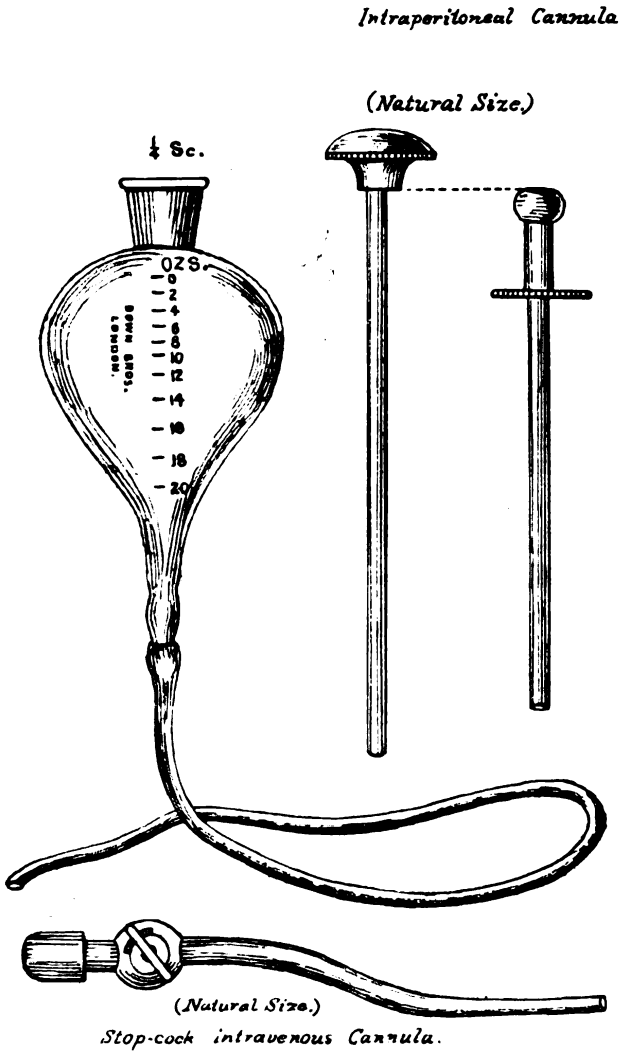
[1] *Proc. Roy. Soc. Lond.*, 1909, s. B., lxxxi, pp. 291-302.

As this formula contains 1·35 per cent. of sodium chloride it is twice as strong as was formerly used as normal saline, and half as strong again as the amount of salts now given as the normal amount by physiologists. The last two chlorides are added to make the proportion of the three approximate to Ringer's solution, which when perfused through an animal's heart will keep it beating very much longer than sodium chloride alone. Parke, Davis and Co. make hypertonic tablets of the above composition such that four to one pint make my hypertonic solution, while three in the same amount make an approximately normal saline. I used them with marked success at Palermo. I have, however, used 2 dr. to the pint of sodium chloride alone in very many cases, with successful results. I may take this opportunity of pointing out that the solution advised for intravenous injection in the article on cholera in the last edition of Allbutt's "System of Medicine,"[1] consisting of 0·4 per cent. sodium chloride and 0·2 per cent. of sodium bicarbonate, is dangerously hypotonic; in fact, I found it hæmolysed a little of my own blood nearly completely in about two hours, so that its use in large quantities would probably have most disastrous consequences.

Blood-pressure and Specific Gravity Estimations as a Guide to Transfusions in Cholera.—The degree of collapse may be accurately measured by taking the blood-pressure with one of the numerous modifications of the Riva-Rocci mercury manometer. Observations on several hundred cases of cholera have shown that, if the blood-pressure has fallen to 70 mm. or less in natives of India, or to 80 mm. or less in Europeans—which means a serious degree of collapse—it is necessary to give intravenous injections, but if above these points, saline injections are readily absorbed when given subcutaneously as hypertonic solutions, or by the rectum as isotonic ones. Of even greater value is the estimation of the specific gravity of the blood by means of a series of twelve small bottles of glycerine and water of each two degrees from 1048, 1050, &c., up to 1070. A small drop of blood from the finger is blown gently from a capillary tube into the middle of a bottle, and if it sinks it is heavier than the solution, and a higher bottle is tried until it just floats for a few seconds, which is the point required. If it sinks in one and rises in the next, the point is between the two. This estimation can be easily made by the bedside in two minutes, and it furnishes invaluable information as to the degree of concentration of the blood, and the necessity for an intravenous injection, as well as an indication of the quantity required. The normal specific gravity being

[1] 1907, ii, pt. ii, p. 473.

about 1056, if it is raised to 1063 or over it is safe to inject intravenously 4 pints in an adult male and 3½ pints in a female. If the blood is very greatly concentrated, as shown by a specific gravity of from 1066 to 1070, or over, as much as 5 or 6 pints may be used, the last one or two being given slowly. The rate of injection can be readily



Drawing of the graduated glass bulb and stopcock cannula used.

controlled by means of the graduated glass bulb and stopcock cannula made for me by Messrs. Down Bros. (*see* figure). The fluid may be run in at the rate of 4 oz. a minute until a full pulse has returned, and then more slowly, especially if there is headache or oppression of

the chest. It is advisable to dilute the blood to several points below the normal specific gravity so as to allow a reserve of fluid for any further diarrhœa and for excretion through the kidneys, whereby toxins will be removed from the blood. The latter effect will be greatly facilitated by the fully normal blood-pressure which will usually be obtained by the injection of the large quantities advised above. In addition to the intravenous injections, whenever the blood-pressure falls to 80 mm. or less in a European, half to one pint of normal saline is also injected high up into the rectum every two to four hours in all cases, to keep the blood fully diluted, and this measure is continued until at least 2 pints of urine are passed in the twenty-four hours.

Control of the Temperature Reaction.—There is always a rise of temperature in the reaction stage of cholera, even when no salines have been used, and older Anglo-Indian writers regard this as the most dangerous stage of the disease. After an intravenous injection a similar marked febrile reaction occurs, often accompanied by a rigor, and it may sometimes run on into a dangerous hyperpyrexia, if not detected in time, and controlled by cold sponging and iced water rectal injections. A temperature of from 101° F. to 103° F. is a good sign, and if it rises to 104° F. or over, it should at once be reduced by the above measures. I have observed that dangerously high reactions occur most during hot damp weather, and especially in patients whose rectal temperatures were above normal at the time of the injection, although the surface heat may have been much below normal. In such cases the salt solution should be injected at below blood heat. Instances in which 4 pints of hypertonic solution were successfully given intravenously at a temperature of only 86° F. in patients whose rectal temperatures were 105° F. and 106° F. are given in my book.

The Prevention of Uræmia.—The greatest danger after recovery from the collapse stage of cholera is the continued deficiency of the renal secretion, terminating in fatal uræmia and toxæmia. I have shown experimentally that this is due to renal stasis from continued sub-normal blood-pressure and concentration of the blood, which can be readily detected by blood-pressure and specific gravity estimations two or three days before the onset of grave symptoms. Further, provided the kidneys were previously healthy, and the patient is not very old, the onset of uræmia may nearly always be prevented by diluting the blood to at least the normal point by isotonic salines, either subcutaneously or intravenously at a slow rate; and by the use of blood-pressure-raising drugs, such as digitalin, strophanthin, caffeine, and the

vaso-constrictors, adrenalin and pituitrin. A number of illustrative cases are given in my book, so I need only mention here, that in Calcutta the deaths from uræmia have been reduced by these methods to one-half their former numbers, and that, too, in spite of so many grave cases of cholera being tided over the collapse stage by hypertonic injections intravenously, to face the dangers of later failure of the renal secretion.

Permanganates Internally to Destroy the Cholera Toxins in the Bowel.—Although a great reduction in the mortality of cholera resulted from the use of hypertonic salines, yet some of the more severe cases died of toxæmia after being rescued from the dangers of collapse. It thus became clear to me that any further progress must depend on the destruction of the toxins in the body. In the absence of any specific serum of proved potency, I experimented to see if the toxins are readily destroyed by any substance which could be safely introduced into the gastro-intestinal tract in sufficient quantity to be likely to be of service, for as the cholera organisms are practically limited to the interior of the bowel, the toxins must be elaborated there and absorbed into the blood. As bacterial poisons are mostly albumoses and closely allied unstable albuminous compounds, I tested the action of oxidizing agents, such as I had previously shown readily destroy all kinds of snake-venoms, which are also albumoses. I found that cholera toxins are rapidly rendered completely inert by the action of permanganates, so I commenced giving these drugs in the following ways. A solution of calcium permanganate (which is somewhat less astringent than the potassium salt and at the same time stronger, as it is divalent) is given to drink *ad lib.* in a solution of 6 gr. or more to the pint. It is usually advisable to dilute it further at first until the patient gets accustomed to the taste and then gradually increase it as much as possible. In addition, the potassium salt is given in pills, being more convenient for this purpose, as it is much less hygroscopic than the calcium salt.

Potassium permanganate	2 gr.
Kaolin	q.s.

Make a pill and coat with salol or keratin, so that it will pass through the stomach and dissolve in the small intestine, where the action is desired. Two pills are given every quarter of an hour for the first three hours, and then two every half hour until the stools become green and less copious, which usually occurs within about twelve hours. The pills are then discontinued, but a course of eight pills is given at the

beginning of the second and third days under treatment, to prevent relapses. In children smaller amounts must be given. Any pills which are vomited are replaced by others.

Results obtained in India.—Such is a brief outline of my methods, and I venture to claim that they amount to a complete system of treatment, based on scientific investigations, and affording means of accurately estimating the exact condition of the patient at any stage of the disease and for correctly meeting the indications furnished by the blood-pressure and specific gravity estimations. The results obtained

TABLE II.—*Results of Different Forms of Treatment of Cholera.*

Period.	*Treatment.*	*Cases.*	*Deaths.*	*Percentage.*	
				Deaths.	*Recoveries.*
1895–1905 ...	*Rectal and subcutaneous saline.*	1,243	783	59.0	41.0
1906	*Normal saline intravenously.*	112	57	51.9	49.1
1907	*Rectal and subcutaneous saline.*	158	94	59.5	40.5
1908 1909 ...	*Hypertonic intravenous saline.*	294	96	32.6	67.4
August 1909 to July, 1910	*Hypertonics plus permanganates.*	103	24	23.3	76.7

during the last four years in India may be summarized by saying that by the use of the hypertonic saline solution intravenously the death-rate in almost 300 cases, during two years, was reduced to 32·6 per cent. from the previous rate in the same hospital during eleven years of 59·6 per cent. Further, during a complete year, with the addition of the permanganates internally, the death-rate was further reduced to 23 per cent. Moreover, I have been informed by Major Megaw, I.M.S., that this last result has been well maintained during the present year; while Captain Rutherford, I.M.S.,[1] in an epidemic in the Central Provinces in

[1] *Indian Med. Gaz.*, Calcutta, 1910, xlv, p. 497.

1910, also obtained 77 per cent. of recoveries, his death-rate being little more than one-third of that of cases in the same outbreak treated by other methods.

The Treatment of Cholera at Palermo among Europeans, and its Lessons.

The above-mentioned results were obtained in natives of India, but in Europeans in Calcutta the disease is more severe, the mortality among them during thirteen years having been no less than 81·6 per cent. With the new treatment the recovery-rate in Europeans in Calcutta has risen from 18·4 to between 50 and 60 per cent., which still leaves room for further improvement. The number of cases of cholera among Europeans in Calcutta are fortunately few, so I was anxious to take advantage of the prevalence of the disease in Europe this year to obtain a larger experience of the disease among them. Thanks to the very kind help of Professors Marchiafava and Bastianelli in Rome, and of Professor Romano, the Director of the Lazaretto at Palermo, I obtained permission to live at the Palermo Cholera Hospital for three weeks during August, and to demonstrate and practise my methods with the cordial assistance of the able staff of doctors residing there, who showed me the greatest kindness and did all in their power to make my efforts a success. The difficulties were great, owing to my ignorance of the language, while, until the staff had learnt the details of my methods, the cases were too numerous to allow of the intravenous injections being used as frequently as was desirable, while it was only towards the end of my visit that it was possible to arrange for their being given at night. Moreover, as much of the nursing was done by relatives admitted to look after the patients, it was not possible to ensure for those who had been rescued from very serious collapse the degree of skilled attention which is supplied by the English system of nursing, and which is essential to obtaining the best results in such an exhausting disease as cholera. For these various reasons a number of patients were eventually lost who might have been saved under more completely favourable conditions of work. Nevertheless, the Palermo medical authorities were rapidly convinced of the great advance in the treatment of cholera afforded by my system, while as the results were even better than had previously been obtained among Europeans in Calcutta, I have good reason to be satisfied with them. Moreover, thanks to the great advantage of living at the hospital, and the large number of serious

cases I was able to watch closely, I have been fortunate enough to add very greatly to my previously extensive knowledge of cholera in all its rapidly changing phases; with the important result that I am not only fully confirmed as to the great value of the new system of treatment, but I am convinced that even still better results will be obtained in the near future by yet earlier and more frequent hypertonic injections. I will therefore first briefly record the results obtained at Palermo, and then detail the lessons I have learned from my experience there and point out their bearing on further improvements in the treatment of this justly dreaded disease.

Some idea of the extent of the work at Palermo may be formed from the statement that eighty-four intravenous injections were carried out in sixty-seven patients in twenty-two days, at almost all of which I was present, having on several days spent at least twelve hours in the wards, the temperature throughout having been at a point which would have ensured the use of punkahs in India. In estimating the results I have divided my stay into two portions, as during the first ten days the difficulties already mentioned were particularly great, while during the last twelve days the Italian doctors had learnt my methods and carried them out enthusiastically as far as was possible, and I was able to make daily observations of the blood-pressure and specific gravity of the blood in all the more serious cases, and generally carry out my system in a more satisfactory manner than at first. It is also necessary to say a few words on the condition of the cases treated, for it is well known that towards the end of an epidemic the disease is less severe than at first, while there may be much difference of opinion as to what should be clinically classed as cholera. In the first place, as many as twenty patients were admitted on some days, while there was no lack of nearly moribund cases, so there was no evidence of any decline in the virulence of the disease. On the other hand, a number of cases of diarrhœa were admitted as suspected cases, some of which did not present the typical symptoms of cholera during their detention. Professor Romano informed me that during my stay the death-rate to total admissions was under 20 per cent. This is striking testimony, but I would not lay undue stress on it, for the reason just mentioned, that a few non-choleraic diarrhœas were probably included in the numbers.

Fortunately the difficulty in accurately estimating the results are easily overcome by only considering the markedly collapsed or otherwise very serious cases requiring intravenous saline injections. As I

have estimations of the blood-pressure and specific gravity in this series the necessary data are available for comparing them with the results I obtained in India in exactly similar cases. I found, however, that a blood-pressure of about 70 mm. in a native of India represents a similar degree of collapse to one of about 80 mm. in a European, whose normal point is about 20 mm. higher than that of an Indian. I therefore took the blood-pressure of 80 mm. or less as an indication of a degree of collapse necessitating an intravenous injection at Palermo, and my experience showed that even this is too low a point for complete safety. Moreover, Calcutta experience shows that such a degree of collapse in a European suffering from cholera was formerly very rarely recovered from, not more than 10 per cent. of such cases being cured under former modes of treatment. Yet among twenty-seven cases treated by intravenous injections during my first ten days at Palermo there were fourteen recoveries, or 51·8 per cent., representing a probable fivefold reduction in the mortality in these very grave cases. This result was all the more satisfactory, because four very old patients were included who were hopeless from the first—a much larger proportion than I have met with in India. Moreover, during this time it was usually only possible to transfuse the worst patients.

During the second period of twelve days no less than forty patients were given intravenous injections, a number of whom had more than one. There were twenty-four complete recoveries, or 60 per cent., thus amounting to a probable sixfold reduction in the mortality of these serious cases. Figures, however, convey but a feeble impression of the remarkable nature of the cures resulting from this treatment, while words can do little more. It is necessary to see patients brought in in a moribund condition and rescued from certain death by the intravenous injections and restored to health within a very few days. It will therefore be well to quote from a generous letter Professor Romano gave the day I left, with permission to publish it. The following is a literal translation by a friend who has long lived in Italy:—

PALERMO, LAZARETTO GUADAGNA,
August 25, 1911.

I, the undersigned Director of this Lazaretto, do declare that Professor Leonard Rogers lived in this hospital from August 4 of this present month until to-day, in order to practise and demonstrate his method of treatment of cholera. The results obtained have been more than satisfactory, they have been surprising, seeing that many of the patients who were admitted at the point of death regained their health and were discharged cured.

It will be well here to refer to the treatment in vogue on my arrival. I am very glad to be able to say that, short of the use of the methods recently introduced by me, the treatment at Palermo was excellent. Sterile normal saline solutions in sealed glass vessels of half a litre capacity were used for repeated subcutaneous injections, with good results in many patients coming under observation before serious collapse had ensued. The injections were given every few hours not only until the patient had passed the stage of acute diarrhœa, but were also continued until a free secretion of urine was obtained. The results were particularly good in very young children, in whom intravenous injections are very difficult: a point I shall return to presently. Careful attention was also paid to dieting and disinfection, and the whole institution was excellently administered. I made full use of the subcutaneous injections to supplement the intravenous ones, only adding some further salt to raise the strength to the hypertonic point during the acute evacuation stage, and I am of the opinion that some part of the improved results over those obtained in Europeans in Calcutta was due to these subcutaneous injections. They were used in all patients admitted with a blood-pressure over 80 mm. in the earlier stages, and proved efficient in preventing collapse in a number of them. Nevertheless, no less than fifteen of my intravenous cases had been admitted with a blood-pressure of over 80 mm. and treated by subcutaneous injections of as much as one litre at first and half a litre every four hours subsequently, but became much worse and passed into severe collapse, requiring an intravenous injection; ten of these ultimately recovered. It is thus clear that the intravenous method will save a large proportion of the cases in which the subcutaneous one has signally failed to avert the progress of the disease. The fact that the former method of treatment at Palermo was very good as far as it went, certainly better than I have seen in India antecedent to recent improvements, makes the further marked decrease in the mortality due to my methods, to which the Italian doctors have borne such generous testimony, all the more satisfactory.

Lessons learnt from my Palermo Experience.

Striking as are the successes with the new methods of treatment, yet it is the failures which teach us most; so a study of the cases in which we did not succeed in rescuing the patients from the deadly grip of severe cholera will be most helpful in the quest for still better results.

The following table shows the causes of death in the cases treated by intravenous injections. The figures in parentheses indicate the number of cases which I think might probably have been saved under completely favourable conditions and with my present knowledge and experience.

Cause of death	First series	Second series	Total
Collapse	10 (4)	... 5 (2) ...	15 (6)
Hyperpyrexia ...	0	... 2 (1) ...	2 (1)
Uræmia	1	... 6 (2) ...	7 (2)
Lung complications ...	2	... 0 ...	2
Asthenia	0	... 1 (1) ...	1 (1)
Total	13 (4)	14 (6)	27 (10)

The largest number come under the head of collapse, but it will be observed that they were much more numerous in the first series, when time did not permit of the intravenous injections being given as early and as often as was advisable. Six fatal collapse cases are placed in parentheses as possibly preventable deaths. Of these, two were patients who collapsed fatally during the night, when it was not possible to give a second injection, while one should have had a second injection late at night, but it could not be given owing to another more urgent, and ultimately successful, case requiring one at the same time. Of still greater interest and importance are the remaining three cases, as these patients were admitted with a very fair pulse and a blood-pressure between 80 and 100 mm., so that an intravenous injection was not considered necessary on their arrival. One later on the same day and the other two on the following morning were found to be collapsed in spite of having been treated with repeated hypertonic saline solutions subcutaneously. An intravenous injection was now given in each, but proved to be too late to save the patients. It is the not infrequent occurrence of such cases which makes one often regret having withheld an intravenous injection at the time of the admission of a patient, and necessitates a careful reconsideration of the rules I have adopted regarding the use of this life-saving measure.

Next in size is the class comprising the deaths from uræmia, and here again the two patients, entered as possibly preventable deaths, were admitted with a fairly good pulse and a blood-pressure between 80 mm. and 100 mm., but subsequently had to be given intravenous injections, owing to the supervention of serious collapse, in spite of repeated subcutaneous injections. I feel convinced that both of these patients would also have been saved had an intravenous injection been given at the time

of their admission. The other two preventable deaths were from hyperpyrexia and asthenia respectively. Had all the probably preventable deaths been averted no less than 70 per cent. of the very serious intravenous transfusion cases would have been saved.

The Treatment of Cholera in Children.

Another aspect of the successful treatment of cholera with regard to which I learnt much from my experience at Palermo relates to the disease in children, who formed a much larger proportion of the cases than in Calcutta. The successful treatment of cholera in young children requires much watchfulness, yet there is no class in which constant care is more fully rewarded by the saving of valuable lives. I have already mentioned that hypertonic salines subcutaneously were often efficient in the pre-collapse stage of cholera in children, while they occasionally were successful even after collapse had occurred. The most striking example of this was in a child, aged 3, admitted with scarcely any pulse at the wrist, cold and restless, while the specific gravity had reached the very high point of 1067. I considered that the only chance was an intravenous injection, which is a difficult procedure at such a low age. As the transfusion apparatus would not be ready for an hour I first gave half a litre of the hypertonic solution subcutaneously, and at the end of an hour the pulse had distinctly improved, so the injections were ordered to be continued every four hours, with complete success. In several other less grave cases in children of from 2 to 7 years of age the occurrence of serious collapse, and the consequent necessity of an intravenous injection, was averted by the use of subcutaneous injections. On the other hand, in three boys of from 7 to 11 years, subcutaneous hypertonic salines failed, and the occurrence of grave collapse necessitated the intravenous injection of from three-fourths to one litre of the hypertonic solution, with success in two of them. Altogether intravenous injections were given in ten children from 6 to 11 years old, with recovery in seven of them, while one was lost from preventable hyperpyrexia when apparently doing well, and one died of collapse, namely a boy, who was moribund on admission. It is thus clear that the intravenous method is eminently satisfactory in children, in whom the use of a small glass cannula (which is readily made from glass tubing with a blow-pipe or the flame of a spirit lamp) is often necessary for this purpose. If the elbow veins are too small the large vein which crosses over the internal malleolus usually suffices, or the internal saphenous vein in the thigh may be used as has been done in Calcutta.

From dissections I have made I am convinced that in this way intravenous injections could even be given in an infant, with the aid of one of the small glass cannulas used by physiologists.

THE ADVISABILITY OF STILL EARLIER AND MORE FREQUENT INTRAVENOUS INJECTIONS IN THE TREATMENT OF CHOLERA.

This is the most important lesson I have learnt at Palermo, and is based on the following considerations. I have never seen any harm result from an intravenous injection of hypertonic solution in the acute stages of cholera, with the sole exception of excessive temperature reaction, which is preventable if the precautions I have advised are strictly carried out. On the other hand, I have often had to regret the decision not to give an intravenous injection in a cholera patient coming under observation with a fairly good pulse and a blood-pressure of over 80 mm., but who subsequently required one owing to the supervention of collapse, and was ultimately lost. Several such cases at Palermo have already been referred to. This shows that I have not hitherto used the intravenous method as often as is advisable, and the rules I have laid down regarding them require to be reconsidered in the light of my further recent experience. It has been shown above that the patients in whom a reliance on subcutaneous injections proved disastrous, all had blood-pressures between 80 mm. and 100 mm. on admission, so that it is clear that withholding intravenous injections when the blood-pressure is over 80 mm. is not an altogether satisfactory rule. Fortunately we have an additional guide in the specific gravity of the blood, and it is of great interest and importance to note that in all the patients admitted with a fairly good pulse, but who collapsed fatally later, the specific gravity of the blood was 1063 or over on their arrival, which means that they had already lost three or four pints of fluid from their blood. Now with such high specific gravities continued copious diarrhœa is almost certain to produce collapse within a few hours, while the blood is already concentrated to such a degree as to make the intravenous injection of three to four pints at a not very rapid rate a perfectly safe and highly beneficial procedure in cholera. Again, we have seen that once serious collapse has taken place, although death in this stage can generally be averted by one or more intravenous injections, yet a considerable proportion of such grave cases will ultimately be lost from one or other of the later complications, such as severe reaction, uræmic or lung complications or asthenia. The further diminution of these still not infrequent causes of death can only

be accomplished by anticipating and preventing the occurrence of serious collapse by replacing the great loss of fluid as early as possible; or in other words, by intravenous injections as soon as the blood has become markedly concentrated, as shown by a specific gravity of 1063 or over, even though the strength of the pulse may only have fallen a little below normal, as indicated by a blood-pressure of not much below 100 mm. I have previously advised that intravenous injections should always be given if the specific gravity is extremely high, such as over 1065, but I would now go further and recommend this treatment to be adopted as soon as the specific gravity has risen to 1063, or over, in the stage of acute copious diarrhœa in order to avert the nearly inevitable collapse if they are withheld. If, however, the acute diarrhœa stage is passed and the reaction stage with a rise of temperature has commenced, the blood-pressure being over 80 mm., an intravenous injection is not necessary, while it is more likely to produce an excessive febrile reaction at this stage.

The Prevention of Uræmia.

I have little to add under this heading, except to repeat that the further reduction in the death-rate from this complication must mainly depend on the earlier use of intravenous injections to prevent the onset of collapse with the accompanying stasis of the renal circulation, which is the essential cause of the subsequent deficient renal secretion. It has long been known that the frequency of uræmia is in proportion to the duration of the collapse stage. This point is further illustrated by the fact that all the fatal uræmic cases at Palermo were admitted on the second and third days of the disease, while all also showed a specific gravity of 1063 or over. Several patients admitted after two or more days' complete suppression of urine and with a high specific gravity and threatened uræmia were treated by slow intravenous injections (isotonic solutions being used if the blood-pressure was nearly normal and the diarrhœa stage past), with the result that the urinary secretion was rapidly re-established, and recovery from a very grave condition followed. If in this stage the specific gravity is not above normal intravenous injections might produce œdema of the lungs, so in that case only subcutaneous and rectal salines should be given. If the respirations continue to be frequent and laboured after urine is being freely passed, œdema of the lungs should be sought for, and if present all saline injections should be stopped.

Such are the lessons I learnt at Palermo, and I venture to hope that

their application in the future will do something to still further lessen the death-rate of cholera. That cases will still be met with of too virulent a nature, or coming too late under observation to be saved from a fatal termination, it is useless to deny. Nevertheless, these should be quite exceptional in previously healthy subjects, who are neither old nor extremely young, and who come under treatment at a fairly early period of the disease. In such I think the recovery-rate should be fully 80 per cent. However that may be, the results already obtained furnish a remarkable example of the successful application of modern methods of research, in discovering a life-saving method of treatment of one of the worst scourges which humanity is heir to, and one which should go far towards robbing the disease of many of its terrors in countries adequately provided with skilled medical men.

ADDENDUM.

Since the above sanguine estimate of the probable further reduction of the death-rate of cholera by the still earlier and more frequent use of hypertonic saline treatment, as advised in the foregoing paper, was written, a report has reached me from Palermo which has more than fulfilled it. Professor Romano has sent me notes, with blood-pressure and specific gravity estimations, of fifty-four severe cholera cases, all with specific gravity of 1063 and over, which were treated by hypertonic intravenous injections during the month after I left Palermo, with only eight deaths, or 15 per cent.; leaving no less than 85 per cent. of recoveries, while two of the fatalities were from late pulmonary complications. After making every allowance for any possible decrease in the virulence of the disease, these are most remarkably successful results. The impression made by them on Professor Romano and his assistant medical officers may be gathered from the opening sentence of his letter, a translation of which reads as follows :—

I have the honour to inform you of the marvellous progress made by the patients whom you left under treatment here at the time of your departure. I have delayed writing in order that I might further communicate, by means of the accompanying letter and forms, the results obtained from a large number of patients who were brought here since then in a hopeless condition, and who have been almost all saved through your valuable and miraculous method of treatment."

The Professor goes on to say that he and his assistants hope to be permitted to go to Tripoli to spread the use of my methods there. This generous testimony can leave no possible doubt as to the value of the treatment advocated in this paper.

DISCUSSION.

Dr. F. M. SANDWITH said he was sure that Professor Rogers and the medical profession in general were to be congratulated upon the great diminution in the death-rate from cholera which the author of the paper had shown. To have brought down the mortality from its usual 60 per cent. in India to 23 per cent. and even 20 per cent., was a sufficient testimony to the value of Professor Rogers's method. And it was very gratifying that the improvement should have come about through the work of the officer of the Indian Medical Service, because many have thought in times past that India had taken very little care either to treat cholera or to prevent it spreading to other parts of the world. Cholera was present in some part of India at all periods of the year and every year. The disease had often spread from India to Mecca and thence to Egypt and Europe. This year at least four European countries had been visited by cholera. His own first connexion with the disease was in 1883, when he volunteered to go out to Egypt to see it. He obtained an introduction to a distinguished sanitary officer attached to the War Office, fresh from India, who knew all about the disease and kindly gave him an hour's demonstration on the subject, showing by diagrams on the blackboard how the troops were to be moved at right angles to the place they were going to. Those were the days of belief in aerial waves. He told the authority that, so far as he knew, he would not be having anything to do with British troops, but would be dealing with Egyptian civilians, and he wanted to know how he was to treat cholera patients in hospital so as to obtain the best results. The officer replied that he did not know much about that, as that aspect of the matter had not come before him very much, but that one could not do better than trust to calomel and opium. He (Dr. Sandwith) knew enough, however, to prevent his acting upon that advice, and Professor Rogers, in his book, told his readers why those two drugs were bad. In 1896 he (Dr. Sandwith) had the advantage of seeing another epidemic. But in spite of all his efforts, $56\frac{1}{2}$ per cent. of his hospital patients died. Half of them were street beggars, and the other half were very poor. The average stay in hospital of fatal cases was only thirty-six hours; they died of collapse. One of the most distressing things in this disease was to nurse a patient successfully through the stage of collapse, and then lose him from uræmia. Sixty-seven per cent. of the hospital cases under him in the last epidemic had complete suppression of urine; sometimes for as long as from three to five days no urine would be passed, and one wondered how they could survive under such conditions. He would say that the prognosis was bad if there were kidney disease or even kidney delicacy; also the prognosis was bad if the subject was alcoholic, and particularly bad if the patient had been in the habit of taking opium. Professor Rogers might be able to say whether alcohol had anything to do with the Italian cases. As far as he knew, the good cases were in those whose kidneys were sound, and when the disease occurred at the age which offered the most resistance, namely, 5 to 25 years of age. He had referred to matters

of early history because there could be no one who appreciated better than he did the great advantages which Professor Rogers's work had given to those who had to treat cholera cases to-day. He wished he had had this knowledge when he was grappling with the epidemics. Although he had not had recent experience of cholera, he had had many opportunities, with his colleague, Dr. Duncan, of examining men in the Indian Medical Service, and others who had come from parts of the world where cholera was rife, and when asked how they would treat cholera, they had replied that they would use Dr. Rogers's method. That was high testimony. He had been glad to hear that Professor Rogers could ascertain the specific gravity of the blood in two minutes. Although it was right and justifiable to work out the new method in hospitals, it must be remembered that it was in cholera camps and villages, where there were no refinements and luxuries, that most of the cases had to be treated. If the ascertainment of the specific gravity of the blood were only a question of carrying a few bottles about, it was very valuable. He asked whether Professor Rogers could say anything about the bactericidal serum which Dr. Strong, of the Philippines, produced in 1904. At that date it had not been tried on many human beings as a prophylactic. It seemed to promise to be very good, even better than those in use in India. There might be some present who could say if Professor Rogers's method had been used in infantile diarrhœa.

Dr. DUNCAN congratulated Professor Rogers on his paper and on the steps he had taken to bring about such a reduction in the mortality. When he (Dr. Duncan) was in India he never found the mortality less than 60 per cent., and it therefore seemed almost incredible it could be brought so low as 15 per cent. But in many epidemics there was a low percentage of recovery following on a higher percentage. As Professor Rogers's experience extended over many months, one could rely on his figures being trustworthy in all cases. When he (the speaker) went to India the state of cholera was very deplorable owing to the opinions which were enforced by the Senior Sanitary Officer. At Netley, Professor Maclean began his lecture on cholera by warning the candidates never to believe a word that the Sanitary Commissioner of India uttered on cholera. He learned his first lessons in cholera under the late Sir George Johnson, whose theory of collapse he regarded as the only one which fully explained the symptoms. That authority treated all his cases with castor-oil. His own first experience of cholera was in the Afghan War of 1879. It was at one of those twelve-year pilgrimages at Hurdwar, which were the starting-places of many cholera epidemics. On the Khyber side the Government of India took the advice of the Sanitary Commissioner, and he said the troops should be marched away from the "climatic zone" of cholera. On doing so, the returning pilgrims were met, and the mortality was so great that it was known afterwards as "The March of Death." On the Kurram side the troops were moved up to the surrounding hills, and the mortality among them was very small. His own regimental headquarters were located up a gorge 8,000 ft.

high, the other half-battalion remained in the plain. He used to visit the latter half-battalion twice a week, and one morning he found two cases in the stage of approaching collapse, and twenty-eight cases not very far advanced. He gave them castor-oil, and all recovered. With regard to cases of cholera being treated with castor-oil, he had had many cases in India, and, in his experience, the cases treated with castor-oil did better than did those treated with opium. The treatment of cholera of late years had yielded progressively better results. Major Harold Brown, I.M.S., introduced the treatment by eucalyptus oil, and had reported great success with it. But no treatment with which he was acquainted equalled that advocated by Professor Rogers, who was well known for his researches on Indian disease extending over many years. It was the greatest benefit which had been conferred by any of the officers with whom he had been associated.

Dr. E. C. HORT said he had never seen a case of cholera, but there were a few remarks he would like to make of some interest from the experimental standpoint, in regard to saline injections. In the first place, he would like to ask whether Professor Rogers had been able to work out what he considered to be the safe limits in injecting saline into man, apart from estimation of the specific gravity and the blood-pressure. For some time his colleague, Dr. Penfold, and he had worked at the Lister Institute on the effect on rabbits of saline solutions and other liquids when injected. The results were of such interest that perhaps they could be applied to man if control observations could be carried out on healthy human subjects. It was found that intravenous injections of saline were not so innocuous as was often supposed—i.e., not so harmless in rabbits as they were supposed to be in man. In fact, in order to avoid unpleasant results, it was necessary to establish a definite limit to the concentration of the salt injected; also there should be an exact determination of the ratio between the volume of liquid injected and the body-weight of the animal receiving it. This was very important. The unpleasant results which had been encountered from time to time included fever, rigors, subnormal temperatures, diarrhœa, hæmorrhage from the bowel—sometimes associated with diarrhœa, sometimes not—Cheyne-Stokes breathing, convulsions, and, under certain conditions, sudden death. He would not detain the Section at that late hour by entering into details, especially as they would shortly be published. But they were not at all satisfied that the fever, which was very inconstant as regards extent, was due to the salt injected, a view which had been assumed in recent work in Germany on the subject. On the contrary, he and his colleague had good reason for suspecting that the fever was due to the water injected in the case of the rabbit, and not to the salt. They had never injected more than one-fiftieth of the body-weight of normal saline, and sometimes symptoms followed the injection of as little as one-thirteen-hundredth of the body-weight in 2 per cent. solution. Those quantities in a rabbit weighing 2,000 grm. would correspond, in a man weighing 10 st., to injections of 1,500 c.c. in the first case, and nearly

60 c.c. in the second. The quantities advocated that evening by Professor Rogers were 3,000 c.c. of a 1·35 per cent. solution of salt and water. It had been found that if one injected quite a small quantity of 12 per cent. solution in rabbits, serious symptoms might ensue, although the bulk of the injection was not large. Another point was, that Dr. Penfold repeatedly found that if a high concentration of salt was injected rapidly, death would ensue with dramatic suddenness, so that by this means one could be certain of killing the rabbit every time. But there the percentage of salt was much higher, varying between 16 per cent. and 25 per cent. The point of his remarks was, that Professor Rogers, by his method, was in the habit of giving much larger quantities than animals could safely be given in the laboratory. What they had found was, that all the unpleasant symptoms which he had mentioned could be avoided in rabbits by paying attention to three points; and therefore it would be agreed that the value of the method of treatment brought forward would be greatly increased if those points were insisted upon, namely: (1) The exact quantity of salt injected in a given case; (2) the volume of the liquid injected as a whole in proportion to the body-weight of the receiver of the injection; (3) the velocity with which the injection was given. Finally, it was worth pointing out that as many of the cholera patients were in a serious condition before the injection, any symptoms produced by the saline might very well be masked and overshadowed. In their experiments in the laboratory the animals were injected while in a healthy condition, and therefore mild symptoms following on the injection were easily observed.

Dr. ROGERS, in reply, said that Dr. Sandwith had spoken of the spread of cholera from India to other countries. Owing to the greatly improved communication with Eastern countries, he felt certain there would be more trouble from cholera in Europe in the near future than there had been in the past. In his book he had illustrated by means of maps the routes of other epidemics, and showed that in 1826 to 1831 the epidemic took five years to spread from India to Russia; whereas the epidemic of 1893, spreading over exactly the same route, occupied only five months in doing so. This was owing to the greater rapidity of communication. As the sanitary condition of Russia was still in a very unsatisfactory state, with no water supply in a large proportion of the villages and towns, the occurrence of 100,000 deaths was likely to be repeated. So certain did he feel of this, that he had already applied for leave to be ready to go when cholera again broke out. With regard to air currents, this theory was stated in an official publication with regard to cholera as late as 1894, but Mr. Ernest Hart, who was in Calcutta about that time, ridiculed the idea so much that the document was withdrawn the next day. With regard to the occurrence of uræmia, the great point was to transfuse early; the earlier this was done the less frequently did uræmia occur. The taking of opium predisposed enormously to uræmia; opium-eaters who had the disease nearly always died from it. He had had a series of observations taken,

and found that the mortality in opium cases was twice as high, and the occurrence of uræmia was seven times as frequent as in other cases. Opium should not be given at any stage whatever during an epidemic of cholera. He agreed with Dr. Duncan that astringents should not be employed, because of the liability to uræmia. The diarrhœa in the patient helped to remove the toxins and to prevent further absorption. With regard to the effect of alcohol, in Italy several of the patients who died were alcoholics, and a history of great indulgence in alcohol was of bad prognostic import, especially if the patient was getting on in years. With regard to ascertaining the specific gravity of the blood, the practitioner might not have the instruments with him, but in his book he gave indications for transfusion in the absence of those instruments. If there were cramps, an intravenous injection should be given; the same was true if there was restlessness, or cyanosis. After a little experience the practitioner could depend on the clinical signs for guidance in treatment. With regard to Strong's prophylactic, this was a modification of and a great improvement on Haffkine's prophylactic. Haffkine's method caused much pain and distress, whereas Strong's method did away with this to a great extent, and the results were better. It had not been tried on a large scale. The difficulty with Bengalis was that they would not readily allow injection to be given, while Europeans were in such little danger of having cholera that it was not worth while to use injections in their case. With regard to castor-oil, Dr. Kenneth McLeod tried the method in India more than once, but he had to give it up, owing to the disastrous results which followed. He (Dr. Rogers) thought that purging in cholera was enough to carry away deleterious matter without the assistance of drugs. With regard to Dr. Hort's interesting observation, that gentleman's results were not quite comparable, because cholera patients had lost enormous quantities of fluid, whereas in the laboratory the injection was given to healthy animals. If it were run in quickly in a patient whose blood was not concentrated, œdema of the lungs might ensue. He had not used as high a percentage as 2 per cent.; high percentages caused more marked temperature reactions. In cholera, when one restored the circulation and increased the salts above the normal one found a re-absorption of some of the fluid by the bowel. He had come to the conclusion that 2 dr. to the pint was a safe dilution. When a patient was collapsed the fluid could be run in rapidly at first, but if there were headache, or depression, or increased rapidity of respiration, the rate of the current must be slowed down. That was the advantage of the stopcock cannula, for it could be regulated at once. The occurrence of hæmorrhagic symptoms as a rule gave a hopeless prognosis for the patient. He had seen such cases recover, but not often. The injection caused the cramp to disappear, and all the symptoms were relieved; indeed, the cholera patient often went to sleep before the injection was finished. If normal saline were used, the diarrhœa would increase within three hours, the fluid would come out again, and collapse ensue, but the success which had now been achieved was due to the solution he recommended. He had tried eucalyptus and other drugs, but had not found them of much use.

Clinical Section.

December 8, 1911.

Sir WILLIAM OSLER, Bt., F.R.S., President of the Section, in the Chair.

Four Cases illustrating Surgical Treatment of Chronic Empyema of the Nasal Accessory Sinuses.

By HERBERT TILLEY, F.R.C.S.

Case I.—Male, aged 71, who sought relief for double vision, and complained of no other symptoms. A small, tense swelling was observed between the right eyeball and the inner half of the "orbital arch." There was distinct proptosis of the eye. Examination of the nasal cavities revealed a free discharge of pus from all the sinuses on the right side. Operation: An incision was made immediately below the right eyebrow and along its whole length, and terminating just above the internal canthus. The swelling above referred to was found to be part of the wall of the frontal sinus empyema, which by destroying the roof of the orbit had forced its way anteriorly. The anterior wall of the sinus was completely removed, when it was found that the whole posterior wall of the sinus had been destroyed, exposing a large area of dura mater, while the floor of the sinus consisted of the contents of the orbit. The right maxillary antrum, ethmoid and sphenoid sinuses contained pus, and were freely drained by the usual surgical methods adopted for these bony cavities. The patient has made an excellent recovery, with very slight deformity. The curious feature of the case is that in spite of the extensive destruction of the bony walls of the frontal sinus, he never suffered from headache, or complained of any purulent nasal discharge. His one trouble was "seeing double."

Case II.—Female, aged 30, admitted to University College Hospital in July, 1911, for inflammation of the "right eye and face" of five weeks' duration. On admission there was intense inflammation and swelling of the right side of face, extending from the frontal eminence

to the level of the right angle of the mouth. The right eye was proptosed and the globe hidden by swollen, œdematous, everted conjunctivæ. The upper eyelid was swollen, œdematous, and of a purple colour, and just below the inner angle of the eyebrow was a pus-discharging fistula leading backwards to the posterior and inner region of the orbit. A purulent discharge was seen in the right nostril and in the naso-pharynx. Temperature, 102° F. Mental condition obtuse. Patient looking very ill. Operation, similar to that in Case I, carried out immediately. Large collection of pus opened on inner side of orbit, and sequestrum removed from posterior ethmoidal cell region. Antrum opened in canine fossa and found to be full of caseated pus; inner antral wall removed to provide for free and permanent drainage. Patient's recovery retarded by symptoms of pleurisy, which eventually cleared up. Only slight deformity is to be noted in region of the frontal sinus, in spite of free removal of all its anterior wall.

Case III: Acute Suppuration in the Frontal Sinus.—Female, aged 13, admitted to University College Hospital on October 9, 1911. History: Seven days ago suffered from "sore throat with shivering fits." Following day, pain in left eye, followed by swelling of the upper eyelid, which had increased every day since. For the past six days there was severe pain on the top of the head, which had kept her awake at night. There had been a yellow discharge from the left nostril for the past three or four weeks, and, three days ago, "rather severe bleeding" occurred from the same side. Condition on admission: General condition fairly good; pulse 102; temperature 99·6° F.; tongue furred. Left upper and lower eyelids swollen, red and œdematous, especially the upper lid, which was level with the eyebrow. Marked proptosis and eye pushed outwards. Conjunctiva lining eyelids swollen, œdematous, and slightly chemosed. Ocular movements impaired in all directions and diplopia when the patient looks upwards, downwards, and to the left. Fundus oculi normal. Pus could be seen in the left middle meatal region. Operation: Under chloroform narcosis the anterior half of the middle turbinal was removed and an incision made, starting just above the internal canthus and continued upwards and outwards immediately below the line of the eyebrow for two-thirds of its length. Thick greenish-yellow pus immediately escaped from the inner portion of the incision. The soft parts were retracted and the globe gently pressed downwards and outwards by a narrow spatula so that the inner region of the orbit could be examined. Pus could now be seen issuing from

the floor of the frontal sinus and from an adjacent ethmoidal cell. The periosteum was then carefully stripped from the anterior wall of the frontal sinus, the latter opened and cleansed. Since the sinus was small and the duration of infection doubtful, the whole anterior wall was removed, the sinus curetted, a free opening made into the nose, and the bony cavity lightly packed with gauze, and the outer end of the skin incision sutured with horsehair. The dressings were removed in forty-eight hours and the sinus lightly repacked every day until it was obliterated by healthy granulation tissue. Patient made an uninterrupted recovery and left the hospital on November 3, free from any discharge and with small trace of operative intervention. The condition is uncommon in children.

Case IV.—Male, aged 60, whose left frontal sinus was opened from the outside, because of severe headache associated with empyema of that sinus and the corresponding antrum. Killian's operation was performed —i.e., the anterior wall and floor of the sinus were removed, with the exception of that portion of the orbital arch which forms the junction of the anterior wall and floor of the sinus. By leaving this bridge of bone very little deformity results, as is seen in this patient.

Mr. HERBERT TILLEY showed the first case because of the extraordinary absence of symptoms, notwithstanding the serious pathological condition present. The patient only complained of double vision. The second case was referred to him by Mr. Godlee. There was marked proptosis of the right eye and the whole side of the face was a mass of inflammatory œdema. Her troubles came on just after her confinement. There was nothing to be seen now beyond the slight scar caused by the operation. The third case was brought into hospital with appearances illustrated by the photograph which was exhibited. The child was very ill, but it was easy to diagnose the case as frontal sinus empyema. He believed that in a few years' time it would be difficult to say which eye had been operated upon because the deformity was so slight. She recovered without causing any anxiety. In regard to Case IV, he explained that the trouble with frontal sinus operations previous to the Killian method was that they produced much depression in the forehead, and that was a great disadvantage. But by saving the bridge of bone in the lower part of the anterior wall of the sinus it was possible to remove the front wall of the sinus, and obliterate the sinus itself as well as the ethmoidal cells through the one incision, and with little deformity. The orbital capsule rose upwards into the sinus and became covered with granulations, and little deformity resulted, because the soft parts could not then fall backwards as in the old type of operation. Hence one could obliterate quite a large sinus with very little deformity. The case very well demonstrated the excellent cosmetic result which could be attained in operations for chronic frontal sinus empyema.

Demonstration, by the Direct Method, of Papillomata of the Vocal Cords.

By HERBERT TILLEY, F.R.C.S.

THE case exhibited was one of hoarseness in an adult female, caused by papillomata of vocal cords. Mr. Tilley briefly demonstrated the mechanism of Brünings's bronchoscope, and having previously anæsthetized the patient's pharynx and larynx with a 20 per cent. solution of cocaine, he passed a tube directly into the larynx so that the papillomata upon the cords were easily seen by members of the Section.

It was pointed out that the direct method of examining and operating upon the larynx was not intended to supersede the older, indirect mode in which the laryngoscope was made use of, but the direct method was invaluable where rapid and complete removal of certain laryngeal lesions could not be carried out by the older method, and this especially applied to small nodules upon the edge of a cord or growths situated in or just below the anterior commissure.

Long-standing Dislocation of the Patella (? Traumatic) and Osteo-arthritis of the Knee-joint treated by Operation Eighteen Months ago.

By MORRISTON DAVIES, F.R.C.S.

E. S., WIDOW, aged 55. Forty-eight years ago she fell down and dislocated her right patella. Since then the patella had remained on the outer side of the knee. There was no history of poliomyelitis, and the electrical reactions did not suggest that the original dislocation was due to paralysis. Thirty years ago patient began to be troubled with pain in the right knee, and ten years ago she was unable to stand or walk without the aid of a special splint and crutches.

At the time of her admission to hospital in May, 1910, there was considerable genu valgum of both legs; the right patella was lying to the outer side of the joint with the anterior surface looking directly outwards. The condyles of the femur were easily palpated, and marked osteo-arthritic changes could be recognized. With the leg fully extended

it was possible only partially to replace the patella. The patient was unable to lift the limb off the bed, or to extend it against resistance. There was considerable wasting of the quadriceps femoris, and especially of the vastus internus; the knee-jerk, however, was obtainable.

Operation, May 27, 1910: The main steps in the operation were: (1) The opening of the knee-joint, and the removal of a large part of the anterior and inferior surfaces of the external condyle, so as to allow the patella to ride over the lower end of the femur when the joint was flexed. (2) The division of the extension of the quadriceps femoris on the outer side. (3) The freeing of tendons of the gracilis and semitendinosus muscles from their insertions in the tibia, and the stitching of the cut ends of these to the inner side of the patella. The tendons were passed under the sartorius, and enclosed in folds of the capsule. (4) The longitudinal pleating of the capsule so as to tighten it on the inner side of the joint.

Massage and passive movements were begun on the fourth day, but a fortnight later adhesions in the joint had to be broken down.

On June 20 the patient could flex the knee to a right angle, and could lift the leg off the bed. She left the hospital wearing only a bandage as support.

The patient celebrated the first anniversary of her operation by going to a dance and dancing hard. She can now run up and down stairs, and work all day long, without pain or undue fatigue.

The actual result from a cosmetic point of view might be still further improved by curing the genu valgum, but considering the patient's age, and the satisfactoriness of the limb, there is no indication for this further operation.

DISCUSSION.

Mr. MORRISTON DAVIES said he had wished to show another case of dislocation of the patella to the outer side treated by operation, but the patient had been unable to come. In this other case, a boy, aged 12, the trouble had been present for five or six years, and had followed an attack of poliomyelitis. It was difficult to get the early history of the case now shown as it dated back forty-eight years ago. In both cases the same type of operation was done, but while in the case of the boy operated on two years previously he had transplanted the semitendinosus muscle only, in the case of this woman he had transplanted both the semitendinosus and the gracilis. In both cases union was good, and passive movements and massage were commenced as early as possible. The result has been satisfactory in both. Previously the boy had always complained of pain in the knee, and when playing with other children he frequently fell to

the ground; now he was free from pain and could run easily. The cosmetic effect was not as good as might be desired, because there was considerable genu valgum. If this was corrected one could bring the patella in the middle line and keep it there. But as she was aged 55 it was not an operation which was called for.

Mr. MAYNARD SMITH said he had a similar case to deal with three or four years ago. The patient was a boy, aged 19, who had a long-standing dislocation of the patella, apparently due in the first instance to an excessive degree of knock-knee. The articular surface of the patella rested against the outer aspect of the external condyle of the femur. The tibia, owing to the consequent oblique pull of the quadriceps, was rotated so that its inner surface looked directly forwards, and the tubercle faced outwards. The boy was able to walk with the leg stiff. If, however, he bent the knee for more than about 5 degrees from the straight line he was unable to support his weight on it and fell. The reason for this was that even this slight amount of flexion caused the line of action of the displaced quadriceps to fall behind the axis through which flexion and extension of the knee-joint took place. The quadriceps extensor, in consequence, became a flexor as soon as the knee was slightly bent. The operation he did was not the same as the one which Mr. Morriston Davies had performed with such a good result. In his case he chiselled away the tubercle of the tibia and reimplanted the tubercle on the inner aspect of the tibia, making a hole for it and screwing it into position. This necessitated the making of a gap in the outer aspect of the capsule in order to allow of the patella being brought round to its position in front of the femoral condyles. An elliptical piece was excised from the redundant capsule on the inner side. The result was satisfactory in every way, and the boy was able to walk well.

Mr. GORDON WATSON said that he had helped Mr. Waring with a similar case. The patella was dislocated on both sides, so that the boy was thrown down. Mr. Waring performed the operation which Mr. Maynard Smith had just described, transplanting the tubercle of the tibia to the femur, taking a reef out of one side and letting out the other. This was more or less successful on one side, but on the other side it was a failure and the condition recurred. Mr. Waring then performed a supracondylar osteotomy, to give the patient genu varum. This was successful. Mr. Waring had had another case, which he treated with success by subcutaneous supracondylar osteotomy.

Mr. DOUGLAS DREW mentioned a case which he recorded in 1907 [1] of congenital dislocation of the patella. There the first operation performed was division of the femur and correction of the genu valgum, which failed to keep the patella in position. He then performed the operation originally carried out by Bilton Pollard, of deepening the trochlear surface of the femur and pleating the capsule on the inner side after dividing it freely on the outer. This was not sufficient, for in flexing the knee the patella still slipped over the

[1] *Proceedings*, 1908, i (Clin. Sect.), p. 11.

external condyle, so he transplanted the tubercle of the tibia with the patellar ligament to the internal tuberosity of the tibia, with complete success. He exhibited the child a year or two after the operation, when she could walk well. In his opinion transplantation of the tubercle of the tibia was the most successful operation, and next to that, transplanting the semitendinosus tendon appeared to be successful in preventing the patella from slipping over the condyle.

Mr. MORRISTON DAVIES, in reply, said that while there was no doubt that transplantation of the tubercle of the tibia did produce very satisfactory results in many cases, yet when the condition occurred in early life and growth had gone on for some time after the patella had been dislocated, then before the operation was done, it was difficult to get the patella to ride satisfactorily over the condyles of the femur when the limb was flexed. The operation, therefore, of transplanting the tubercle of the tibia might require the additional procedure of removal of some part of the condyle of the femur so as to let the patella come into position.

Acute Periostitis of Rib with Separation of Sequestra (Non-tuberculous, Non-traumatic).

By HENRY CURTIS, F.R.C.S.

THE comparative rarity of the condition seems to justify the record of this case. The patient, a Hebrew boy, aged 14, was admitted to hospital on Friday, March 31, 1911, for pain in the chest. He first complained of pain over the heart, compared with the "sticking of needles," on the previous Monday, and it became steadily worse.

On admission, the facial expression was anxious, with a hectic flush on the cheeks, furred tongue; temperature 102·5° F.; pulse 112; respiration 28. No constipation. Right chest, front and lower axilla fuller than left, and slight but distinct œdema, with widely diffused, marked tenderness, which extended well over the contiguous upper abdomen in the right hepatic region, where there was considerable rigidity. Some loss of note over the right front below, but no abnormal breath-sounds. Leucocytosis of over 20,000.

Diagnosis: Empyema or acute abdominal suppuration—e.g., subphrenic abscess—were possibilities, but exploration of the chest negatived empyema; a second needle puncture striking what appeared to be bare rib, and evacuating pus. Following the needle down by an oblique incision downwards and forwards along the line of the fifth right rib,

pus was found welling out in large quantities from beneath detached, thickened, congested periosteum, between it and the rib. Free drainage above and below.

The wounds closed by May 17, but two sequestra were removed on September 19; one from the upper one in the anterior axillary line, the second from the anterior wound in the parasternal line; the third, and largest, sequestrum was removed from the lower wound on November 11, both incisions now being soundly healed.

The pathologist's report negatived tubercle, a pure culture of *Staphylococcus pyogenes aureus* being obtained. There was no history of injury.

Case of Healed Double Empyema.

By E. M. CORNER, M.C., and L. GRANT, M.D.

E. K., MALE, aged 29, was admitted with both pleural cavities containing pus, to the Passmore Edwards Hospital, Wood Green, under the care of Dr. Leonard Grant, on Monday, September 11, 1911. A rib was resected on the right side, as that side contained most fluid; three pints were withdrawn. Next day (September 12) one pint of pus was withdrawn by aspiration from the left side, and on September 13 a rib was resected on that side.

The left side healed and closed in eleven weeks, the right side in about ten and a half weeks. Since then the man has made steady progress, adding 3 lb. to his weight the week previous to the last time he was seen. His heart is still displaced, and the pulse-rate still rapid.

The case is shown to elicit information on the following questions:—

(1) Should both pleuræ be opened at once?
(2) If not, which side should be opened first?
(3) Will the heart remain displaced?
(4) Will the pulse-rate return to normal?
(5) What are the prospects of his living a useful life?

DISCUSSION.

Mr. CORNER added that the case was an unusual one. He did not see why, when each lung was compressed above atmospheric pressure with pus, and when the patient was already running the risk of operation, both pleuræ should not be opened at once.

Dr. LEONARD GRANT said the patient's occupation was that of a boot-repairer. He had never had a serious illness until this. He was seen by his colleague on August 16, when he complained of pain in the episternal notch, and thought he had swallowed a bone. Two days afterwards there were physical signs at the right base indicating pneumonia and pleurisy. The whole lower lobe was affected. There was a similar condition on the left side eight days later. For three weeks the temperature ranged between 100° F. and 105° F. On September 10 he was aspirated, and pus was found in both pleuræ, and next day he was removed to the hospital, under Mr. Corner. After the resections the left side healed in eleven weeks and the right in ten and a half weeks. The man had put on weight, the heart was coming back towards the normal but was still displaced. His pulse was still above 100, but his general condition was vastly improved.

Mr. MORRISTON DAVIES, in regard to the recovery of the heart in such cases, said that the return of the heart to the normal position depended on how soon the empyema had been opened up. If the case was one of acute empyema which was operated on at once, then probably the adhesions which developed were not great and the thickening of the pleura inconsiderable. In such cases it was possible for almost complete return of the heart to the normal position to ensue. It was not possible, however, to say that the return of the heart would be complete in all cases, because after pneumonia, with possibly dry pleurisy, it was not unusual to see in skiagrams taken of the chest of such patients, even years afterwards, that there was still some displacement of the heart although the lungs and pleura appeared to be perfectly normal.

Anterior Gastro-jejunostomy; Perforated Jejunal Ulcer; Detachment of Jejunum; Excision of Ulcer; Gastro-duodenostomy.

By R. P. ROWLANDS, M.S.

E. R., MALE, aged 30. June, 1905: Anterior gastro-enterostomy, elsewhere, for pyloric obstruction. Complete relief for four and a half years. Then pain in abdomen to left of navel, partly relieved by food. Wasted. For three weeks before perforation pain unusually severe.

On September 2, 1910, at 6.30 p.m., during micturition, suddenly felt a terrible burning and sharp aching pain running downwards from navel to pubes. Pain not relieved by morphia. Patient admitted to Guy's Hospital seven hours after perforation, and abdomen opened half an hour later. Large amount of sero-purulent sanious fluid and gas escaped. The pelvis, both flanks, and the sub-diaphragmatic space on

both sides were full of this sero-purulent liquid. Anterior gastro-jejunostomy without entero-anastomosis found; proximal limb of jejunum greatly dilated and hypertrophied. Dense adhesion between its gastric end and parietes, indicating old perforation. Large perforation in distal limb 1 in. below gastro-jejunal opening. The latter only ¾ in. in diameter. Perforation closed, abdomen washed out, and tube inserted at lower angle of wound. Patient did well while in hospital, but soon afterwards symptoms returned in spite of continued medical treatment. Readmitted once for prolonged medical treatment. Improved a great deal, but had not been out many days before old pain and vomiting returned. Burning pain and feeling of bursting.

Operation, May, 1911: Jejunal ulcer now adherent to the liver, detached, and thus opened. Pylorus almost occluded and adherent high up and far back. Jejunum then detached from stomach. Ulceration around stoma and jejunal ulcer excised. Large jejunal wound closed transversely without narrowing of lumen. Gastric wound enlarged up and to the right for 3 in., and joined to similar opening made in front of first and second part of mobilized duodenum. Symptoms have abated. Patient has gained weight, although at work for several months.

Mr. ROWLANDS said the question had arisen as to whether a posterior gastro-jejunostomy should be done, or if the anterior wall of the stomach should be slit upwards to join to the front wall of the duodenum. He did the operation described, because the jejunum was rather damaged by the former operation. He could not do a Finney's operation, as the pylorus was adherent to the liver. The patient was not yet quite well, but was better than Mr. Rowlands had yet seen him. He resumed work six weeks after the operation, and had been able to continue at work ever since.

Gastric Carcinoma; Partial Gastrectomy two years ago.

By R. P. ROWLANDS, M.S.

J. W., AGED 51. Gastric symptoms for four months: vomiting, anorexia, indigestion, constipation, chronic hæmatemesis and melæna, very severe anæmia and wasting; lost 22 lb. in fifteen days.

Operation, November, 1909: Extensive, but not complete, gastrectomy; draining glands and omenta removed; duodenum closed; gastro-jejunostomy. Microscopical section shows carcinoma.

Patient now quite well and working. At first he could only take small meals; small remainder of stomach has dilated.

DISCUSSION.

Mr. ROWLANDS said the patient's corpuscles numbered under two millions, and he was very thin, so that something had to be done quickly. He preferred to do the complete operation at once, and not to do a preliminary gastro-jejunostomy. He did not do the Moynihan operation, as he preferred to save a little of the cardiac portion of the stomach, because of its utility in making an anastomosis to the jejunum. The patient did not work for four months after the operation, but he had been working since, as a liftman on the South London Railway. He had gained much weight, and there was no sign of recurrence. He was very deaf just before and after the operation, but since then his hearing had gradually improved. The deafness was partly due to anæmia and weakness. In reply to the President, he said Dr. Hertz examined the man as the only case of anything like complete gastrectomy he could find alive two years after operation in which the stomach sensations were good and found that the small cardiac remainder had dilated considerably. He could now eat an ordinary meal comfortably, whereas for several weeks he could only take small quantities.

Mr. W. G. SPENCER asked if Mr. Rowlands found any particular difficulty in dissecting off the great omentum from the transverse colon, and if afterwards there was any threatened paralysis of the transverse colon. He had no doubt Mr. Rowlands did it on account of the particular development of the great omentum in connexion with the stomach.

Mr. ROWLANDS replied that there was no great difficulty in removing the great omentum in this case; he removed practically the whole of it in a line with the gastric incision at the left boundary. The right boundary was at the duodenum. There was nothing unusual in the after-course of the case, except that the patient was so ill for a week or ten days that it was thought he would die; he was thought to have pulmonary embolism. The lung on the left side was dull, and he did not doubt there was an embolus in the base of it. The colon did not get unusually distended after the operation.

Excision of Large Myeloid Sarcoma of the Ischium.

By C. GORDON WATSON, F.R.C.S.

MALE, aged 51. History: twelve years ago operated on at St. Mark's Hospital for ischio-rectal abscess. On June 17, 1911, he came to St. Mark's Hospital complaining of "return of the abscess."

Condition on examination: There was an elastic swelling bulging into the ischio-rectal fossa, the rectum and adductor region of the thigh. The tumour was about the size of a coco-nut, was fixed to the pelvis and deep to the thigh muscles, which were wasted. Marked egg-shell crackling was obtained on firm pressure all over the tumour. X-ray examination showed that the tumour had a bony shell and was attached

to the ischium and pubes. An exploratory incision, followed by microscopic examination, proved the tumour to be a myeloma.

July 7: Operation at the Metropolitan Hospital. Ligature of the internal iliac artery.

July 10: Excision of the tumour with the patient in lithotomy position. The greater part of the ramus of the ischium, the tuberosity and the descending ramus of the pubes were removed with the tumour. The adductors were very adherent to the tumour and were divided, together with the hamstrings. The sciatic nerve passed over the surface of the tumour, but was not injured. The internal pudic artery was ligatured. A portion of the right crus of the penis was removed. Hæmorrhage was not serious.

The tumour was composed of soft, vascular tissue, with a thin, bony shell, and consisted of giant cells. Recovery was good. The patient has good use of all his thigh muscles and walks well, without pain.

Mr. GORDON WATSON, in regard to the "egg-shell crackling," threw out the warning that in the case of myeloid sarcomata the testing for the crackling might split the capsule, and allow the tumour to spread into surrounding parts, become adherent to muscles, and stimulate it to grow more rapidly. He was of opinion that this had happened in the case under discussion. Although at the operation he divided the adductors, the man could use those muscles and walk with freedom.

Painful Osteo-arthritic Hip.

By C. GORDON WATSON, F.R.C.S.

MALE, aged 64; subtrochanteric osteotomy of femur; relief of pain. The man had fallen from a scaffold three years before, and had been treated at the West Ham Hospital. For three years he had had constant pain in the hip. The X-ray picture showed that he had had a fracture of the neck of the femur, and there was much osteo-arthritic change in the head. He also had marked adduction. There was both real and apparent shortening, and the man was willing to submit to the operation suggested by Mr. Watson. He performed subtrochanteric osteotomy to relieve the adduction a year ago; the man had never had pain since, and could walk and work well.

Mr. GORDON WATSON added that he had operated upon a woman, aged 62, with the same condition, in whom the adduction was more marked. The result in her case was equally successful, and he would like to know if other Fellows had tried the operation, and, if so, with what results. He showed a skiagram of the condition since operation.

DISCUSSION.

Mr. W. G. SPENCER said, with regard to Mr. Gordon Watson's first case, that he had had one case of sarcoma occupying about the same position, in a football player, aged 20, who had been treated a long time for sciatica. On examination he found a sarcoma, markedly pulsating; it had been there about a year. He could only partially remove that growth, in spite of controlling the internal iliac artery. The patient soon had signs of secondary extension, with sarcomatous glands in the abdomen, and ascites. Although endosteal, it was of malignant type. With regard to the second case, probably Mr. Watson would say that, owing to the fracture, the lines of pressure had been put wrong, and that the subtrochanteric osteotomy directed the man's weight more in the line it should go. It was interesting, because even in osteo-arthritis the pain was probably due to the weight of the body going in the wrong direction.

Mr. ROWLANDS said he had one remarkable case bearing on Mr. Gordon Watson's second case. It was that of a woman who had flexion of the right hip following upon some form of arthritis soon after birth. She was middle-aged, very stout, and wore an instrument for supporting the limb, and a high boot. She was in great pain; there was flexion and some adduction. He did not advise anything to be done, but one day when going downstairs in a hurry she fell, sat on her foot, smashed the femur, and cured herself. She was now free from pain and could walk fourteen miles.

Mr. A. E. BARKER asked if the movement of the joint was fairly good. He wondered whether the man in walking was relying upon any false joint. The movements appeared to be particularly good.

Mr. GORDON WATSON, in reply, said Mr. Barker could judge for himself by the skiagram that the joint was good. In reply to Mr. Spencer, he was induced to do the operation in consequence of the success which followed the operation he performed on the woman referred to. In her case the operation was undertaken for relief of extreme adduction to enable her to walk. Complete relief of pain followed. He believed that the pain which occurred on walking was mainly due to the unequal distribution of weight on the adducted side.

Two Cases of Rodent Ulcer treated by Fulguration.

By P. TURNER, M.S., and C. IREDELL, M.D.

Case I.—W. M., male, aged 56, was admitted to Guy's Hospital on May 29, 1910, for a large rodent ulcer which had perforated the right cheek from the anterior border of the masseter to the angle of the mouth, and had also invaded the muco-periosteum of both the superior and the inferior maxillæ. The disease appeared nineteen years before in the form of a pimple which did not give rise to any trouble until five

years ago, when it began to ulcerate. In 1907 he had X-ray treatment, under which the ulcer cicatrized. About a year before admission the scar broke down and the ulceration rapidly extended. Radium and X-ray treatment were tried for six months, but without benefit. On June 2 chloroform was administered and the ulcer curetted and scraped, when a large backward extension deep to the masseter and involving the mucous membrane of the cheek was found. When the ulcer had been thoroughly scraped the resulting raw surface was cauterized by the use of the high frequency current (diathermy). After ten days a number of superficial sloughs separated, leaving a healthy granulating surface. A sequestrum also separated from the superior maxilla, leaving a large opening into the antrum. The ulcer completely cicatrized in about three months. The obturator for the gap in the cheek was suggested and designed by Mr. F. J. Pearce, Assistant Dental Surgeon to Guy's Hospital.

Case II.—C. A. N., male, aged 57, came under observation in 1908. History: In 1896 patient noticed a small sore on the right cheek, about half an inch from the ala of the nose. In 1898 it was excised, and recurred two years later. It was then scraped, and, later on, cauterized on several different occasions. In 1902, X-ray treatment was started, and he had several hundred treatments. When first seen he had an ulcer about an inch in diameter on the left cheek, equidistant from the left side of the nose, eye, and lip. He was treated with 22½ mg. of radium, having upwards of fifty applications of thirty-six hours each during the three succeeding years. A piece of lead 1 mm. thick was placed between the radium and the patient. He also had three treatments with carbon dioxide snow. In October, 1910, the ulcer, which had never healed up, seemed to grow more rapidly, and in January, 1911, he was seen by a surgeon, who suggested removing the whole of the superior maxilla. At this time the lip was becoming involved and also the ala of the nose. The patient refused to undergo the operation suggested and was seen again in May. The left upper lip was then destroyed and a good deal of the left side of the nose. Ulceration had also spread on to the left malar bone. The patient was anæsthetized and the edges of the ulcer were cauterized by the method of using high frequency known as diathermy. The right eye being blind, the patient was most anxious that no harm should be done to the left eye, and therefore the part of the ulcer near the inner canthus was left untreated. Since the operation there has been no spread of the ulcer except towards the eye. This patient is shown as an example of treatment by diathermy and also to obtain suggestions for his future treatment.

DISCUSSION.

Dr. C. E. IREDELL, referring to the first case, said it might be stated that radium in a larger dose might have been used, but the second patient had an application of a £400 piece of radium for thirty-six hours each sitting, as well as treatment with smaller pieces. Therefore radium had had a fair trial. Mr. Turner and he would be glad to hear further suggestions as to treatment. He feared that the patient might lose his eye. There was no doubt that the diathermy treatment was much the same as using a cautery; much heat was developed, and the tissues beneath could be seen to be coagulated, if not even burned. Thus it might be suggested that the cautery would be as useful in such a case. He doubted this, and thought if these patients had had the actual cautery the condition would have recurred already. He suggested that by diathermy the heat penetrated more deeply.

Mr. SPENCER said the safest way would be to run the actual cautery round the orbit now. Applying that to the margin would not hurt the eye, and might save it.

Case of Achondroplasia.

By V. ZACHARY COPE, M.S.

A. F., MALE, aged 9, is about the size of a child aged 5. He is intelligent for his age. Stunted growth noted since he was about 2 years old. All limbs much shorter than normal, and long bones exhibit marked enlargements in region of epiphysis, which radiography shows to be due mainly to thickening and irregular ossification of end of diaphyses and deficient development of epiphyses. No curves like those of rickets. Fingers and toes short and thick. Vault of skull well developed; base rather smaller than normal. Very slight beading of ribs. Head of fibula higher than normal on both sides and takes part in knee-joint on one side. Deficient development of the carpal bones on both sides.

Three brothers and two sisters, all well developed. This boy is the youngest child; mother was aged 41 at his birth. She has a marked enlargement of the right lobe of thyroid, which has been present as long as she can remember.

The case is suggestive of relation between achondroplasia and derangement of thyroid.

Mr. ZACHARY COPE said that the mother had a well-marked goitre, chiefly on the right side. Some thought that anomaly of the thyroid secretion was a factor in the causation. The condition was said to arise between the third and sixth months of fœtal life. Occasionally it developed after birth. In this case the skiagram of the wrists showed deficient ossification. On both sides the os magnum and unciform were visible, but in the left carpus no other ossific

nucleus could be seen, whilst on the right side there were only faint traces of other bony deposits. This would appear to indicate active disease after birth. The fibula was unusually long relatively to the tibia, and on the right side took part in the articular plateau of the knee-joint.

Congenital Specific Stenosis of the Fauces and Pharynx.

By W. G. SPENCER, M.S.

FEMALE, aged 19, shows persistent nodes and a gummatous scar on the scalp. In June she was admitted under Mr. Arthur Evans for severe ulceration of the fauces and pharynx. Tracheotomy was done and salvarsan administered, after which the ulceration rapidly healed, resulting in stenosis. Mr. Evans then performed a plastic operation upon the soft palate and fauces; the tracheotomy tube was left out, and the patient was discharged on August 19, breathing and swallowing well.

On October 15, she was readmitted, under Mr. Spencer, with dyspnœa; hardly able to swallow fluids. After reintroducing the tracheotomy tube it was found that the nasopharynx would allow of the passage of only a very small catheter, whilst a No. 10 passed with difficulty through the fauces and pharynx. On gently dilating to admit a finger, the fauces and pharynx, down to the larynx, were found to be changed into a very dense ring, in which the vessels of the neck had become involved. Therefore a No. 10 œsophageal tube was passed through the left nostril down the pharynx, through which the patient was at first fed, but soon she swallowed beside it. Later, a full-sized œsophageal cannula was passed, beside which the patient can now swallow ordinary hospital diet, and has rapidly put on flesh. She can herself take out the tube when she goes out of doors and put it back before a meal. For the present, therefore, the necessity for gastrostomy has been avoided. The tracheotomy tube will be required permanently. The very dense and extensive fibrosis which has followed the rapid healing of the ulceration after salvarsan is a disappointment.

Mr. SPENCER added that the patient had had ulceration of the nasopharynx down to the epiglottis for nearly four years; this healed up rapidly under the salvarsan given by Mr. Evans, but was followed by the dense ring from the palate and tongue to the epiglottis and upper part of the larynx. He hoped that by keeping up the dilatation the fibrosis would disappear in a year or so. He believed it was somewhat less since she had been wearing the tube through her nose. The fibrosis was very marked soon after the healing.

A Case of Ligation of the Innominate Artery for Subclavian Aneurysm.

By C. A. BALLANCE, M.V.O., M.S.

A MAN, aged 43, a clerk, was admitted to St. Thomas's Hospital on October 23, 1911, with a large pulsating swelling above the right clavicle. His past history was good; there was no history of specific disease. The swelling above the clavicle had been noticed for eighteen months. It had slowly increased in size. There had been very little local discomfort, but occasionally a little pain had been experienced down the arm. There had been no cough and no hoarseness of voice.

On admission there was a prominent pulsating swelling situated above the clavicle on the right side (fig. 1). Above the clavicle it extended more than 2 in., externally it reached beyond the middle of the clavicle and internally nearly to the head of the bone. The pulsation was expansile. The right radial pulse was markedly feebler than the left. The carotid and temporal pulses were equal. The right pupil was contracted. No tracheal tug. No pressure on bronchus detected. Some impairment of percussion note below right clavicle. Larynx normal; vocal cords move well and equally. No thoracic disease detected. Arteries thickened generally. Wassermann reaction negative. X-ray examination: There is no evidence of thoracic aneurysm. The aneurysm obscures the apex of the right lung and its edge is at the level of the lower border of the first costal cartilage; the edge is convex downwards.

Operation (November 1): An oblique incision was first made in order to expose, by Sédillot's method, the first part of the right subclavian artery. The sternal and clavicular origins of the sterno-mastoid were separated and retracted. The adjoining margins of the sterno-hyoid and internal jugular vein were then carefully retracted, when it became evident that the aneurysm had extended so as to cover the first part of the right subclavian artery. The ligature of this artery was then abandoned and the superficial incision extended so as to allow of the exposure of the innominate artery by incision in the middle line. The right sterno-hyoid and sterno-thyroid muscles were divided and the lower part of the right common carotid artery defined.

It soon became obvious that the innominate artery was not to be found in the neck and the upper half of the manubrium sterni was removed. Separating the pleural margins, the right common carotid was followed downwards till the bifurcation of the innominate was discovered low down in the superior mediastinum. The first half-inch of the first part of the right subclavian artery was not aneurysmal, but the close proximity of the aneurysm made further dissection of

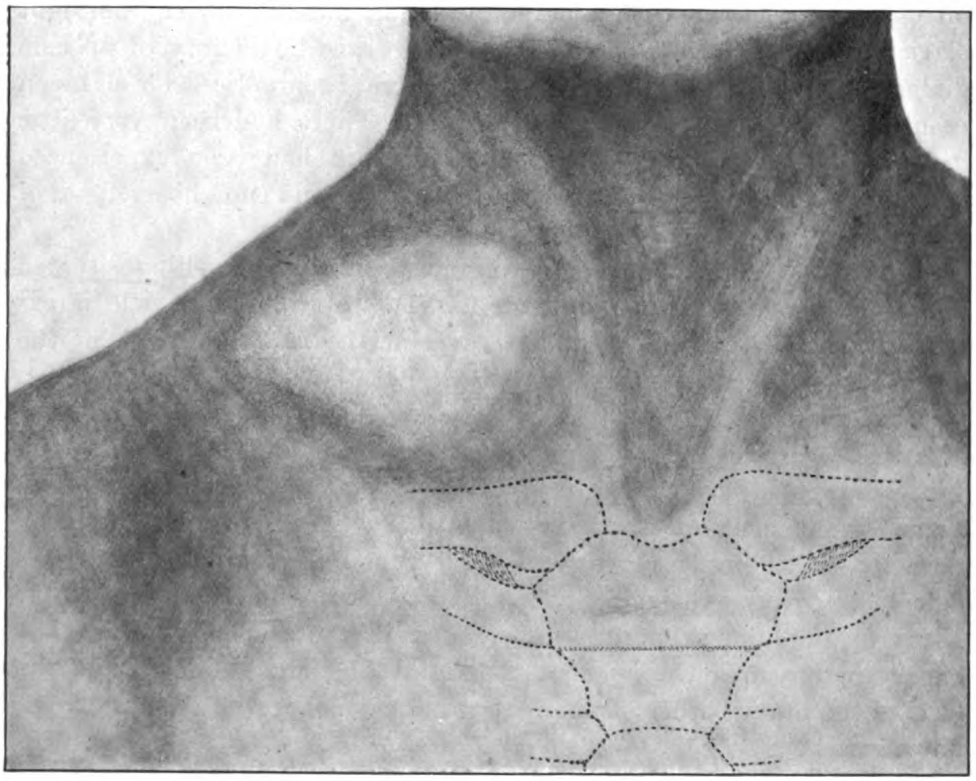


FIG. 1.

Aneurysm of the subclavian artery (from a cast of the patient's neck before operation). The line of section of the manubrium is shown by a dotted line. The cartilages of the first ribs were divided close to the sternum. The sterno-clavicular joints were cut through. The rhomboid ligaments prevented displacement of the clavicles.

this vessel inadvisable. A stay knot of two strands of kangaroo tendon was placed around the innominate artery, so as to occlude it without rupturing its coats. The wound was closed and dressed in the usual way.

Progress since operation: The pulsation of the aneurysm ceased

from the moment of ligation of the innominate and has not returned. No pulse has been felt in any of the arteries of the upper extremity since the operation, but at no time has there been any lack of warmth in the limb. At first there was some numbness and tingling in the fingers, especially in the little and ring fingers; the fingers, too, were stiff and moved with difficulty. For twenty-four hours after the operation the right pupil was dilated; it then again contracted and has

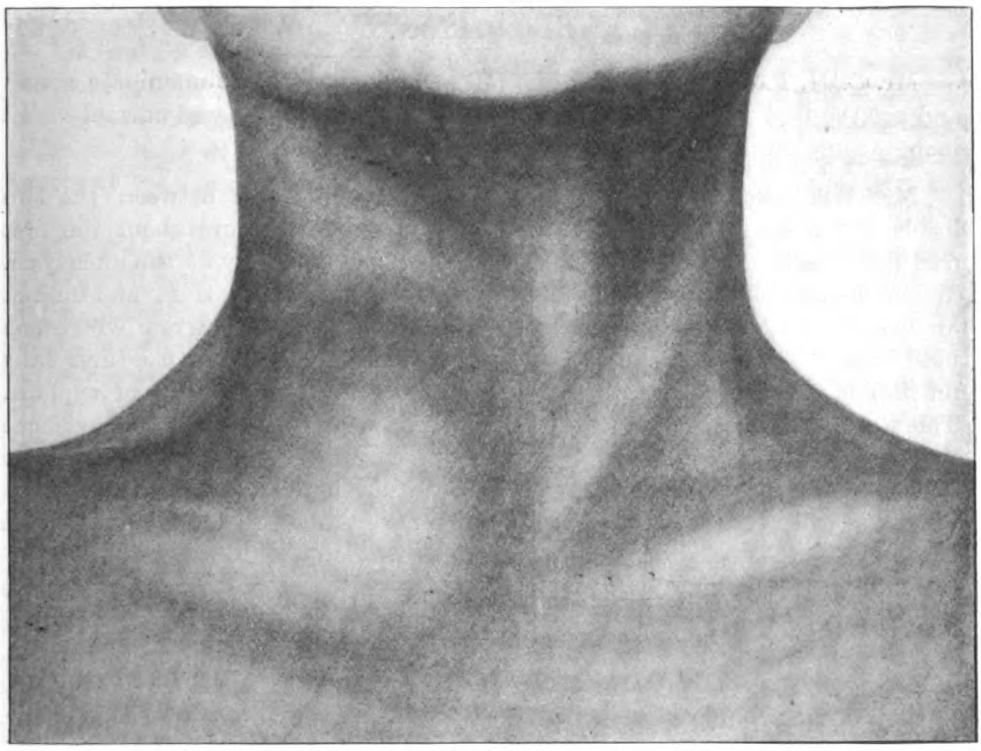


FIG. 2.

This figure is inserted for contrast with fig. 1.

Aneurysm of the upper half of the innominate artery, of the first and second parts of the right subclavian artery and of the root of the right common carotid artery. (From a cast of the patient's neck before operation.)

remained so since. The tumour is slowly decreasing in size. Massage of the arm was commenced on the twelfth day and the patient got up on the twenty-second day after the operation.

Remarks.—The symptoms clearly pointed to subclavian aneurysm and excluded aneurysm of the innominate artery or of the aorta. The aneurysm probably involved the second and third portions of the artery

and probably also the distal part of the first portion. The contracted pupil made it likely that the distal part of the first portion of the artery was aneurysmal. The view expressed of the extent of the artery which was aneurysmal is confirmed by museum preparations and experience of previous cases. For the purpose of comparing the superficial aspect a drawing of a cast of a case of aneurysm at the bifurcation of the innominate artery (fig. 2) is added.

DISCUSSION.

Mr. C. H. FAGGE asked why Mr. Ballance ligatured the innominate artery and not the first part of the subclavian, which he had stated was normal for an inch or more on the proximal side of the aneurysm.

Mr. BALLANCE said he had had experience in going between the two heads of the sterno-mastoid, and was afraid to put a ligature about the first part of the subclavian artery, because of the near proximity of the aneurysm. When one got near the aneurysmal wall one found it very thin, and did not know quite what would happen. The cases of subclavian aneurysm which one used to see seemed to be almost always in sailors, who had to use their arms much and were subjected to strains, and they were all subjects of syphilis. This was the only case of external aneurysm in which he thought the evidence was against the patient having had syphilis. The man seemed to have had no strain or injury, nor had he had typhoid. In another case under his care, two years ago, with an aneurysm in much the same position, it was concluded that it was not pure subclavian aneurysm, but one which commenced at the bifurcation of the innominate and involved all three arteries. It suddenly began to extend, and he told the patient the condition was desperate, and the only thing was for an attempt to be made to tie the great artery beneath his aneurysm. When he saw the patient at mid-day it had definitely extended since the morning of the same day. That afternoon the patient was given an anæsthetic, and Mr. Ballance made an incision on the left side of the median line so as to avoid the aneurysm. He made a resection of a portion of the manubrium and cut through the left clavicle and the first and second ribs. In separating with the finger the margins of the pleuræ—which was easy, and which he saw Trendelenburg do when he removed a clot from the pulmonary artery—he came down upon the artery underneath the aneurysm, and could then feel distinctly that there was a portion of innominate artery which was not aneurysmal. He put a ligature round the part which was not aneurysmal, and the pulsation in the extending aneurysm ceased. The wound was being wiped and they were getting ready to close it up, when there was a sudden gush of blood, the aneurysm having broken. At that moment nothing was being done. He put his finger in the hole in the aneurysm. The bleeding was stopped, and he plugged the sac with gauze, but unfortunately the man died next day. It was because of that calamity that he did not tie the first part of the subclavian artery.

The PRESIDENT (Sir W. Osler, Bt., F.R.S.) remarked that a point about the pupil symptoms was very interesting. Many cases of inequality of pupil in aneurysm were difficult to explain by pressure on the sympathetic. The view had been put forward, and seemed true in some instances, that it was a matter of blood-pressure in the eye and iris; when there was low blood-pressure the pupil dilated and with high blood-pressure it contracted. The patient's right pupil was at first contracted, and yet for twenty-four hours after the operation the narrow pupil dilated, which supported the view of Wall and Walker that it was a question of blood-pressure.

Mr. A. E. BARKER suggested that it would be well to have the Wassermann reaction done again. One observation on such a case did not seem to be enough. Seeing that the man was said not to have had syphilis, two or three tests ought to be made. Comparatively few aneurysms were seen nowadays, and he supposed the reason was that syphilis was now under better control.

Mr. FAGGE thanked Mr. Ballance for narrating his most interesting experience, which clearly showed his reason for ligaturing the innominate in this case and completely answered Mr. Fagge's question; this had been prompted by a somewhat similar difficulty, for when applying a catgut ligature to the common femoral artery during the excision of a large aneurysm in Scarpa's triangle the artery had torn through and the external iliac had to be made use of.[1]. From his own experience Mr. Fagge had fought shy of applying ligatures close to aneurysms owing to far-reaching gross arterial disease, and as he had never read any opinions confirming his own he had been anxious to obtain Mr. Ballance's. Mr. Fagge had for some years looked after a woman with a large aneurysm involving the innominate, right subclavian and right common carotid. Mr. L. A. Dunn operated on her seven or eight years ago, ligaturing the common carotid and subclavian in its third part. Pulsation had ceased for four or five years, but more recently it had returned, the tumour had increased, and laryngeal paralysis had become troublesome. The woman died during the past summer, but for a long time the disease had been kept in check, and she had supported a paralysed husband and family by doing washing and working as a charwoman.

Mr. ROWLANDS said a man with a similar condition who had had a like operation was alive three years afterwards. In his case the tying of the innominate was impracticable, because the X-rays showed there was a dilatation of the aortic arch as well. He came into hospital on account of severe pain and extension of the swelling. His operation of ligaturing the distal arteries did not stop the pulsation, but it diminished the swelling, and the man returned to work, doing hard work for two years. But recently he was not able to work because of the increasing swelling and pain.

[1] See *Proceedings*, 1908, i, p. 232.

Mr. W. G. SPENCER referred to Mr. Heath's specimen in the Westminster Hospital Museum; the patient lived nearly twenty years after ligature of both the carotid and the subclavian. [Mr. Ballance: It was not an aneurysm. There was dilatation of the aorta.] Mr. Stonham had operated upon an aneurysm of the left subclavian which was an equivalent operation on the left side to Mr. Ballance's on the right. Mr. Stonham had to go quite low and tie the first part of the subclavian. In order to complete it he had to tie the distal part and some of the branches. He saw the man recently, seven years afterwards, and his aneurysm had remained cured, but there was marked dilatation of the transverse aorta.

Mr. BALLANCE, in reply, agreed that the Wassermann reaction should be done again. Mr. Fagge had mentioned some interesting cases, but did not refer to a very extraordinary case in Guy's Hospital Museum in which an aneurysm had blocked the innominate and the left carotid artery, and the patient lived a year without a carotid or right radial pulse. He believed it to be the most remarkable specimen of blocking of great arteries in existence. Mr. Fagge raised the question of disease of arteries in the neighbourhood of aneurysms which were given off by them. It was an old question, which was settled by John Hunter. Hunter maintained, against the rest of his colleagues at St. George's Hospital, that the artery was not diseased above the aneurysm, at least not to the extent that he could not put a ligature upon it. Hunter said that his colleagues successfully performed amputation for cases of popliteal aneurysm and successfully tied the artery, whereas he preferred to tie the artery and not to amputate. Mr. Ballance was sure that in many cases the aneurysm was the only part of the artery seriously diseased. There were many cases of aneurysm of the subclavian which were limited aneurysms, in which the first portion of the artery was healthy, and that was one of the reasons which led him to try first to ligature the first part of the artery.

Multiple Melanotic Sarcomata of the Skin, possibly Secondary to a Melanotic Sarcoma of the Skin, removed Eighteen Months ago.

By V. WARREN LOW, F.R.C.S.

A. H., FEMALE, aged 34, had always had a mole on the inner side of the left leg below the knee. Three years ago this was knocked off, and bled, and a scab formed. When this either fell off or was removed, a raw, bleeding surface remained. Gradually a dark reddish swelling began to form. The patient was first seen in April, 1910, when the tumour had

reached the size of a pigeon's egg. It was pigmented, and presented a raw surface covered by a scab. There were also several small pedunculated and pigmented tumours attached to the skin on the left thigh. The left femoral glands were enlarged. On April 11, 1910, the tumour was excised with a wide area of skin around. The femoral glands were also removed, and the three pedunculated moles on the left thigh.

The material excised was examined microscopically by Dr. Shaw. The main growth proved to be a melanotic sarcoma, the glands contained secondary deposits similar in character to the growth. The pedunculated tumours showed the ordinary character of simple moles.

In January, 1911, the woman again came to the hospital with some small nodules in the scar of the old wound; these were removed and



Multiple melanotic sarcomata of the skin.

microscopically showed the same characters as the original growth. Shortly afterwards a number of small superficial pigmented tumours began to make their appearance on the left leg below the knee, and this occurrence still continues. At first these made their appearance as small reddish spots in the skin, they rapidly increased in size and became pigmented; they generally project above the level of the skin, but lately a few small subcutaneous tumours have appeared. During this period the glands in the groin have again enlarged, and now a large mass of glands can be felt in the left iliac fossa extending to the brim of the pelvis. The more superficial glands appear darkish in colour under the skin.

The patient has been treated for three months by X-rays by Dr. Robert Knox, who states in his report that there has not been any

improvement. The patient also has some unpigmented moles on the left side of the neck and on the right scapula; a small pedunculated tumour has also appeared on the left thigh in the vicinity of the scars of those removed eighteen months ago. The patient, who appears to be otherwise healthy, is married, and has two children, one of whom has had a large mole removed below the right eye.

Pathological Report by E. H. Shaw.

(1) Main growth of leg (which was soft, pedunculated, white for the most part, with black patches in places, and measured 1¼ in. in diameter), roughly divided into two parts; the outer part is partly covered by an irregular layer of squamous epithelium. The cells of the tumour are arranged in masses separated by connective tissue, they are small and large in size, and very variable in shape, some round and many spindle-shaped in outline. All the stages of transition from round to spindle shape are well seen. Many cells are deeply pigmented, and in some instances whole masses of cells are of a golden-brown colour. Many of the pigmented cells are very large, and some contain two or more nuclei. The deeper part of the tumour is composed of smaller cells mostly round in shape, among these are many well-formed blood-vessels, separated from the cells by a ring of loose connective tissue. The arrangement and character of the cells in the outer part are like that seen in "fleshy" moles, and the tumour undoubtedly arose from one of these. It is now a mixed round and spindle-celled melanotic sarcoma.

(2) Inguinal gland: The gland tissue is almost entirely replaced by growth. The growth is composed for the most part of round cells; spindle cells are present in places. Masses of pigmented cells are seen here and there. The tumour is a typical sarcoma.

(3) Two very small moles with wide pedicle. They are covered with squamous epithelium, beneath this there are groups and columns of cells rounded in shape. Many of the cells contain two nuclei and some contain three or four. The groups are separated by well-formed connective tissue; many cells lie free in the loose tissue just under the epithelium.

Clinical Section.

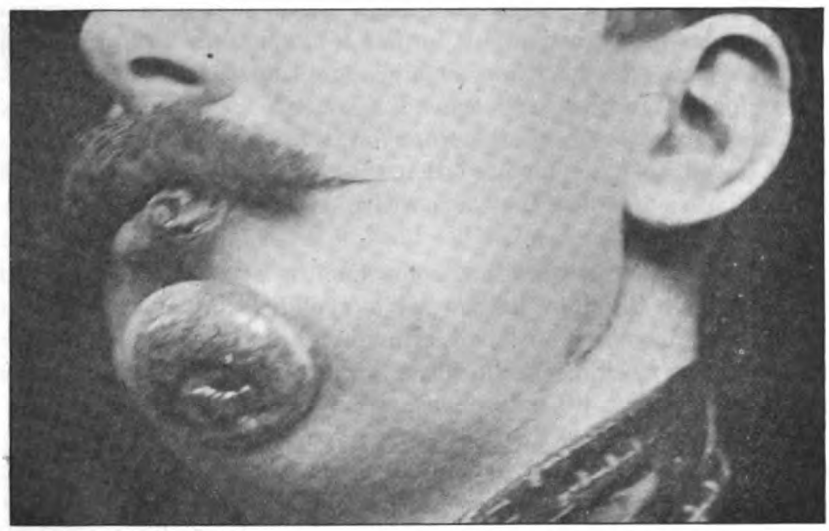
January 12, 1912.

Sir Wm. Osler, Bt., F.R.S., President of the Section, in the Chair.

Primary Chancres of the Lip.

By Philip Turner, M.S.

W. R., male, aged 27, was admitted to Guy's Hospital for an ulcerated swelling of the lower lip, which was first noticed eight weeks ago. The swelling, which was at first soft, rapidly increased in size,



Primary chancres of the lip.

and in four weeks the skin broke and it commenced to discharge. About this time a second swelling appeared on the free margin of the lip. For two weeks the patient has complained of a sore throat. There has never at any time been any sore on the penis.

When seen on January 1 there was a round, raised swelling on the chin just to the left of the mid-line. In the centre of this was a circular ulcer about 1 in. in diameter, the base of which was composed of unhealthy-looking granulation tissue discharging thin pus. A second small indurated area, with slight superficial ulceration, was present on the mucous surface of the lip at its junction with the skin. The submental, submaxillary, and cervical glands were enlarged but not tender, and the axillary glands were also palpable. There were ill-defined patches of a dull red erythematous rash on the chest and abdomen. Superficial ulcers with grey bases were present on both tonsils.

DISCUSSION.

Mr. TURNER added that there seemed to have been a double infection, because both the lesions must be described as primary chancres, although one appeared a short time before the other; the patient was not certain of the exact interval that elapsed between the appearance of the sores. He noticed that Fournier, in his book on Syphilis said: "Labial chancres are single in 95 per cent. of the cases." He also described four or five types. One he called "papulo-hypertrophic," which was rare, but very essential to know, on account of the regrettable errors in diagnosis which not infrequently had led to extensive operative interference. "In that form the chancre was very exuberant, just like a neoplastic infiltration. It formed a tumour, and assumed the aspect of a malignant new growth. Thus such chancre tumours had more than once been taken for epithelioma, and treated as such." This description exactly described the appearance of the sore on the chin in the present case. Dr. G. W. Goodhart had reported that *Spirochæta pallida* was present in smears from both sores and that a positive Wassermann reaction had been obtained.

Dr. J. D. ROLLESTON said that some months ago, at the Section for the Study of Disease in Children, he showed a syphilitic infant to illustrate the difficulty in the diagnosis of labial chancres.[1] Six weeks after marriage a woman developed a swelling of the lower lip, for which she sought advice at a London hospital, but its nature was not recognized; it was treated as a poisoned lip, and only local treatment was given. She gave birth to a child a year later, and when it was 5 months old it came to hospital with nasal diphtheria, and presented a number of syphilitic phenomena. He sent for the mother, and she showed a scar in the middle of the lower lip, typical of that left by labial chancre. There was no doubt about syphilis in the father, who was indifferently treated for it. An interesting feature in Mr. Turner's case was the early appearance of the secondary rash, which was a frequent occurrence after labial chancres.[2]

[1] *Proceedings*, 1911, iv (Child. Sect.), p. 96.

[2] Gaucher and Milian, *Ann. de derm. et de syph.*, Par., 1908, 4me sér., iv, p. 226; Luisi, *Thèses de Par.*, 1904-05, No. 39; Garrie, *Thèses de Par.*, 1906-07, No. 298.

Dr. GALLOWAY remarked that the difference in the appearance of syphilitic chancres of the face was very striking. When the chancre occurred on the muco-cutaneous surface of the lip where there were no hairs, the induration and thickening were sometimes very slight, a crack with a little thin parchment-like induration marking the site of inoculation. When it occurred on the cheek and the hairy part of the face in men the tumour formed, as in Mr. Turner's case, was sometimes excessive. This was due, no doubt, in part to the virus of the infection, but perhaps mainly to the very considerable pyogenic infection which was also liable to be present and to pass into the hair-follicles, causing a deep-seated, inflammatory œdema. In intermediate positions all degrees of induration could be observed. Even more striking than the tumours of the syphilitic chancre were those produced by the inoculation of "horse-pox" in the human subject. This infection usually occurred in the face of men who had to tend the infected horses, and the great tumour-like swelling which resulted not only produced a remarkable appearance, but serious symptoms attended with high fever. In such cases also probably the greater amount of the tumefaction and of the inflammatory reaction was due to the pyogenic infection which occurred simultaneously with the specific infection of the disease.

The PRESIDENT (Sir Wm. Osler, Bt., F.R.S.) said that in cow-pox there might be an accidental infection in the face, which might look like a chancre. He had seen a case, reported by Dr. Crookshank, in which the sore was beneath the right eye. There was very little swelling, and there were indurated edges, and a black eschar on the top. It looked like an ordinary hard chancre with firm edges, but there was no doubt what it was. The man was a milker of cows, and he had cow-pox. The man's grandmother on the same farm was inoculated, as a girl, with the same disease on her forehead. Lip chancres were sometimes overlooked. He remembered the case of a young girl who had a very obscure arthritis, with fever, and enlarged glands. There had been no suspicion that the slightly raised hard sore on the lip was a chancre.

A Case of Tumour below the Right Hypochondrium. ? Riedel's Lobe.

By HERBERT FRENCH, M.D.

P. S., AGED 34, a married woman who had six children, the youngest of whom was now 2 years old, came to the hospital "for a bottle of medicine," complaining of indeterminate symptoms of which the chief were a feeling of being less strong than she was, a deficiency in her usual appetite, with a tendency to be sick in the morning. There were no symptoms directly referable to the abdominal tumour, which was only

found as the result of ordinary routine examination. The woman herself has defective teeth, which probably account for her vague stomach symptoms, and she looks strong and well in herself. There is more than the usual degree of abdominal pigmentation, but this, she says, has been present for years. The patient has long had a tendency to constipation, but this has shown no increase of late.

Extending down from beneath the right costal margin for a distance of 3½ in. there is a firm, painless, smooth tumour, more or less the shape and size of a goose's egg; it lies external to the right mid-clavicular line and abuts upon the right flank, but does not extend backwards so as to fill up the loin. It can be grasped easily bimanually or by one hand when the fingers are behind and the thumb in front. It moves up and down with respiration, is dull to percussion, and the dullness over it is continuous with the hepatic dullness. There is no general enlargement of the liver. The lump seems to be too much to the right to be a gall-bladder; it does not fill up the loin as a renal tumour or a suprarenal tumour would, besides which the right kidney can be felt separately behind it, and the urine contains no albumin, sugar, pus, or blood; the mass is smooth and permanent, and does not suggest from the feel of it that it is due to fæcal accumulation in the bowel or to a carcinoma of the hepatic flexure, and although the diagnosis is open to question the case is brought before the Section as an example of a Riedel's lobe or semi-detached portion of the liver. There being no symptoms referable to the tumour, it seems to require no treatment.

DISCUSSION.

Dr. FRENCH added that it was easy to exclude malignancy on account of the duration of the case. The lump had been there for three years.

Mr. PHILIP TURNER said he had that afternoon operated on a distended gall-bladder in that situation, which before admission to the hospital had been diagnosed as hydronephrosis. In the present case he thought the regular, smooth outline and the mobility of the tumour were in favour of it being a gall-bladder, but if so, its situation was very unusual. Some years ago he was asked to operate on a small child who was supposed to have intussusception. There was an abdominal tumour which was thought to be due to that cause, but it was a Riedel's lobe, and that was the only time he had met with that condition.

Dr. POYNTON said that another condition which it was difficult to exclude was hydatid cyst, which sometimes projected in the same way. He had seen in children, also, tubercular collections attached to the peritoneum in the

region of the transverse colon, which seemed in shape like the gall-bladder, or a hydatid, or a pendulous neoplasm. The operation in one case was done under the idea that it was a hydatid. He advised exploratory operation, to see what the condition was, seeing that such serious possibilities were in view.

Mr. FAGGE said he had now under his care in Guy's Hospital a woman, aged 60, who was operated upon as a case of appendicular peritonitis because of the extreme tenderness and rigidity on the right half of the abdomen. When the patient was under the anæsthetic a distinct large lump could be felt below the right lobe of the liver, to which he had drawn attention, and said that it seemed to be too far to the right to be a distended gall-bladder. He made his incision in the appendix region, and passing his hand upwards found an acutely inflamed, distended gall-bladder, with local peritonitis; the incision was enlarged and cholecystotomy performed. He urged Dr. French to have the woman operated upon, not out of curiosity, but because be believed the woman was in a condition of some danger in which even the most conservative physician would advise an operation, if the organ affected were the appendix. He did not see why she should not submit to an operation which could carry no immediate risk, and which would rid her of an organ that was distended, probably owing to the cystic duct being blocked by a gall-stone. He suggested that the symptoms which brought the patient to Dr. French were due to her distended gall-bladder.

The PRESIDENT said that if it was a Riedel's lobe it was not of the ordinary type: none which he had felt had the rotundity and ball-like character. On the other hand, there was a type of tumour of the normal liver tissue which was very deceptive, namely, the portion of liver which was cut off by a syphilitic cirrhotic band. The liver might be nearly cut in half in the antero-posterior direction, and a large portion of the right lobe practically loose; it might be a very globular portion. He believed it to be gall-bladder, enlarged, hard and calcified. The possibility of it being hydatid could not be excluded. A few years ago he saw a hydatid cyst in that situation, which was thought to be gall-bladder. The woman was operated upon. He agreed with Dr. French that this case should not be operated upon. Certain cases of gall-bladder tumour disappeared without giving trouble. Some years ago he did a post-mortem examination on a Scot who left instructions that a post-mortem was to be made. For years he had had a tumour which had distressed him very much. It was movable in the upper right portion of the abdomen. It gave no trouble, and year by year it became smaller. The gall-bladder was completely contracted and contained a large gall-stone.

Dr. FRENCH, in reply, said he did not intend to have the patient operated upon, as he saw no grounds for so doing. She had no symptoms referable to the abdominal condition, and the tumour was known to have been there for three years. He did not consider that the mere presence of the tumour was a justification for operation.

Recurrent Aphasia with High Blood-pressure.

By F. Parkes Weber, M.D.

The patient, S. P., aged 51, a Polish-Jewish tailor in London, was admitted into the German Hospital on March 8, 1910, having had eight attacks of slight temporary aphasia during the last seven weeks before his admission. Each of these attacks of aphasia was accompanied by slight hemiparesis; in seven of them the paresis was on the right side, but in one of them on the left side. For six or seven weeks he had had headache, but there had been no vomiting or loss of consciousness. The patient was a well-nourished, intelligent man. He had optic neuritis, best marked in the right eye. The plantar reflex was of the extensor type (Babinski's phenomenon) in both feet, best marked in the left. The knee-jerks were active on both sides. The brachial systolic blood-pressure was 160 mm. Hg. and the radial arteries felt somewhat thickened. The urine was free from albumin and sugar, but a few tube-casts were detected by the centrifuge method. Nothing else abnormal was discovered. There was no history of syphilis. He was treated by iodide of potassium and had no further cerebral attacks. On April 10, 1910, when he left the Hospital, he said that all he had to complain of was slight headache in the morning. There had been no fever whilst he was under observation. The pulse had varied from 74 to 88 per minute. The daily quantity of his urine had averaged about 2,500 c.c. He had increased somewhat in body-weight. His brachial systolic blood-pressure was 145 mm. Hg. Later on (in June, 1910) it was 150 mm. Hg. When seen again recently (December 20, 1911), the patient said he had had no further cerebral attacks and had not taken any medicine for a year. His only complaint was that attempting to work brought on a headache at the top of his head. Nothing abnormal could be discovered in his heart, lungs, or abdominal organs. His brachial systolic blood-pressure was 140 mm. Hg. No paresis anywhere. The pupils were of medium size and reacted normally. The optic neuritis had subsided, but there was still slight blurring of the right optic disk. The vision was nearly up to the normal standard. The ophthalmoscopic examinations were made by Dr. R. Gruber, who likewise noted that the patient was red-green colour-blind. The plantar reflexes were of the normal (flexor) type. The Wassermann sero-reaction for syphilis gave

a negative result when kindly tried by Dr. E. E. Atkin, at the Lister Institute, in January, 1912. The question arises: Were the attacks of aphasia in 1910 due to a temporary organic cerebral disease or were they of the kind described by Dr. George Peabody in 1891, and recently discussed by Sir William Osler in the *Canadian Medical Association Journal* for October, 1911, under the heading "Transient Attacks of Aphasia and Paralyses in States of High Blood-pressure and Arterio-sclerosis"?

An example of attacks of temporary aphasia preceding a spontaneous hæmorrhage (in the neighbourhood of the thyroid gland) in a man, aged 54, with very high blood-pressure (up to 230 mm. Hg.) and chronic renal disease, was shown by Dr. Weber at the Clinical Society of London on March 22, 1907.[1]

DISCUSSION.

Dr. HERBERT FRENCH did not consider that a blood-pressure of 160 mm. of mercury could be regarded as *very* high in a man of this age; he would apply that term when it reached from 250 to 300 mm. The pressure was somewhat above the normal, but it was not very high.

The PRESIDENT considered that such cases were more common than was generally supposed or than the literature indicated. Most of the cases he had seen had been associated with arterio-sclerosis, or high blood-pressure, or both. He had seen a large number of such cases, of transient character. The complete absence of enduring paralysis, the recurrence, the fact that identical attacks occurred in connexion with Raynaud's disease coincident with spasm of the vessels of the finger, were points strongly in favour of the view that these attacks of transient monoplegia and transient aphasia were of the same nature. In certain instances one could see the spasm in the retinal arteries in association with various forms of amblyopia.

Chronic Splenomegaly of Uncertain Origin, with Persistent Leucopenia.

By F. PARKES WEBER, M.D.

THE patient, A. N., is a married Jewish woman, aged 23. Her spleen reaches a good hand's breadth below the left ribs, but does not feel hard. Otherwise the patient appears well, though rather pale. The history is that, after a confinement in August, 1910, she suffered

[1] *Trans. Clin. Soc. Lond.*, 1907, xl, p. 270.

from pains in the loins, headache, and giddiness. She had previously had no special illness; never malaria or jaundice. On admission to the German Hospital (October 6, 1910) she was found to have decided enlargement of the spleen, but, beyond some abnormality in the blood, nothing else abnormal was discovered. The red cells were 4,850,000 and the white cells only 2,575 to the cubic millimetre of blood; the hæmoglobin was 80 per cent. On October 30, 1910, whilst under arsenical treatment, the patient was suddenly attacked with severe abdominal pain and vomiting, and, as there was likewise free fluid in the peritoneal cavity (shown by movable dullness) the abdomen was opened by Dr. E. Michels. Nothing abnormal, however, was detected, excepting the presence of some ascites and enlargement of the spleen and liver; the capsule of the spleen was adherent to the surrounding parts; the peritoneum appeared extremely vascular. The patient recovered well from the operation and from the ascites, and left the Hospital on November 19, 1910. Since then she thinks she has had good health. Whilst in the Hospital occasional moderate fever was observed. Wassermann's sero-reaction for syphilis (Dr. H. R. Dean, at the Lister Institute) gave a negative result, and Pirquet's cuti-reaction for tuberculosis was likewise negative. A blood examination (by Dr. G. Dorner), on October 24, 1910, gave: Red cells, 4,222,200; hæmoglobin, 92 per cent.; white cells only 1,200. Differential count of white cells: Polymorphonuclear neutrophiles, 27·4 per cent.; small lymphocytes, 44 per cent.; large lymphocytes, 11·3 per cent.; eosinophiles, 9·3 per cent.; transitionals, 7·3 per cent.; myelocytes, 0·7 per cent. Two nucleated red cells were seen. The white blood cells numbered 2,680 on November 19, 1910, and 2,650 on January 2, 1911. A blood count in July, 1911 (when the patient was pregnant), gave: Red cells, 4,660,000, and white cells 3,500 in the cubic millimetre of blood. At that time the patient's disease might perhaps have been termed one of "Anæmia splenica sine anæmia." In October, 1911, she lost much blood at her confinement. The most recent blood count (Dr. Dorner, January 8, 1912) gives the following figures: Red cells, 3,922,000; hæmoglobin, 65 per cent.; white cells, 1,975 per cubic millimetre of blood. Differential count of white cells: Polymorphonuclear neutrophiles, 68·3 per cent.; small lymphocytes, 14·7 per cent.; large lymphocytes, 4·6 per cent.; eosinophiles, 5·7 per cent.; transitionals, 6·7 per cent. Two normoblasts were seen during the count of 300 white cells.

DISCUSSION.

Dr. PARKES WEBER added that he wished to call attention to the persistent leucopenia associated with the chronic splenomegaly. Leucopenia was not rarely found associated with chronic enlargement of the spleen, no matter whether the enlargement were due to malaria or syphilis, or occurred in cases described under the heading "Splenic Anæmia," or Banti's disease. He had also seen it recorded in a case of splenomegaly due to primary tuberculosis of the spleen. He likewise wished to draw attention to the sudden occurrence of the painful abdominal symptoms, which led to operative interference in the present case. He believed that attacks of abdominal pain (a kind of what might be termed "abdominal crises") not rarely complicated the various morbid conditions associated with chronic splenomegaly.

Dr. POYNTON asked if there was any history of jaundice in the case. Some cases of family cholæmia showed only an enlarged spleen, and no definite blood change. It might, he supposed, possibly be leukæmia. There was at present a case in University College Hospital very much like this, with an enlarged spleen and no characteristic blood change. There was a slight increase in the differential count of the lymphocytes, and the lymphocytes were now tending to increase. That case was also a puzzle.

Dr. PARKES WEBER, in reply, said that he did not think the case could be regarded as one of leukæmia. The patient had been under observation since October, 1910, and if it had been a case of leukæmia the blood examinations would have probably given some indication by this time.

Three Cases of Alcohol Injection of the Gasserian Ganglion for Trigeminal Neuralgia.

By WILFRED HARRIS, M.D.

Case I.—T. A., a man, aged 44, began to suffer three years ago from paroxysmal darting pains in the right cheek and side of nose, brought on by any movements of the face such as eating or talking. After suffering daily for eighteen months, the right infra-orbital nerve was divided on the cheek by a surgeon, giving him complete relief for three months. Then, in July, 1910, suddenly, severe pain seized him along the right lower jaw into the ear, the paroxysms continuing every half hour or less during the daytime until he was admitted under my care at St. Mary's Hospital on November 24, 1910. There was then no trace of anæsthesia remaining on the cheek. He has never suffered from toothache, but had

two teeth removed for the neuralgia without benefit before the infra-orbital nerve was cut. On November 27, thirteen and half months ago, after freezing the skin with ethyl chloride spray, I injected the right foramen ovale first with 6 minims of 2 per cent. eucaine, followed after thirty seconds with 1½ c.c. of 90 per cent. alcohol. Using a 2½-in. needle, the lips of the foramen were felt by the point of the needle, and after injecting 1 c.c. of alcohol, I worked the point of the needle gently through the foramen for another ⅛ in., or a total depth of 2 in. from the surface, and then injected another ½ c.c. of the alcohol. As this was being done he said he felt some pain around the eye and in the forehead, but this died away in a few seconds. He says now that he felt scarcely any pain at all during the whole process. Immediately afterwards, on testing, I found complete anæsthesia to touch, pinprick, and even to pressure over the whole right fifth nerve area, with complete loss of taste on the right side of the tongue. Next day he could feel ordinary touches on the forehead and cheek, though still completely insensitive to pinprick on that side. Now, after thirteen and a half months, he has never had the slightest recurrence of pain, and he is still as insensitive to pinprick over the right fifth nerve area as he was a few days after the injection, and light touch is also lost on that area. Taste has returned on the right side, though somewhat diminished.

Case II.—Mrs. G., aged 41. I injected the left Gasserian ganglion twice, in October last, with ten days' interval, using nearly 2 c.c. of 90 per cent. alcohol on each occasion. She had suffered from severe tic douloureux of the left second and third divisions for five years. Two years before she came to me she had obtained relief from a course of numerous injections of alcohol by another practitioner, for which she was put under general anæsthesia six different times. As a result of that treatment she had diplopia, which lasted for nearly three months, and paralysis of the pupil, though the pain was completely relieved for nineteen months, when in July last it returned furiously. When I saw her there was no trace of anæsthesia remaining, though she says she was quite anæsthetic on both lips and jaws on the left side after the treatment. The left pupil was typically Argyll-Robertson in character, there being no trace of reaction to light, but a slow, full reaction to accommodation. On October 16 last I injected the third division at the foramen ovale, local anæsthesia only of ethyl chloride spray and eucaine being used. Using the method I have already described above, I injected the nerve at the foramen ovale, and also the Gasserian ganglion,

using altogether 2 c.c. of 90 per cent. alcohol. The immediate result was complete anæsthesia on the lower lip and tongue and the rest of the third division, with complete loss of taste on the front of the tongue on that side, with deep analgesia and loss of light touch over the remainder of the fifth nerve supplied by the second and first divisions. Although the conjunctival reflex was partially lost, no keratitis followed, but pain of a darting character still troubled her in the lower jaw and cheek and also in the temple, where she had not had it before. Ten days later I therefore injected again into the Gasserian ganglion through the left foramen ovale, passing the needle to a depth of just 2 in. I used no eucaine this time, but injected 2 c.c. of 90 per cent. alcohol slowly into the ganglion. When finishing the injection she became somewhat faint and retched a little, and she was kept in bed for the next two days, slight giddiness and retching continuing for that time. Total anæsthesia to all forms of sensation, touch, pinprick and pressure, was immediately produced over the whole area of the fifth nerve, including the eyeball, auditory meatus, and to a less extent the membrana tympani. There was complete loss of taste on the front of the tongue on that side, as before, and now also loss of taste on the left half of the front of the palate. During the next fortnight she complained of darting neuralgic pains in the left cheek and temple, which gradually diminished and then disappeared entirely, the anæsthesia remaining as intense as before. Keratitis developed in the anæsthetic eye on the fifteenth day after the second injection, necessitating sewing together of the lids for a time, but she is completely cured of her neuralgia, and I think is likely to remain so permanently.

Case III.—J. B., a man, aged 64, has suffered for the past eight years daily intermittent paroxysms of pain in the right lower jaw. The pain shoots up into the cheek, right side of the nose and upper lip, and round the right eye, in the depths of the ear and behind the ear, shooting into the upper part of the neck and below the angle of the right lower jaw. The pain prevents him from eating and talking, and the spasms may last several minutes, and even for hours without cessation; he has not had a day free from pain in the last eight years. Physical examination negative, except for a large crop of small whitish ulcers on the inside of the right cheek. This morning, January 12, I injected the right Gasserian ganglion through the foramen ovale, using 5 minims of 2 per cent. eucaine, followed by 2 c.c. 90 per cent. alcohol, after using ethyl chloride spray to freeze the skin. A $2\frac{1}{2}$-in. needle of 1·1-mm.

calibre was passed through the lower border of the sigmoid notch, on the incisura-ala-nasi line, and struck the inferior maxillary nerve at the foramen ovale at a depth of 1¾ in., the cheek being hollowed and thin: 1 c.c. of alcohol injected at this point caused complete anæsthesia of the whole distribution of the inferior maxillary nerve, though the patient at no time complained of any sensations or pain referred to the lower lip or chin; the needle was then passed another ¼ in. deeper through the foramen ovale into the ganglion, and 1 c.c. more of 90 per cent. alcohol slowly injected. Anæsthesia rapidly spread over the cheek and forehead and top of the head on the right side, and at the conclusion of the injection there was complete loss to touch, pinprick, and pressure over the whole right fifth area. After ten minutes tactile sensation had almost completely returned over the first and second divisions of the nerve, though still insensitive to pinprick. After the lapse of eight and a half hours, when shown at the meeting there was still some analgesia on the first and second divisions of the right fifth nerve, and complete anæsthesia to all forms of sensation over the whole area of the third division, and he is unable to recognize taste on the right half of the tongue. He has had no pain of any kind since the injection, and feels perfectly well.

DISCUSSION.

Dr. HARRIS added that the reason he showed the cases was that he thought that by injecting the Gasserian ganglion itself with alcohol there was a very good chance of procuring permanent relief of the neuralgia, instead of having to renew the injection of the nerve-trunks every year or two years. The three cases illustrated three stages after injection of the ganglion. Case I had now been perfectly well for thirteen and a half months, and the analgesia on all three divisions of the fifth nerve was still precisely the same as it was the week after the injection. This augured well for permanence of cure of the neuralgia, or at least for several years. In Case II much more complete destruction of the ganglion had been produced, causing lasting and complete anæsthesia of the whole of the left fifth nerve; indeed, keratitis had developed in the anæsthetic eye, but that was now quite cured. The third case he brought to show the effects a few hours after the injection into the ganglion had been done, and that the man was perfectly comfortable, and as well as ever.

Dr. JAMES GALLOWAY remarked on the case of severe neuralgia associated with the presence of cutaneous myomata in the distribution of the fifth nerve which he had brought before the Society at a previous meeting.[1] He hoped that Dr. Harris would be kind enough to treat this case by means of alcoholic injections into the course of a branch or branches of the fifth nerve, and that

[1] *Proceedings*, p, 7.

the patient might be presented later to the Section. He would be glad if Dr. Harris could state the reasons for hoping for a favourable result in such a case as this, in which the pain originated in the extreme peripheral distribution of the nerve, due either to the presence of the cutaneous tumours themselves or to the electrolysis to which at one time they had been subjected.

Dr. HARRIS, in reply, said that in Dr. Galloway's case the pain was certainly peripheral in origin. He believed it was due not to the tumours but to the treatment by electrolysis which the man had some years ago. Alcohol injections of the nerve-trunk or of the ganglion would relieve the pain in that case if the anæsthesia were made deep enough. It was only in cases of peripheral origin that alcohol injections were of use. In herpetic neuralgia, where the trouble was presumably in the ganglion, the injection of the supra-orbital nerve made the man worse. A year or two ago he showed, at the Neurological Section, a patient with sclerosis of the spinal root of the fifth nerve; he had tic douloureux in the analgesic fifth, and he had crossed analgesia. The man was sent to him for treatment, and as he was not then certain that in cases of central origin alcohol injections were of no use, the treatment was tried, but failed. He was absolutely convinced that in trigeminal neuralgia the pain was of peripheral origin, not central and not ganglionic.

A Case of Enterogenous Cyanosis.

By HECTOR CHARLES CAMERON, M.D.

M. A. B., FEMALE, aged 36. Health began to fail indefinitely seven years ago. About three years ago cyanosis was first noticed and has been present ever since, varying in degree. She complains of weakness, dyspnœa on exertion, and constipation. After the birth of four healthy children she has had seven miscarriages at short intervals.

There is no abnormality of heart and lungs. The cyanosis shows most plainly in the face, lips, mouth, feet and hands. There is no appearance of pallor associated with the cyanosis. The fingers are not clubbed. The tongue has been coated and the breath foul. There is much pyorrhœa alveolaris with spongy, hypertrophied gums, from which a growth of streptococci has been cultivated. A blood count shows 5,350,000 red cells, 9,375 white cells, and 90 per cent. hæmoglobin. Spleen is not enlarged. Spectroscopic examination of the blood shows no abnormal pigment. The patient denies having taken any drugs except those prescribed by her doctors. The patient was under Dr. Pitt's care in Guy's Hospital twelve months ago, when spectroscopic examination was also negative.

DISCUSSION.

Dr. CAMERON added that the cyanosis had considerably diminished. He saw the case first a fortnight ago. She had consulted Dr. Stevens because of the history of seven miscarriages at short intervals in the past few years. When Dr. Stevens sent her to him, he was struck by the extraordinary hypertrophy and septic condition of her gums. She had made steady improvement without any treatment. If the case was not one of enterogenous cyanosis, it was difficult to assign another cause. The examination of her blood was made both times in the winter; cold had a great effect upon the degree of cyanosis, more than in a case of methæmoglobinæmia or sulphhæmoglobinæmia. He had not yet received the results of the blood culture.

Dr. PARKES WEBER said the case corresponded with those which had been described, chiefly on the Continent, as acro-cyanosis. Such conditions occurred mostly in rather young people, and the symptoms were nearly always worse in cold weather. Any kind of toxæmia from the alimentary canal (colitis, &c.) seemed to favour the development of conditions of acro-cyanosis. In this case there was doubtless the possibility of a toxæmic condition arising from the pyorrhœa alveolaris. Mental trouble might also make the condition worse. Some years ago, during the winter months (January and February, 1906), he saw, at Mount Vernon Hospital, a young woman, aged 20, who had swollen, red, moist hands; her cheeks had the appearance of tense, shiny, somewhat livid red pads. Two years afterwards Dr. Weber saw her again (likewise during winter) without anything of the former swollen, chilblainy look about her hands and face. She had been married in the meantime, and there might possibly have been a mental factor in the case. Acro-cyanosis and vasomotor neuroses were sometimes associated in young women with hysterical symptoms, such as hysterical anæsthesia or hysterical tremors.

Pulmonary Stenosis in a Woman, aged 34.

By H. E. SYMES-THOMPSON, M.D.

J. S., FEMALE, aged 34, married, enjoys good health on the whole, and has had five children, including twins. She has noticed undue shortness of breath on exertion as long as she can remember, and she complains of a certain amount of dyspepsia and constipation from time to time. Over the base of the heart there is a loud systolic murmur and a systolic thrill, which are most marked over the pulmonary area. The cardiac dullness is normal. There is no clubbing of the fingers, cyanosis, or

anæmia, and no history of rheumatic fever, scarlet fever, or diphtheria. She has 6,100,000 red corpuscles per cubic millimetre, with a hæmoglobin value of 90 per cent., and a colour index of 0·73.

DISCUSSION.

Dr. SYMES-THOMPSON added that the patient's age, the markedly abnormal physical signs, and the fact that there was no definite enlargement of the heart, were points of interest. If his diagnosis was correct, she must also have a communication between the two ventricles, and perhaps between the two auricles also to act as safety valves; otherwise the heart would be much enlarged. The fact that she was able to lead the life of an active woman was remarkable.

Dr. PARKES WEBER agreed that the murmur corresponded to the murmur associated with pulmonary stenosis, but there was also the possibility that it might be due to patent ductus arteriosus, although its point of maximum intensity was close to the sternum. The murmur in cases of patent ductus arteriosus did not always extend through the whole cardiac cycle, as it did in a case described by Dr. G. A. Gibson, of Edinburgh, in which the presence of a patent ductus arteriosus was afterwards proved by post-mortem examination. Sometimes the murmur was only systolic; sometimes there was no murmur at all. He knew also of a case or cases where the post-mortem examination showed absence of patent ductus arteriosus, but in which during life a continuous murmur extending through the whole cardiac cycle had been heard,[1] a murmur not due to any opening in the interventricular septum.

Dr. SYMES-THOMPSON, in reply, said that there was a very marked thrill, such as he was not aware occurred in other congenital malformations, and that was the chief point which had inclined him to the diagnosis which he had suggested.

Epithelioma of Tongue; Operation; Subcutaneous Recurrences on Back Five Years later.

By C. H. FAGGE, M.S.

G. B., AGED 54, commissionaire, had syphilis as a young man; noticed a sore area on the dorsum of the right half of the tongue in 1892. During subsequent years it disappeared and recurred several times; until 1897 he smoked about 4 oz. of tobacco in a pipe weekly;

[1] *See* references to cases described by Dr. T. R. Whipham, &c., quoted by Dr. F. P. Weber in *Proceedings*, 1911 (Child. Sect.), v. p. 25.

after this he reduced the amount. For two months before admission the tongue was ulcerated; in November, 1905, he sought admission into Guy's Hospital for an indurated, raised ulcer 1 in. in diameter on the right half of the tongue; the left half of the dorsum showed chronic superficial glossitis; there were no palpably enlarged submaxillary or cervical lymph glands. The right half of the tongue with the right submaxillary salivary and lymphatic glands were removed and patient was not seen again until December, 1911, when he complained of two hard, fixed masses on his back: one over the upper left ribs near the spine, and the other, the larger, on the right side; part of the latter was excised under local anæsthesia.

Microscopical sections, brought to the meeting, show that the lingual ulcer was a typical squamous epithelioma; the lymph glands showed no secondary deposits; there was some difference of opinion as to the nature of the other sections from the subcutaneous mass, and Mr. Targett has been kind enough to confirm my opinion that it is a secondary epitheliomatous deposit, and has drawn attention to the extensive necrotic areas.

Mr. FAGGE said that he had regarded the lumps on the back as probably gummata, but as others thought they were secondary deposits, he had a portion removed under local anæsthesia. The patient was now having iodide of potassium, and was to undergo X-ray treatment; he would be glad to hear the opinion of any member as to the nature of the slides from the subcutaneous tumour.

Supernumerary Mamma in a Male.

By CYRIL A. NITCH, M.S.

FIRST noticed by patient ten years ago. The breast is situated on the posterior fold of the axilla and the postero-internal surface of the upper arm on the right side. It is well formed and possesses a well-shaped nipple. The organ does not secrete any fluid.

The Relative Value of Immediate and Delayed Laparotomy in Pneumococcal Peritonitis.

By HECTOR CHARLES CAMERON, M.D.

I HAVE collected the cases of pneumococcal peritonitis admitted into Guy's Hospital during the last nine years in order to attempt to estimate the effect of immediate laparotomy in this condition. The majority of cases have occurred during the last five years; in the earlier years many cases have had to be omitted because of the absence of bacteriological evidence. In one case included in the series bacteriological examination gave only a growth of *Bacillus coli communis*, but for reasons given later the peritonitis was almost certainly pneumococcal. In one other case no bacteriological examination was made.

I do not propose to occupy time in describing minutely the symptoms of pneumococcal peritonitis, yet, since I am about to propose that in certain cases it may be better for the patient to withhold immediate laparotomy, I may perhaps be allowed to refer briefly to the symptoms and signs which seem to me of greatest service in making a diagnosis.

The age and sex of twenty-six cases collected were as follows:—

Nineteen females		Seven males	
Age	No.	Age	No.
27 years ...	1 case	22 years ...	1 case
20 ,, ...	1 ,,	18 ,, ...	1 ,,
15 ,, ...	2 cases	16 ,, ...	1 ,,
14 ,, ...	1 case	10 ,, ...	1 ,,
11 ,, ...	1 ,,	4 ,, ...	1 ,,
9 ,, ...	1 ,,	3 ,, ...	1 ,,
8 ,, ...	5 cases	2 weeks ...	1 ,,
6 ,, ...	3 ,,		
5 ,, ...	2 ,,		
3 ,, ...	1 case		
2 ,, ...	1 ,,		

Fifteen out of nineteen cases in females occurred in little girls between the ages of 5 and 15. At such an age the only common forms of peritonitis, besides that due to pneumococcal infection, are peritonitis of appendicular origin and peritonitis due to gonococcal infection. The latter can of course readily be excluded by an examination of vulva and

vagina. Peritonitis due to infection of the urinary or biliary passages, to suppuration in a mesenteric or retroperitoneal gland, to perforation in typhoid fever, or to perforation of a gastric or duodenal ulcer, is so rare that it can be excluded from consideration.

The differential diagnosis between peritonitis due to appendicitis and peritonitis due to pneumococcal infection depends upon the evidence, in the latter condition, of the presence of a pneumococcal septicæmia. This shows itself in the feeling which we often have when we stand by the side of a case of pneumococcal peritonitis, that the patient is, in general, more ill than can be accounted for by the degree of peritonitis present. Often there is present evidence of infection of other parts, either antecedent or simultaneous. In thirteen of the present cases it was possible to say with certainty that there were other pneumococcal infections, such as pleurisy, pneumonia, or pericarditis, present at the same time. Of these twelve died. One case which certainly had lobar pneumonia as well recovered. Even where there is no evidence of involvement of lung or pleura, the aspect of the patient is sometimes that familiar in lobar pneumonia, with grunting respiration and alæ nasi working vigorously. In five cases herpes labialis was present; in others the onset was with rigors, shivering, or convulsions. Where a leucocyte count was done a high leucocytosis was present. In several of the cases it is noted that, comparatively soon after the onset of peritonitis, the signs of free fluid in the peritoneal cavity were present. An early and copious exudation of lymph is characteristic of this form of peritonitis. In Case II opportunity was afforded for timing the rapidity with which fluid accumulates. At the operation there was no free fluid in the peritoneal cavity; after death, thirty hours later, the peritoneal cavity was completely filled with turbid serum. In Cases I and II, which happened to be seen within a few hours of the onset of the peritonitis, the temperature was over 104° F. Such a height of temperature must be very rare at the onset of appendicitis, or of perforative peritonitis.

I wish, however, especially to draw attention to the existence, in the last four cases of pneumococcal peritonitis which I have seen, of well-marked follicular ulceration of the colon. In three of these cases—Cases I, II and III—there was evidence, in the history, of the existence of this colitis for some days, and in one case for three weeks, before the involvement of the peritoneum. In Case II the colitis was seen during life at the operation, at a time when the peritoneum had only just become involved.

Case I.—J. S., aged 22, while on holiday in Ireland, developed a chill, with shivering, abdominal pain and diarrhœa. He felt so ill that he came home to London on August 8. He stayed in bed for three days, when on August 12 he walked to hospital, and was seen in the Out-patients' Department, where a diagnosis of appendicitis was made. His temperature when first seen was 104° F., his pulse 120. Mr. Turner operated. On opening the peritoneum some blood-stained serum escaped; the appendix, which was red and inflamed, but not perforated, was removed. Within twelve hours the patient died. At the autopsy I found that the cæcum and the first 6 in. of the ascending colon showed an acute follicular colitis. The wall of the gut was so œdematous as to pit on pressure. The glands by the cæcum were congested, and in one there was a hæmorrhage. There was a little recent peritonitis around the cæcum and stump of the appendix. Cultivation of the heart blood proved sterile; cultivation from the peritoneum gave only a growth of *Bacillus coli communis*. I believe, however, that this case was certainly due to the pneumococcus. It is not unusual to find the pneumococcus overgrown by the *Bacillus coli communis*. Moreover in this case there was a thick yellow layer of lymph over the upper left lobe from which, unfortunately, I did not cultivate.

Case II.—E. S., aged 20, was admitted under Dr. Hale White on November 11, 1911. On Wednesday, November 8, she felt ill, had abdominal pain and diarrhœa. On November 9 she stayed in bed; on November 10 and 11 she went back to work. On the afternoon of November 11 she was suddenly seized with pain, and vomited. On admission, at 10 p.m., her temperature was 104° F. The abdomen moved badly, and the appendicular region was rigid. I saw her at 11 p.m., and got Mr. Hughes to operate. The wall of the cæcum was stiff and rigid, and there were a few flakes of lymph on the peritoneal coat. The appendix was healthy. With Case I fresh in my mind, I recognized the condition as one of colitis. The peritoneum had just begun to be affected. She died thirty hours later, the temperature continuing high till the end. At the autopsy there was acute follicular ulceration of the cæcum and colon, just spreading to the base of the appendix. In thirty hours the peritoneal cavity had completely filled with thick purulent lymph. A pure growth of pneumococcus was obtained on cultivation from the lymph.

Case III.—The next evening I saw L. F., aged 14, admitted on November 12 under the care of Dr. Shaw. For three weeks she had had severe intermittent colicky pains, and had felt ill, and often shivered. On November 12, after dinner, she had a very severe attack of pain, and fainted. She was brought to hospital. I saw her soon after admission. The abdomen was rigid and tender. She was very dyspnœic, but without signs of consolidation of the lungs. She was extremely ill, and almost comatose. I decided not to operate, although I diagnosed pneumococcal septicæmia, peritonitis, and colitis. After consultation with Dr. Shaw, the abdomen was opened the next afternoon. The bowel wall felt thick; a little free blood-stained fluid in the peritoneal cavity, from which the pneumococcus was cultivated. On November 17 signs of consolidation of

the lungs appeared. On November 18 she died. At the autopsy broncho-pneumonia and diffuse pneumococcal peritonitis were found. Recent vegetations on aortic valve. The cæcum and ileum in its lower part acutely inflamed. The ileum had actually given way, and there was a perforation which Dr. French, who did the post-mortem, thought had occurred during life. I can find no other record of such an accident.

From a consideration of these points, the age and sex, the onset with rigors, convulsions, or herpes labialis, the early appearance of delirium, or of pronounced diarrhœa, the simultaneous presence of pleurisy, pericarditis or pneumonia, the evidence of antecedent colitis, the great and rapid exudation of fluid into the peritoneal cavity, the high temperature at the onset, the marked leucocytosis—it is often possible to make a diagnosis with certainty.

In pneumococcal peritonitis it is possible to recognize three stages:—

(1) A stage of onset, in which all the symptoms set in with great violence, and in which, in many cases, death occurs. Seen at this stage the diagnosis is to be made from perforative peritonitis or from acute gastro-enteritis. In many cases there is truly an enteritis present as well as the septicæmia, but the leucocytosis and the height of the temperature should serve to distinguish the double condition from uncomplicated gastro-enteritis.

(2) If the patient survives the onset, after some hours or days, there is usually considerable improvement in the general condition of the patient, and a retrogression of all symptoms. The pyrexia usually continues for some two or three weeks, during which time a diagnosis of typhoid fever may be suggested. The greater intensity of the abdominal pain, the persistent vomiting, the high leucocytosis, and the presence, in some cases, of evidence of free fluid in the abdominal cavity, should prevent this mistake.

(3) Lastly, after recovery from the pneumococcal septicæmia, a residual collection of pus—often subdiaphragmatic, sometimes, however, filling the whole peritoneal cavity—is usually left behind, just as an empyema may complicate convalescence from lobar pneumonia. Such a condition, seen for the first time, is likely to be mistaken for a case of tuberculous peritonitis with local or general effusion. Moreover, the pneumococcal abscess tends, like that due to tuberculous peritonitis, to point near the umbilicus. It would seem, however, that when such an umbilical fistula appears after pneumococcal peritonitis, it may be formed with much greater rapidity than is common in tuberculous peritonitis.

I have notes of four cases who were admitted after recovery from pneumococcal septicæmia with quiescent residual collections of pus, and who made complete recovery after evacuation of the pus.

Case IV.—C. D., female, aged 27, was admitted to Clinical Ward, November, 1907. Three weeks before had had an acute illness with vomiting, diarrhœa, herpes labialis, and abdominal pain. When she was "almost convalescent" her abdomen began to swell. On admission there were signs of free fluid in the abdomen and dullness over the bases of both lungs. Exploration of the left chest revealed pus. A rib was removed, and many pints of pus escaped. The abdomen rapidly diminished in size, and pressure on it increased the flow through the drainage-tube. A subsequent laparotomy showed that the drainage tube had passed through a hole in the diaphragm. The peritoneal cavity contained much pus, which gave a pure growth of the pneumococcus. Recovery followed, and she remained well eleven months later when seen by Dr. Hale White. (Reported by Dr. Hale White in an address published in the *British Medical Journal*, December 12, 1908.)

Case V.—W. J. T., male, aged 10, was admitted into Dr. Taylor's wards in September, 1906. On August 24 he had been taken ill with pain and swelling of abdomen, vomiting, and diarrhœa. Until admission he had appeared to improve, but had remained languid and sleepy. The abdomen was held tight, moved badly, and the left flank was dull. Tuberculous peritonitis was diagnosed. He was discharged, much improved, on November 15, 1906. Seven months later, June, 1907, he was readmitted under Dr. Hale White's care. Since discharge he had often had attacks of pain, and had wasted. Three weeks before a swelling at the umbilicus had burst. Again a diagnosis of tuberculous peritonitis was suggested. Opsonic index to tubercle 0·83. Tuberculin treatment instituted, without improvement. As the pus was free from fæcal odour, a cultivation was made, and a pure growth of pneumococcus obtained. A vaccine was prepared and administered, and he went out on February 3, 1908. The sinus had almost healed.

Case VI.—D. S., female, aged 5, was admitted to Clinical Ward, April 5, 1911. Six weeks before had had an acute illness. At first appendicitis was diagnosed, later pneumonia. After the first week the abdominal pain disappeared, but the child remained without appetite. Before admission it was recognized that there was free fluid in the abdominal cavity. A diagnosis of tuberculous peritonitis was made, and the child was kept out of doors. Opsonic index to tubercle 1·2. On April 30 a swelling appeared at the umbilicus, and became so prominent that it was decided to operate. As soon as the peritoneum was opened pus poured out; three pints were collected. A pure growth of pneumococcus was obtained. Recovery followed, and the child was discharged, well, on July 8.

Case VII.—No cultivation was made in the following case, to which Dr. Taylor, under whose care it was, has called my attention. Dr. Taylor has

since regarded it as a case of pneumococcal peritonitis. W. A., female, aged 8, admitted July 9, 1903, under Dr. Taylor. On April 20 she suddenly had abdominal pain. An acute illness of many weeks' duration followed, which was diagnosed as typhoid fever. In the fourth week she was still ill, and never got well. On July 7, Dr. Taylor saw the child, and had her admitted. The abdomen was swollen and contained fluid. On the day before admission a fistula formed at the umbilicus. Mr. Lane operated, and 1½ pints of *greenish-yellow* pus escaped. The child recovered, and went out on September 3, 1903. In March, 1905, the child was well.

Such cases must suggest the possibility that recovery may take place without any operation at all. No proof can be given, but I offer the following as such a case of recovery without operation. I saw the child many times when it was under Dr. French's care. It was that of a girl, aged 15 (Case VIII), who was admitted with vomiting and abdominal pain of two days' duration. The abdomen was swollen, rigid, and tender. In both flanks there was dullness, which shifted on rolling the child over. There was herpes labialis. The temperature was high, between 102° F. and 104° F., and the pulse rapid. There was a leucocytosis of 15,600. The abdominal pain and vomiting ceased after four days, and the child made a good recovery. Dr. French diagnosed pneumococcal peritonitis. Cultivation from the herpetic vesicles gave a growth of *Staphylococcus albus*. Such recovery, either with or without residual abscess, must be regarded as a rare and fortunate chance. If immediate laparotomy can claim to save even a small percentage of cases, there can be little doubt that it should be always advised.

I find that eight cases (IX, X, XI, XII, XIII, XIV, XV and XVI) admitted in the acute stage were not operated upon before death. In seven of these pleurisy, pneumonia, pericarditis or endocarditis, were present as well as peritonitis. The eighth case died as soon as admitted. Twelve cases (I, II, III, XVII, XVIII, XIX, XX, XXI, XXII, XXIII, XXIV and XXV), were submitted to immediate laparotomy. Nine of these died: four on the day following operation, the remainder within a few days. Three cases recovered, but in all three the laparotomy performed at the onset of symptoms failed to produce immediate improvement. The patients passed through a long and critical illness, developed during convalescence the signs of abdominal abscess, and recovered after the evacuation of the pus. No case recovered without the formation of residual abscesses and without a second operation being required.

Two large series of cases of pneumococcal peritonitis have been

published recently in this country, but in neither is the effect of immediate laparotomy considered in detail. Rischbieth, in the *Quarterly Journal of Medicine* for January, 1911,[1] records the cases from Great Ormond Street Children's Hospital. In forty-five cases in which the peritonitis did not become localized the mortality was 100 per cent. Of nine cases seen with residual local collections of pus, six recovered. Annand and Bowen, in 1906,[2] collected from the literature forty-one cases of local abscesses, of which thirty-seven recovered and six died after operation; of forty-six cases in which the peritonitis was diffuse, six recovered and forty died.

It would appear that those cases of pneumococcal peritonitis which recover are those which pass successfully through the pneumococcal septicæmia. After the termination of the acute septicæmia only a minority of cases die as a result of the failure to secure drainage of the residual collections of pus. This was, however, the cause of death in the last case, to which I should like to refer (Case XXVI), that of a little girl, aged 11, who was admitted on October 10, 1911, with pneumococcal peritonitis, so ill that operation was delayed. She was almost comatose, with incontinence of urine and fæces. She improved greatly for three weeks, when a left subdiaphragmatic abscess was opened. Three further operations had to be performed to drain abscesses. After the last of these, unfortunately, a fæcal fistula formed, and the child gradually sank, and died on December 20. For many weeks it seemed likely that she would recover. That she would have died at once if operated upon on the day of admission is, I think, certain. This is the only case I can find in which the policy of waiting was purposely adopted, but I think that it is likely that in certain cases it is the right course to pursue. I can find no evidence that establishing drainage by laparotomy, at the onset of the disease, increases the chance of recovery from the septicæmia, or helps to cut short the process in the peritoneal cavity. On the other hand, I think that there are cases so ill at the onset of the disease that laparotomy may turn the scale against recovery, and I suggest that in these cases it may be wiser to wait until the septicæmia is at an end, and until the disease has become localized in the peritoneum, just as is done in dealing with the empyema which follows upon pneumonia. At the same time, I am fully conscious of the complexity of the problem, and of the small help which statistics such as mine can give in deciding the wisest course of

[1] *Quart. Journ. Med.*, Oxford, 1911, iv, pp. 205-31.
[2] *Lancet*, 1906, i, p. 1591.

action in any individual case. It is in the hope that I may learn from the experience of others something which may in the future guide me in dealing with this intensely fatal disease that has induced me to record these cases.

In cases seen at the onset I should recommend that the patient should be made to sit upright in bed, that ice or heat should be applied to the abdomen, that morphia should be given, and that the intense toxæmia should be combated by subcutaneous saline infusion.

Finally, I have to acknowledge my indebtedness to the physicians and surgeons of Guy's Hospital, who have given me leave to publish the cases, and especially to Dr. Taylor and Dr. Hale White, under whose care a large number of the cases were admitted.

Cases seen during Acute Stage and not Operated on.

Case IX.—M. A. S., female, aged 8, complained of abdominal pain on the night of May 31, 1911. She was admitted to Mary Ward, under Dr. Shaw, on June 1. The child was very ill, with profuse vomiting. The abdomen was tender and a little rigid. A diagnosis of pneumococcal peritonitis was made, but the surgeon called in hesitated to operate. On the next day the aspect was distinctly that of pneumonia. The respiration-rate was 60. The breathing grunting, with much dyspnœa. There was dullness at both bases. A diagnosis of pneumonia was made. On June 4 the child died. There was very extensive pneumococcal pleurisy over both lungs, with much yellow lymph. There was no pneumonia. The last few inches of the ileum with the appendix were acutely inflamed and covered on the peritoneal aspect with lymph. The lymphatic glands were much enlarged. No note was made of the condition of the mucous membrane.

Case X.—W. S., male, aged 14 days, was admitted, moribund, on December 12, 1906. The illness began on December 8 with vomiting, constipation and dyspnœa. On admission there were signs of bronchopneumonia. He died on the day of admission. Post mortem, bronchopneumonia and general suppurative peritonitis due to the pneumococcus was found.

Case XI.—D. E., female, aged 8, had abdominal pain and vomited on January 22, 1906. On January 22 she was dyspnœic, and herpes appeared on the lip. On January 26 she was admitted to Clinical Ward, moribund. At the autopsy there were confluent bronchopneumonia, pleurisy, early pericarditis, and acute purulent peritonitis. The culture from the peritoneum showed only *Bacillus coli communis*.

Case XII.—G. C., male, aged 3, became ill on December 20, 1905, with dyspnœa, diarrrhœa and vomiting. On admission on December 23 he was

drowsy, with a temperature of 104° F. There were signs of bronchitis. The Widal test was negative. He died on December 28. At the autopsy there was recent pleurisy, with pneumococcal peritonitis, with much recent lymph over the cæcum.

Case XIII.—A. B., female, aged 18, but with the aspect and development of a child aged 12, was seized with abdominal pain, diarrhœa and vomiting on April 3, 1904. She was admitted on April 5, with a diagnosis of gastro-enteritis, but died two hours later. General pneumococcal peritonitis was present. The appendix was inflamed, red and swollen, but not perforated or gangrenous. Its peritoneal coat was covered with lymph.

Case XIV.—E. D., female, aged 2, had severe abdominal pain and vomiting on May 29, 1905. She was admitted on May 31, 1905, with signs of confluent bronchopneumonia with tenderness of the abdomen. She died on June 2, when bronchopneumonia and general pneumococcal peritonitis were found.

Case XV.—W. J., male, aged 18, was admitted into a surgical ward on February 28, 1903. About January 22 he had had "influenza." On February 1 vomiting and severe headache began and a discharge from the ear was noticed. He was admitted on February 2 and Mr. Steward explored the mastoid region with negative result. On February 5 optic neuritis was noticed. On February 25 he was delirious and a pericardial rub appeared. On February 26 an empyema was evacuated from the right chest. He died on February 27, when pleurisy, pericarditis, and peritonitis were found, all due to the pneumococcus.

Case XVI.—J. N., male, aged 16, was admitted on July 10, 1903, with lobar pneumonia. He continued very ill and delirious till July 21, when he died. During the last few days he made complaint of abdominal pain. At the autopsy, double pleurisy, lobar pneumonia, and acute peritonitis, all due to the pneumococcus, were found.

Cases seen during Acute Stage and Operated on.

Case XVII.—H. S., male, aged 4, was admitted on April 19, 1907, with a tender and rigid abdomen. He had had abdominal pain for two days. On laparotomy the appendix was swollen and hæmorrhagic, but not perforated. It was removed. There was general peritonitis due to the pneumococcus. Death followed on the next day. Post-mortem showed no cause for the peritonitis.

Case XVIII.—M. M., female, aged 6, was admitted on November 18, 1910. Taken ill with diarrhœa and vomiting on November 15. The abdomen was rigid and tender. Laparotomy was performed on admission; general peritonitis without obvious cause. The pneumococcus was obtained on cultivation.

The temperature curve and pulse-rate were unaffected by the operation. Before death, on November 21, the respiration-rate rose to 60 per minute. General peritonitis, with pericarditis and bronchopneumonia, were found post mortem.

Case XIX.—R. J., female, aged 3, had abdominal pain, with vomiting and diarrhœa, on May 1, 1910. On May 5 she was admitted moribund, with a distended, rigid abdomen. There was a rapid respiration-rate. The abdomen was opened on May 6 and diffuse pneumococcal peritonitis was found. She died the next day.

Case XX.—F. A., female, aged 6, was taken ill on April 13, with abdominal pain and dyspnœa. The abdomen was tender, immobile and rigid. Laparotomy was performed the same day and general peritonitis found. The pneumococcus was cultivated from the pus. Death occurred the next day. The autopsy showed extensive exudation of lymph and great engorgement of the abdominal lymphatic glands.

Case XXI.—B. A., female, aged 8, was admitted on July 18, 1908, with abdominal pain and vomiting of four days' duration. The respirations were very rapid. The abdominal wall was rigid and tender. A laparotomy was performed. The appendix was normal. There was much purulent fluid, which gave a growth of pneumococcus on cultivation. Death occurred on July 20. The autopsy showed general pneumococcal peritonitis.

Case XXII.—G. G., female, aged 9, was admitted on December 21, 1908, with a tender, rigid abdomen and great abdominal pain. There had been diarrhœa, with abdominal pain, for nine days previously. Movable dullness in the flanks was present. A rub was heard over the liver. Mr. Turner operated and found general pneumococcal peritonitis. The appendix was normal. Death occurred on the next day. No post-mortem was allowed.

Case XXIII.—D. K., female, aged 5, was admitted on December 8, 1909. On December 3 she had had abdominal pain, vomiting and constipation, which had continued since with high fever. On admission the abdomen was rigid, tender, and immovable. Temperature, 101° F.; pulse, 140; respiration, 40. Immediate operation was performed, and general peritonitis was found, for which no cause could be discovered. Cultivation showed a pure growth of pneumococcus. The operation had no immediate effect upon the curve of the temperature chart or upon the pulse, but the child gradually improved. For four weeks the appearance of the chart was unaltered; thereafter the temperature became irregular. On February 19 a right subdiaphragmatic abscess was opened and drained; thereafter improvement was rapid, and the child made a good recovery.

Case XXIV.—E. W., female, aged 6, was admitted on July 27, 1910. She had been taken ill on July 21 with headache and feverishness. On July 22 first had abdominal pain; this had continued since. On admission there

was distension of the abdomen, with rigidity and tenderness. Herpes labialis was present. Dr. Fawcett diagnosed pneumococcal peritonitis. Laparatomy showed general peritonitis, cultivation of which was at a first attempt sterile. A second specimen, on cultivation, showed a pure growth of pneumococcus. On August 9 a residual abscess was opened, and the temperature thereafter kept down. Unfortunately the temperature chart has been lost.

Case XXV.—A. B., female, aged 8, was admitted to Clinical Ward on November 18, 1904. On November 10 there had been sudden abdominal pain and a rigor. On November 11 the temperature was 104° F., and a doctor diagnosed pneumonia. On November 12 herpes appeared about the mouth. On November 18 she was removed to Guy's Hospital. Dr. Taylor diagnosed double basal pneumonia and thought the abdominal condition due to pneumococcal peritonitis. Laparotomy was performed and general peritonitis due to the pneumococcus found. On December 1, 20 oz. of pus removed from the left pleural cavity. December 18, a collection of pus opened and drained in the left iliac fossa. February 21, discharge. (Reported by Dr. Taylor in the *Transactions of the Clinical Society*, 1904, xxxvii, p. 91.)

Case of Delayed Operation.

One case which was admitted in the acute stage, but which was not operated on until after three weeks had elapsed.

Case XXVI.—M. D., female, aged 11, was admitted to Clinical Ward on October 10, 1911. Five or six months before she had had an attack of abdominal pain which was thought to be similar to the present attack. She was then in bed for a week. On October 3 she first felt ill, and since then had been feverish and shivering. On October 10 she had severe abdominal pain and vomiting, and was admitted. On admission the child was very ill, collapsed and almost comatose, with incontinence of fæces. The tongue was dry and glazed. The abdomen was not tender; it moved badly, and was a little rigid in the lower part. The breathing was rapid. Dr. Fawcett and Mr. Steward in consultation decided that the child was too ill for laparotomy. Although the pulse and temperature remained high, the general condition gradually improved and the abdominal condition did not become more obtrusive. There was a leucocytosis of 31,300. About October 25 signs of a subdiaphragmatic collection of pus began to appear on the right side, and pus was evacuated through an abdominal incision from under the right dome of the diaphragm on October 30. This pus gave a growth of pneumococcus in pure culture. On November 5 the wound was re-opened and 10 oz. of pus were removed. On November 25 a further operation was necessitated to drain another collection of pus. Unfortunately a fæcal fistula formed. Death took place from exhaustion on December 20. At the autopsy there was much matting of the intestines; the wall of the colon was inflamed and ulcerated in places.

DISCUSSION.

Dr. GALLOWAY desired in the first place to thank Dr. Cameron for the trouble he had taken in preparing this paper and for the clear way in which he had brought out the salient points in the differential diagnosis of cases of peritonitis which might be pneumococcal in origin. No doubt in recalling their experience many present would recollect cases of mysterious peritonitis in which the severity of the illness and septicæmia were out of proportion to the amount of local disease—cases which eventually had a prolonged course, ending in localized suppuration. Dr. Galloway commented on such a case recently under his own observation, followed by operation, presenting very similar signs and symptoms to some of those related by Dr. Cameron. He thought that such papers as the one they had just heard, presenting points of both medical and surgical importance, were of special interest to members of the Section.

Dr. PARKES WEBER said that Dr. Cameron raised a most important question, for both physicians and surgeons, in regard to pneumococcal peritonitis, namely, whether early operation was to be avoided; but, unfortunately, the great difficulty was to diagnose at once whether a case of peritonitis was of pneumococcal origin. He recently saw a fatal case of the disease in a girl, aged 19, who, after getting up and taking breakfast in the ordinary way was seized with severe pain in the abdomen. When he was consulted, in the middle of the day, the condition somewhat resembled one of so-called "acute abdomen," and a surgeon was called in to see her. The surgeon was in favour of waiting. The temperature on the first day had been up to 105·2° F. Next morning she obviously had peritonitis, and the abdomen was opened, but no signs of appendicitis, gastric or intestinal perforation, pelvic disease, or tuberculosis, were discovered. The peritoneum contained slightly turbid fluid and fibrinous flakes. A practically pure culture of the pneumococcus was obtained from the peritoneal discharge. He did not think early operation was likely to be avoided in such a case. The only fact which might have put one at once on the right track was that some days previously the patient had had tonsillitis, and he heard that a swab from the throat had been taken and examined for the bacillus of diphtheria, but with a negative result. It was not specially examined for the pneumococcus. If the sore throat had been found to be a pneumoccocal sore throat the early recognition of the peritonitis as pneumococcal peritonitis would have been facilitated.

Mr. C. H. FAGGE said that Dr. Cameron, by this paper, had added another to the surgeon's difficulties, which here centred around the questions of diagnosis. He had operated upon five or six cases of pneumococcal peritonitis and all ended fatally, but he could not recall, neither had a cursory search through notes of cases shown him, that in any of those cases there were any of the symptoms which Dr. Cameron regarded as important in diagnosis. He could remember only one case of the kind in which peritonitic symptoms being

present, the physician asked him to see the case; it was decided to be general pneumococcal infection and no operation was undertaken. He believed that the evil done by an operation in cases of pneumococcal peritonitis was less than the converse evil of delaying operation in appendix cases, in which it was important to operate at once.

Dr. JEWESBURY reminded the meeting that one of the cases related in the paper was very successfully treated by a pneumococcal vaccine, and he would like to know if such vaccine was of use only in quite chronic cases, or whether Dr. Cameron had used it with good effect in more acute cases. He also asked whether from the point of view of diagnosis, one would not be justified, in these instances, in aspirating the abdomen, in the same way that one would aspirate a pleural effusion, so as to ascertain whether the pneumococcus was present or not in the fluid withdrawn.

The PRESIDENT said that the paper of Dr. Cameron was of equal value to the medical and the surgical divisions of the profession. He was particularly interested in the case of ulcerative colitis, and how it was determined that it was that condition. Those cases of innominate colitis were not very uncommon: a colitis occurring without known cause might be due to the pneumococcus. In ordinary pneumonia, as Bristowe pointed out many years ago, colitis was not a very uncommon complication. He had himself reported a series of cases of croupous colitis in connexion with this disease. It would be interesting to know how early in these cases the evidence of the septicæmia occurred. That was important in connexion both with diagnosis and prognosis. He would also like to know whether leucocytosis occurred early, because in nearly all the pneumococcic infections the leucocytosis was early and great in degree, whereas in tuberculous conditions there was often no leucocytosis. He did not remember whether there was early leucocytosis in the gonorrheal cases. The question asked by Dr. Jewesbury in regard to vaccines being pushed early and actively was very important, and it would be useful to hear how far that measure was useful as a proper alternative to operation. The paper was of a kind much needed nowadays; it was bacteriological and surgical, and at the same time it had clinical and general aspects.

Dr. CAMERON, in reply, said the only evidence he had that the colitis was pneumococcal was that at the operation which Mr. Hughes did on one of the patients he could feel the stiff cæcum and see the lymph which had just begun to form over the ulcerative patch, and that that lymph was pneumococcal. There was a specimen in the Guy's Hospital Museum, of the year 1850, Specimen No. 912, labelled " Ulcerative colitis in lobar pneumonia," and it was of interest, a propos of what was said, that it was noted that the patient died of early general peritonitis. That colitis might be the determining factor in peritoneal infection had been denied. Dr. Rischbieth, in his collection of cases from Great Ormond Street Hospital, doubted the colitis being preliminary to the peritonitis; he regarded it rather as the result. In several of the cases

mentioned in this paper, diarrhœa and intermittent colic, shivering and pyrexia, had preceded by several days, and even weeks, evidence of spread to the peritoneum. In Dr. Shaw's and Mr. Turner's case there was a three weeks' history of intermittent colicky pain and shivering, presumably also pyrexia, before the spread to the peritoneum. The question might certainly be raised whether such a pneumococcal colitis might not at times form the starting point of certain chronic ulcerative colitis conditions. He had had practically no experience of pneumococcal vaccine. The only case mentioned which was treated by it was Dr. Hale White's case. But he would like to employ that vaccine in these cases if possible. He agreed that the whole question was one of diagnosis. No doubt it was only in a percentage of the cases that it was possible to say with absolute confidence that the condition was pneumococcal peritonitis. But at times one could say so with confidence, and when that did happen he raised the question whether it was not better to delay operation.

Clinical Section.

February 9, 1912.

Sir WM. OSLER, Bt., F.R.S., President of the Section, in the Chair.

Excision of Epithelioma of Lower End of Pharynx.

By F. J. STEWARD, M.S.

MRS. F., aged 40, was admitted into Guy's Hospital on July 3, 1911, with a three months' history of pain and difficulty in swallowing; now, can take fluids only, and these with difficulty.

Endoscopic examination on July 4 revealed a growth involving the anterior and left lateral aspects of the extreme lower portion of the pharynx. Nothing could be felt externally, and there was no evidence of glandular infection.

July 11: Angular incision, transverse limb over thyrohyoid membrane on left side, longitudinal limb downwards along left sternomastoid. Depressor muscles, thyrohyoid membrane, superior cornu of thyroid cartilage, and left lateral wall of pharynx divided. From this point the anæsthetic was administered through a tracheotomy tube passed into the upper opening of the cocainized larynx, the tube being packed round with a gauze strip. The extent of the growth could now be examined with the finger; it was found to be quite small, involving principally the anterior and left lateral walls of the lower end of the pharynx and the upper portion of the œsophagus, and freely movable on larynx and trachea. In order to give further access during the remaining stages of the operation I now removed the left lobe of the thyroid gland as it was slightly enlarged. By tilting the larynx over to the right and drawing the sternomastoid and carotid sheath backwards, the site of the growth was brought well into view. The growth, which was about the size of a sixpenny-piece, was now excised together with a margin of healthy tissue about ¾ in. wide. This involved the removal of the whole

circumference of the œsophagus with the exception of a narrow strip of its right posterior wall, and exposed most of the posterior surface of the cricoid cartilage and the upper three rings of the trachea. Catgut sutures were now used to bring together the thyrohyoid membrane, part of the pharyngeal incision, the muscles, and the transverse limb of the skin incision. A soft rubber feeding tube was then passed from the lateral incision into the œsophagus, fixed in position by a suture, and packed round with gauze.

The patient stood the operation fairly well, but she was rather collapsed, no doubt owing to previous semi-starvation. She made good progress, the wound being kept sweet by daily dressing and syringing with hydrogen peroxide.

August 3: Tube removed, and replaced by one passing through the nose, the wound in the neck now being allowed to granulate. This tube was kept in place for ten days, and as the wound was now nearly closed, the patient was allowed to take food naturally, bougies being passed daily. Trouble now commenced, owing to difficulty experienced in keeping the stricture dilated, and on several occasions anæsthetics were needed in order to pass a bougie. Finally, as the stricture rapidly contracted in spite of all attempts to keep it dilated, and swallowing became extremely difficult, Mr. Fagge, who kindly took charge of the case during my absence in September, re-opened the upper end of the œsophagus through the neck, and passed a tube, through which the patient was fed.

Hoping that the continual swallowing movements would dilate the pharynx above the stricture, the patient was sent home on October 10. She was re-admitted on November 2, and endoscopic examination then proved that considerable dilatation had taken place, and after some little trouble a bougie was forced through the stricture, which was then dilated and a rubber tube passed through it from the nose and left in situ.

The patient bore the inconvenience of the tube for ten days, when it was removed. From this time no further trouble has been experienced, the stricture having been kept dilated by passage of bougies. This was done three times a day at first, and the frequency gradually diminished, so that when she left the hospital on December 10 she was having a No. 18 bougie passed once every two days, and this she has continued to do herself since.

The section of the growth shows it to be a typical epithelioma.

The growth was removed as described, without sacrificing the larynx

and without removal of glands, because it was clearly very recent, and moreover, its free mobility on the underlying structures led me to hope that it was in the primary stage, so that local removal would give a fair chance.

I would call attention to the extreme importance of not closing the wound after operations upon the pharynx. This method of wound treatment, although perhaps less artistic, is much the safer, for in this way the risks of secondary hæmorrhage and septic infection of the air passages are reduced to a minimum.

The plan of administering the anæsthetic through a tube inserted into the cocainized upper opening of the larynx has proved very useful in these cases; it is quite convenient and satisfactory, and, moreover, avoids the risks associated with a tracheotomy opening. There was no sign of œdema of the glottis afterwards, although the mucous membrane was divided close to the arytænoids.

The way in which the stricture was overcome by allowing the lower end of the pharynx to dilate was very striking, the extra amount of mucous membrane so formed being evidently sufficient to bridge the gap caused by the operation, as there has been no further difficulty since the final communication with the œsophagus was made.

DISCUSSION.

Mr. STEWARD added that now such cases as these could be examined endoscopically it was hoped that an early diagnosis would be made often, so that operation would offer some chance of cure. As the growth occupied the extreme upper end of the œsophagus or lower end of the pharynx, the lumen soon became much narrowed and symptoms were therefore produced fairly soon, so that patients came for treatment early. There seemed little doubt that these cases were only locally malignant, the patients dying, as a rule, of starvation, without much glandular infection. It had not yet been explained why such cases practically all occurred in women, and mostly in comparatively young women. He emphasized the manner in which the stenosis resulting from removal of the growth was overcome. There were no signs of secondary deposit, or of glandular infection, and the patient had increased considerably in weight since the operation. She could now take ordinary diet.

Dr. F. DE HAVILLAND HALL said that if a physician might be allowed to intervene on a surgical question, he would like to make a remark on the anæsthetic. At the Hospital of St. Luke, when Mr. Carless removed half a tongue for epithelioma, he was impressed by the convenience of the intravenous use of ether—1 oz. of ether to a pint of normal saline solution, introduced into the median cephalic vein. The patient took the anæsthetic very well, and it

very much facilitated the operation, because no trouble in the air passages ensued. He supposed one would be told that there were risks about the anæsthetic given by that method, but for operations about the mouth the advantages of the intravenous method were so obvious that it would probably become general. The sexual proclivity of some of the forms of malignant disease had been mentioned, and it seemed also that malignant disease affecting the pharyngeal aspect of the cricoid cartilage was mostly found in women, and generally in fairly young women.

Anomalous Œdema.

By Frederick Langmead, M.D.

A girl, aged 18, who since the age of 12 had suffered from an unexplained œdema. It began in the legs, the left being affected first. This leg had been encased in plaster of Paris at that time, and afterwards put up in a Thomas's knee-splint. There is no record to show why this was done, but the patient said that it had never been painful. When the plaster was removed the swelling recurred and was unaffected by splinting, but, on the contrary, a similar swelling began in the other leg. The ankles were affected first. She came under the exhibitor's observation in March, 1910, for a similar condition of the hands, at first in the left and shortly afterwards in the right. In the hands the swelling started on the dorsal aspects. The condition then found has persisted, but fluctuates considerably in intensity. Symmetrical œdema of all four limbs is present, accompanied by blueness, coldness, and sweating.

The œdema is most marked in the legs and usually is confined below the knees, but has extended above on one or two occasions. Sometimes pitting cannot be obtained by pressure, at other times firm pressure causes it. As in the legs, so also in the arms the œdema is most pronounced at the distal extremities. It lessens if the parts are raised, and increases if they are allowed to hang down. It is almost or completely absent in the morning, but gradually increases during the day, especially if the patient stands much. The dependent position of the arms during a washing-day is always followed by considerable swelling of the hands. The blueness is intensified by cold, to which she is very susceptible, but no sequence of events as occurs in Raynaud's disease was detectable. No redness, throbbing, or burning (suggestive of erythromelalgia) has been noted.

The hands and feet are always clammy, not infrequently beads of sweat standing out from the skin, and are cold at the same time. Recently she has complained of swelling of the cheeks after meals, which subsides in an hour or so.

She is very prone to septic sores. Any slight injury produces a sore, which weeps clear fluid freely, and a sodden, indolent ulcer results which heals with difficulty. Reddish-purple swellings have occurred occasionally in the œdematous regions, looking like chronic abscesses. A few of these have suppurated, but the majority subsided. On one occasion *Staphylococcus aureus*, and on another a streptococcus, was isolated from the discharge.

There is no sign of heart or kidney disease. The blood count is normal. The patient's general health is very good. She does her work as a general servant without trouble until the evening, when the swelling of her hands and feet causes her difficulty in holding small objects (such as a needle) and in walking. She is the only child: no history of any similar affection in the family can be elicited. The only treatment which has been beneficial is the use of elastic stockings. Thyroid extract has made no difference.

DISCUSSION.

Dr. LANGMEAD said that he brought the case for three chief reasons. First, the patient seemed to form a connecting link between a good number of different kinds of cases which had been shown before the Section and before other societies. In that she had œdema of her legs, which was sometimes so hard that it would not pit, the case resembled those which had been called, perhaps wrongly, trophœdema, but in that condition he believed the arms had never been affected. The face was swollen also, but only transitorily; it swelled up quickly after she had drunk tea, or when she became nervous, looking like an urticaria. The œdema of the legs and arms did not seem to be influenced by her nervous disturbance. Possibly the œdema was vasomotor in origin. In favour of that view was the blueness and coldness and much sweating of the extremities. It did not quite fit in with any of the described cases, as far as he knew. It also somewhat resembled acrocyanosis. Secondly, he would be glad to hear the opinions of members as to whether there was any relationship between the various conditions described as anomalous œdema, trophœdema, acrocyanosis, Raynaud's disease, and giant urticaria, whether there was a fundamental factor common to them all. Lastly, he would be glad of help as to treatment. She had been better as a result of wearing elastic stockings, but no form of medical treatment which he had given by the mouth had benefited her; this included thyroid and vaso-dilators. The condition was progressing.

The PRESIDENT (Sir William Osler) thought the fact that the œdema never disappeared entirely excluded the case from the group of angio-neurotic œdema. He did not think there was an instance of the latter on record which had developed into permanent œdema of either hands or legs. A patient might have recurring attacks for eighty years without the œdema becoming permanent. This case was anomalous in his experience; it looked more like the condition which Charcot described as "blue œdema."

Diffuse Symmetrical Lipomatosis.

By F. PARKES WEBER, M.D.

THE patient is a publican, aged 53. He is a man of good muscular physique, but has a number of soft subcutaneous swellings characteristic of diffuse symmetrical lipomatosis. These are situated on both sides, in the mastoid, parotid, deltoid and inguinal regions, and also in the middle line, just below the chin, just above the sternal notch, and over the symphysis pubis. They were first noticed about four years ago, and are neither painful nor tender to palpation; neither does the patient suffer from rheumatic or other pains. The thyroid gland appears to be well developed. The liver cannot be felt, but there is a suspicion of commencing cirrhosis. The conjunctivæ over the sclerotics have a yellowish tinge. The urine (January, 1912) is of high colour, of specific gravity 1025, and free from albumin, sugar and bilirubin. The patient has been a publican for seventeen years, and has been accustomed to take much alcoholic drink every day; he had severe pleurisy ten years ago, and gout in both big toes about three years ago; he is subject to hoarseness; he denies ever having had any venereal disease. He says he has never noticed any sexual impotence; he has three children living. The case is a good example of diffuse symmetrical lipomatosis occurring, as it generally does, in a man of robust build, who has been accustomed to rather free and habitual indulgence in alcoholic drinks. The case is a less advanced one than that in a man, aged 52 (connected with a public-house), shown by Dr. Weber at the old Clinical Society of London on January 22, 1904,[1] but more advanced than that in a woman shown by Dr. Weber at the same meeting.[2]

[1] *Trans. Clin. Soc. Lond.*, 1904, xxxvii, p. 221.

[2] *Ibid.*, p. 220.

Osteitis Deformans (Paget's Bone Disease) with Chronic Eczema.

By F. PARKES WEBER, M.D.

THE patient, A. R., aged 70, a compositor, presents the typical thickening and bending of his tibiæ and femora characteristic of osteitis deformans. The disease is as yet of only moderate degree, and is limited to his lower extremities. The changes commenced, or at least were first observed, in the left tibia six years ago, and at present they are best marked in that bone and the left femur. The eczema, to which he has been subject more or less for many years, at present chiefly affects his legs. On the whole the patient seems to have enjoyed good general health, but has suffered twice from rheumatic fever, forty and twenty-four years ago respectively. There is no history of any venereal disease. Nothing abnormal can be discovered in the thoracic or abdominal viscera or in the urine. A noteworthy feature of the present case is that as yet there has been no pain connected with the bone disease.

DISCUSSION.

Dr. DORNER, who showed the case for Dr. Weber, added that the eczema had some relation to the old age of the patient. There was no specific disease, and he had healthy children. In answer to the President, he said there was no involvement of the head and no pain. The condition had come on gradually during the last six years.

The PRESIDENT regarded the case as a variety of that remarkable disease osteitis deformans, in which the change was confined either to the tibiæ or the fibulæ, occasionally involving the femora in old persons; a senile change, and not progressing to the head or arms or other parts of the body. He had seen four instances of it, three of them in friends whom he had known for a number of years, and whose cases he had followed. There was a reduction in height owing to the bending of the tibiæ and the gradual bowing, but no pain or discomfort.

A Case of Persistent Nystagmus associated with Periodical Vomiting.

By GEORGE F. STEBBING, M.B.

J. J. P., MALE, aged 28, fishmonger, though at times he has worked as a barman.

History: Scarlet fever at the age of 3; discharge from both ears when a baby, but patient does not know how long the discharge lasted. At the age of 21 patient had seven fits during one week, but has had no others either before or since. In these fits, of which he had no warning, he was unconscious and incontinent; he bit his tongue once and twice "bruised his eyes." Fifteen years ago patient began to have pains in the lower and back part of the head, and these pains have remained since. Ten years ago, on a day that he remembers well, he ate his tea with a good appetite and soon afterwards, without any feeling of sickness, vomited it; later, the same thing occurred with his supper. For several days he vomited everything he swallowed, and then regained his usual health. Ever since he has been subject to attacks of vomiting which last from a few days to six or eight weeks. These attacks are gradually becoming more frequent and more severe, and he states that he has lost flesh considerably. During these attacks he sometimes feels sick but in many of them he vomits without any sensation of sickness. Since these attacks of vomiting started he has had pain in the lower part of the abdomen; he states that the pain is not relieved, but is occasionally made worse, by the act of vomiting. Three and a half years ago his friends told him that "his eyes were always moving about." Two years ago both his legs began to get weak so that he became unable to stand without support, and one year ago he "lost all feeling in them." Patient was treated as an out-patient at St. Bartholomew's Hospital in 1907 for frequency of micturition.

Condition at present: The patient is thin, but does not appear to be markedly wasted, and he has not wasted during the last year while he has been under observation. He has a nystagmus which is most marked while the eyes are at rest, and is of a mixed type, consisting of coarse horizontal movements with a fine rotatory movement superadded. The nystagmus is present in all positions of the eye, but is more marked when patient is looking to the left than to the right, and

is only slightly marked when patient is looking up or down. When patient fixes his gaze on some particular point the nystagmus momentarily ceases. There is no ptosis or squint; pupils are equal and react well to light and accommodation; there is a slight degree of hippus. The patient states that after reading for some time the print becomes blurred and he has to rest before he can go on.

Dr. Marcus kindly examined the patient and reports as follows: "The pupils are round, not quite equal in size, sometimes the right, sometimes the left pupil being the larger one (see-saw pupils). This change in size was observed within the time of my examination (about half an hour). The reaction of the pupils to light and accommodation occurs with perhaps excessive readiness; this great sensitiveness of the pupils may have something to do with the hippus that is present in the case. The fundi are quite normal. Vision and refraction are as follows: Right, + 1·0; cylindrical axis, 45; vision, $\frac{5}{18}$. Left, + 1·0; cylindrical axis, vertical; vision, $\frac{5}{18}$. The patient can read small newspaper print; he gets tired after reading for about half an hour. There is no central scotoma for colours, and the visual fields appear to be free."

The power in both upper limbs is good, and there is no deformity of the hands. Sensation is normal all over the upper limbs. There is no tremor. Abdominal and cremasteric reflexes are normal. There is marked loss of power in both legs, but very little wasting. Knee-jerks are present; plantar reflexes are sluggish, but when obtained are both flexor. There is anæsthesia of both legs of the "stocking type," the upper limit, however, not being quite horizontal; over the affected area sensation is lost to light touch, pain, heat and cold; there is no hyperæsthetic area. There is no loss of control of the sphincters, but patient has had occasional attacks of frequency of micturition; no abnormal constituent in the urine. Ever since patient has been under observation he has complained of abdominal pain and tenderness to deep pressure between the umbilicus and symphysis pubis, the tenderness being slightly more marked on the left side than on the right. Patient has also complained, at intervals, of pain in the occipital region, and there is tenderness to pressure over the upper part of the occipital bone.

Mr. Jenkins kindly examined the patient's ears, and reports that there is a cicatrix on each membrane, and slight internal ear deafness, more marked on the left side than on the right. The caloric test is positive on each side.

The Wassermann reaction is negative, and large doses of potassium iodide combined with mercury, administered for a period of six weeks, did not produce any change in patient's condition.

Dr. STEBBING added that the notes supplied were several months old, and since then there was some change in the condition; there was some improvement in the sensation of the right leg, and the patient had now some perception of the difference between heat and cold. On the left side, however, he still remained quite anæsthetic to light touch, pain, heat, and cold. The remarkable point was the periodical vomiting which he had had for ten years. He believed one or two of the attacks had responded to dietetic treatment, and he was puzzled about the correct diagnosis. The classical signs of disseminated sclerosis were not present, and the nystagmus was not of the type seen in that disease. He had a tender point to the left of the umbilicus, and this had been present during the year he had had the man under observation. The patient said the pain was worse during the attacks of vomiting, and was not relieved by the vomiting, but often made worse by it.

A Case of Hirschprung's Disease: Congenital Dilatation of the Colon.

By HECTOR MACKENZIE, M.D., and W. H. BATTLE, F.R.C.S.

A FEMALE, aged 9, admitted, under the care of Dr. Hector Mackenzie, into St. Thomas's Hospital on November 27, 1911. When a baby she suffered from attacks of diarrhœa. Since the age of 4, when she had broncho-pneumonia, she has suffered from constipation. A year ago the constipation was accompanied by pain and vomiting. This occurred without any relation to food. She was treated for tuberculous peritonitis. The constipation and other symptoms have increased since that time, and have been bad for the past five weeks; a purgative being required every other night. The pain has been very severe at times, the child having been doubled up with it.

On admission moderate distension was present, more marked on the left than on the right side, and most evident in the upper region of the abdomen. On inspection, particularly after palpation, slow waves of peristalsis are seen, most marked in the umbilical region, and seeming to travel from left to right, but also present over the descending colon and passing downwards, but without pain. Tympanitic note general over the abdomen.

Occasional attacks of pain with peristalsis later, nearly every day, varying in time of onset. Abdomen generally larger than normal and sigmoid easily felt. The distance between the ensiform cartilage and the umbilicus is greater than usual at her age. The result of X-ray examination after giving of a bismuth meal: "It (the meal) all went to the right side just above the iliac crest. Four days later, there having been one action of the bowels, there was still some bismuth low down in the mid-line of the pelvis."

DISCUSSION.

Dr. LANGMEAD pointed out that the patient had facial irritability, and that it was known that in a certain number of patients with dilatation of the colon tetany was engrafted as a complication. It would be of interest to see whether tetany would develop, or whether facial irritability would persist alone.

Mr. KELLOCK said the surgery of these cases was not only of interest, but extremely important; his experience of the treatment of them had not been very satisfactory. The outlook, he believed, without operation was practically a hopeless one; the dilatation increased, and the child would probably die eventually from wasting. This prospect was a justification for trying to remedy the condition. Two courses were open if it were to be dealt with surgically. One was anastomosis, and the other excision of the large bowel and planting the ileum on to the upper part of the rectum. This had been done with temporary success, but eventually there was recurrence, and the condition became almost as bad. The bowel seemed as prone to distend when unoccupied as when occupied, and the danger of ulceration was then almost as great as when fæcal matter was passing along it. So although this seemed the milder operation, the condition was not much remedied thereby. The other alternative was resection of the whole of the large intestine, and this seemed to offer the better chance. The operation could be done in two stages, and was, he thought, quite feasible in this case. Another thing which might be tried in this case was the hypodermic injection of salicylate of eserine, which was so successful in reducing distension of the abdomen after operations. He took it that all medical methods to get the bowel to act had been tried before the patient came under Mr. Battle's care.

Mr. GORDON R. WARD suggested that there was a third surgical alternative—namely, the minimal operation of appendicostomy—to allow of direct medication of the bowel. He had seen one similar case benefit much by this procedure, which had also been of use in a case of paralytic distension of the intestines following conservative treatment of an appendix abscess some years before.

Mr. C. H. FAGGE said he considered that the right treatment was to remove the colon, as suggested by Mr. Kellock, in two stages. But the great feature in the first—namely, the short circuiting operation—was that the ileum should be

implanted into the rectum itself, not into the pelvic colon: in doing this the ileum should be divided, and its proximal end anastomosed into the side of the rectum. That part of the intestine now known as the pelvic colon should be excluded from the alimentary canal with the ascending and descending colon and iliac colon. In the cases he had seen, if the ileum was implanted into the pelvic colon, very little good resulted; whereas if the implantation was done into the rectum, temporary benefit resulted, even in much more feeble children than this patient, and it was then possible later to undertake the greater operation of resection of the whole colon under more favourable conditions. He believed this child would stand colectomy if it were done in two stages.

Dr. THURSFIELD wished to suggest that the resources of medicine had not yet been exhausted in this case. At the present time there was, in St. Bartholomew's Hospital, a child suffering from the same disease, in whom the bowels had been kept open by means of enemata and purges for over nine months, and the patient was now beginning to pass his motions and clear out his colon of his own accord. He did not see the present patient, but from the history given in the notes he gathered that the condition was not a *congenital* dilatation of the colon, but that it had come on at about 4 years of age. Therefore he would have hoped that persistent emptying of the colon, continued, if necessary, over months, might have induced the colon to recover its activity. He suggested this because the surgical opinion seemed to be that any operation was of only doubtful value. If he might offer a surgical opinion he would say that if, after a considerable trial of medicinal means, there was no great benefit perceptible, it might be worth while to try appendicostomy combined with medical methods, keeping the colon quite empty for months before resorting to the more severe surgical procedures.

The PRESIDENT said he had been interested in the disease for many years, and had had, perhaps, an unusual experience of it. A few of the patients grew up, but rarely one found instances in persons of 40 years of age. There was the famous "balloon man" of Philadelphia, whose abdomen was enormous. The colon, now in the Museum of the University of Pennsylvania, was as large as that of a horse, and held 40 lb. of fluid. The cases were liable to serious accidents, such as acute obstruction. One of his cases had ulceration and perforation. Medical treatment as a rule was hopeless. In one case, however, some good followed the measure mentioned by Dr. Thursfield—persistent irrigation of the bowel, and keeping the colon empty. That child did well for a year or two, and it was regarded as a satisfactory recovery; unfortunately, however, the condition returned, and the child died of acute obstruction. Any diversity of opinion was on the side of the surgeons, because most physicians who had seen much of the disease felt that it was hopeless from the medical standpoint. Cure followed removal of the colon by Sir Frederick Treves. The condition was really a serious and critical one, and the cases should be handed over to the surgeon at the earliest possible date.

Dr. MIDELTON said the subject had been discussed at the Section for the Study of Disease in Children, and the opinion there generally expressed was that surgery was not very successful. The colon was tremendously hypertrophied as well as dilated, and during the operation for removal it offered great resistance under the surgeon's hand, violent peristalsis occurring, so that he had great difficulty in carrying out his manipulations.

Mr. BATTLE asked what was the age of the patient referred to by Dr. Thursfield. He also asked whether Mr. Kellock was speaking about cases in which the small intestine had been placed in the upper part of the rectum, or those in which it was placed in the pelvic colon, or whether he was speaking of cases in which the intestine had not been cut across, but lateral anastomosis only performed.

Dr. THURSFIELD replied that his patient was 6 years of age, and the condition was present since babyhood.

Mr. KELLOCK replied to Mr. Battle that he referred to cases in which the small intestine was implanted into the lower end of the sigmoid, and the communication with the cæcum divided, but the descending colon not cut off from the pelvic colon.

A Case of Tabes Dorsalis with one Knee-jerk absent and the other brisk.

By HERBERT FRENCH, M.D.

B. C., AGED 59, who has all his life been occupied as a costermonger, pushing a fruit barrow, and who has lived a very exposed life, frequently subsisting on beer between breakfast time and supper, and often attending race meetings and sleeping out in the open in all sorts of weather, states that he had gonorrhœa when aged 16, for which he was treated for six weeks at St. Thomas's Hospital, but he is not aware of having had a chancre. His wife has had one miscarriage, but no living child. The Wassermann reaction is positive. Five years ago, when aged 54, he suffered from pain in his right foot, spreading thence to the right ankle and after a few weeks upwards into the calf of the right leg; since this time he has been subject to shooting pains which are confined to the right leg, never affecting the left, and at the same time, whilst the left leg feels to him normal the right has become numbed, cold and affected by a feeling which he describes as one of uselessness, although objectively he can move it very well. Perhaps the most striking feature about the case is the fact that whereas the right

knee-jerk is entirely absent, the left is not only present but is even brisker than normal.

It is by no means uncommon to find some degree of persistence of the knee-jerk in tabes dorsalis, but it is less common to find one knee-jerk entirely absent and the other as brisk as is the case in this patient. The following is a summary of the sensory and motor phenomena presented by the patient:—

Eyes.—Pupils: Right slightly larger than left; both circular; the right reacts very slightly to accommodation and not at all to light; the left reacts slightly to accommodation, but not to light.

There is no ptosis, no optic atrophy, and no ocular paralysis.

Sensory Changes.—(*a*) Subjective:—

In the right leg typical lightning pains; no gastric or other visceral crises, but a definite girdle sensation, of which he has complained on two occasions recently, though it is not present continuously.

(*b*) Objective:—

Anæsthesia.—He shows deficient sensibility to light touch all over his body, though he appreciates heavier stimuli in all parts. The most marked deficiency in cutaneous sensibility is upon the outer aspect of the dorsum of the left foot, the dorsum of the right foot, the peroneal region of the right leg, and the ulnar aspects of both forearms.

Heat and Cold.—He is very inaccurate in distinguishing heat and cold stimuli all over both legs, especially the right; he is less inaccurate on the abdomen and arms, though even here there is impairment of the power to distinguish heat from cold.

Hyperæsthesia.—None present.

Analgesia.—(*a*) Superficial: He feels a pin-prick, but does not appear to mind a pin being stuck through the skin anywhere over his body or limbs. (*b*) Deep: He has almost complete deep muscular analgesia in the right calf and thigh, but resents a grip of the left calf or thigh that would not be complained of on the right side.

Sense of Joint Movements.—He does not appreciate passive movements of the toes and ankles of either side accurately; sometimes his replies are correct, but more often he thinks his toes are being bent downwards when they are being dorsiflexed, and he is especially inaccurate in his replies when the joints are moved laterally.

Vibratory Sense.—This is absent over tibiæ, fibulæ and sacrum: it is present over radii, ulnæ and sternum.

Testicular Sense.—Both testes are relatively anæsthetic and analgesic.

Sphincters.—There is no trouble with either the bladder or the rectum.

Sexual Desire and Power.—Both present.

Locomotion.—Romberg's sign is very well marked. He is unable to walk toe to heel. There is no marked ataxy in the arms; the ataxy of the legs is less marked when he is standing on his left leg than when he is standing on his right.

Hypertonus is not marked.

Trophic and Vasomotor Disturbances.—There is no perforating ulcer of either foot and no definite vasomotor change, unless possibly that the right leg is slightly colder than the left; he complains that it feels to him very much the colder, but this is not so obvious objectively.

Reflexes :—

	Right	Left
Knee-jerks	*Absent*	*Brisk*
Tendo Achillis jerks	Absent	Absent
Arm-jerks	Present	Present
Abdominal reflex	Present	Present
Scrotal reflex	Present	Present
Plantar reflex	Flexor	Flexor
Ankle-clonus	Absent	Absent

DISCUSSION.

Dr. FRENCH added that he showed two other cases of persistence of one knee-jerk in tabes dorsalis at the Medical Society of London two years ago, and in one of them it was a question whether there had not been hemiplegia, due to thrombosis of the contralateral middle cerebral artery, to cause exaggeration of the knee-jerk which otherwise might have been absent. There had been no such seizure in this case, and he believed the knee-jerk had never been different from what it now was.

Dr. J. D. ROLLESTON asked if the unilateral knee-jerk was due to the man's occupation; whether the right leg had been specially subject to some strain. Some years ago an interesting case of unilateral loss of knee-jerk was reported by a Swiss writer[1] in a tabetic cobbler, and the author attributed it to the fact that the cobbler hammered the boots on his right knee, the slight continued trauma being the cause of the unilateral loss of knee-jerk. The same writer examined twenty other healthy cobblers in the same town, and found their knee-jerks were normal on both sides.

Dr. FRENCH, in reply, said he did not think the occupation could have anything to do with it, as the man had been a hawker of fruit from a barrow all his life, and in the other two cases he mentioned he did not think occupation entered into the matter either; one was a grocer and the other a schoolmaster.

[1] R. Burnand, *Rev. méd. de la Suisse romande*, Geneva, 1908, xxviii, p. 282.

Neuro-fibroma of the Supra-orbital Nerve in the Orbit.

By W. H. Battle, F.R.C.S.

Ten years ago the patient, a girl, aged 22, was under Mr. Battle's care for a neuro-fibroma of the left median nerve above the internal condyle of the humerus. He removed it without difficulty, and there had been no trouble with the nerve since. Five or six years ago she came with a pachydermatocele on the right side of the head, behind the ear, of 2 in. circumference. There was a change in the bone underneath. The portion of scalp affected was excised, and numerous little plexiform neuromata were found. The surface of the skull could be felt to be considerably changed, the external table appeared to be hollowed out, as if there had been pressure over it by a firm tumour. A few days ago she came with neuralgic pains over the right forehead and scalp, which appeared to be due to small neuro-fibromata in the supra-orbital nerve. The patient thought her pain was due to them, but the tumour for which she was exhibited to the Society, which appeared to be about the size and shape of a small date-stone, was found on examination of the course of the nerve at the junction of the inner with the outer two-thirds of the orbit. The lesion was awkwardly placed for operation, and it was a rare position for neuromata. She had molluscous tumours about the body, with pigmentation of the skin over them. No other disease or tumours could be found anywhere. Apparently she was naturally dark-skinned, and he thought it possible from that and other signs there was some mixed blood in her. Her brother had had a painful tumour removed from the right side of his neck, and the mother was said to have tumours on the back of the left forearm.

Congenital Absence of the Left Half of the Diaphragm, simulating Pneumothorax.

By WILFRED HARRIS, M.D., & W. H. CLAYTON-GREENE, F.R.C.S.

WE have deemed the following case worthy of putting on record, since to the best of our knowledge it is unique, and we can find nothing similar in a brief examination of the literature. The case came under our notice in the following manner: Mrs. X., aged 31, a lady 5 ft. 10 in. in height, strongly built and heavy, married five years ago, with two children, about the end of 1910 noticed she was getting somewhat stouter, having to let out her corsets. About the same time she noticed an aching pain in her left side about the level of the sixth rib, which she thought was due to pressure of her corsets.

In April, 1911, her family thought she must be pregnant, and about this time her periods, which had previously been profuse, lasting ten days every month, now became scanty and lasted only three days, though they remained regular in this manner throughout. She was examined at various times by three different doctors in the country, each of whom confirmed the question of pregnancy, in spite of the regular though scanty monthly flow; she therefore made all preparations, and in December had down a monthly nurse from London to await events.

Since April she had increased 10 in. in girth, and the pain in her side had gradually become more troublesome, making her limp on the left leg, and she could not sit properly on a chair, having to sit sideways and on the edge of the chair. She had become more and more breathless, but was encouraged to go about and do everything as usual. Five years previously she had had a bad fall downstairs and had broken two ribs on her left side, and about a year ago she had again jarred her left side badly when motoring.

On December 21, as matters did not appear to be progressing, Dr. Bott was called down to see her, and he found no sign of pregnancy, but on examining her chest found dullness at the left base, and he therefore brought her up to London at once. She vomited twice in the train and again after arrival. The following day the pelvis was examined under an anæsthetic, and the uterus was found to be normal. She was then skiagraphed by Dr. Harrison Orton, the skiagrams demonstrating

a sickle-shaped shadow at the left base, with a brilliant area above and internal to the shadow.

The diagnosis was then made of localized pleuritic effusion, with a separate localized pneumothorax, and partial collapse of the left lung. One of us (Dr. Harris) was then called in to see her, on December 29, with Dr. Bott. She still complained of the pain in her left side, and of breathlessness and difficulty in walking. On examining her chest he found hyper-resonance of the left front, with absence of air-entry, and the heart sounds loudest to the right of the xiphisternum. Behind, the air-entry was slightly audible at the apex, and there was dullness at the base, and in the axilla up to the eighth rib. Auscultation at the back while she drank some water revealed loud sounds below the spine of the scapula, soon after swallowing. There was no succussion, nor splashing sounds, nor was there any coin sound, nor any bulging of the chest and intercostal spaces. There was loud air-entry over the whole of the right side of the chest, and the abdomen and spine were normal. She complained of a good deal of hyperæsthesia over the left fifth to seventh dorsal skin areas, disliking the touch of the bedclothes, and saying it felt like sand-paper when the hand was rubbed over this area. No analgesia or other loss of sensation. She limped on the left leg in walking, and sat on a chair on the front edge, with her left side bent down for support, and her left leg drawn away under the chair so as to extend the hip. Taking into conjunction the symptoms suggestive of pneumothorax with the curious attitude on sitting and walking, which were suggestive of some sub-diaphragmatic mischief, Dr. Harris made the diagnosis of "diaphragmatic hernia," and considered that the rupture of the diaphragm might have occurred as the result of the bad fall downstairs five years ago, when she broke two ribs, and that the further blow on the left side a year ago had aggravated matters, so that the stomach and some of the small intestines had gradually been pushed through into the left chest, with consequent collapse of the left lung, and thus causing the increasing breathlessness and pain in the side. He therefore advised against aspiration, and ordered 4 oz. of bismuth carbonate to be given with a pint of bread and milk previous to a further examination of the chest by X-rays two hours afterwards. This was done on the following morning, when Dr. Orton took further photographs, and was also able to demonstrate on the screen the mass of bismuth within the chest. Indeed actual peristalsis of the bismuth was observed within the left chest, proving beyond the possibility of doubt the presence of intestine in the left thorax. On standing her up

again in front of the screen, it was found that the sickle-shaped shadow seen at the left base a few days previously had now vanished, and its place was taken by a horizontal layer of fluid immediately beneath the brilliant area, and on shaking the patient slightly the fluid was seen to splash in the most characteristic fashion. This no doubt was the stomach with its contents.

Considering the diagnosis of diaphragmatic hernia to be now proved, a laparotomy was advised in order to deal with the condition, in the hope of reducing the hernia, and repairing the rent in the diaphragm. Mr. Clayton-Greene was therefore called in, the operation being performed on January 1. On inserting his hand he found numerous peritoneal adhesions, and a total absence of the left half of the diaphragm and of the parietal pleura on the left side, it being possible to feel the ribs posteriorly practically up to the clavicle. The left lung was extremely small, and the left thorax filled with stomach and intestines, the right half of the diaphragm being normal. The wound was at once sewn up, and nothing further done. She has since made an uninterrupted recovery, and is perfectly well, except for slight tenderness on the left side.

DISCUSSION.

Mr. CLAYTON-GREENE said he saw the patient on the morning of January 1, having seen the skiagrams. He derived very little assistance on the subject of diaphragmatic hernia from the books on surgery and anatomy. When he opened the abdomen, with the idea of exploring, he found, on passing the hand into the thoracic cavity, that there was no definite peritoneum in the pleural region, and no trace of diaphragm at all. The intestines were adherent together, and he thought much of the patient's pain was due to localized peritonitis in the thoracic cavity. He and Dr. Harris passed a hand up as far as the clavicle, and felt resistance, but he could not say whether it was diaphragm or lung. The abdomen was closed. The patient said she was better, but he did not know whether the effect was moral or physical. Since the operation he had learnt much about diaphragmatic hernia from Professor Keith, who had written largely on the subject. In Professor Keith's experience there was an instance in a man who lived to over 70 years with a similar state of affairs, and there were specimens at the College of Surgeons bearing on the condition; three showed absence of diaphragm in the fœtal stage, and in one of them was a condition very similar to that in this case—namely, apparently complete absence of the diaphragm at the level of the thoracic opening, and a compressed lung, with viscera crowded upon it at the top. His opinion previously had been that such a condition was not compatible with life. Anatomists were not agreed as to the mode of origin of the condition or as to the development of the diaphragm. It had been regarded as a species of septum, splitting

off the pleural from the peritoneal cavities. Professor Keith did not agree with the older ideas of development of the diaphragm at all, and in his paper had certainly made out a good case for the theory of the development of the pleural cavities, and of the diaphragm itself. His idea was that in the upper part of the pleuro-peritoneal cavity there were three muscular layers, as in the abdomen, and he compared the budding-off of the lung to the descent of the testis, ultimately reaching the scrotum, which was preformed by the various layers of the abdominal wall. He suggested that the lung was budded off into a recess between the external and internal intercostal muscular group on the one hand, and the transversalis on the other, and that the anterior and posterior portions were stripped off and crushed down towards the abdominal cavity by the developing lung. Failure of the portions to meet in the middle resulted in a congenital abnormality. The extent of the abnormality would depend on two things: the degree of development of those two folds, which might be suppressed to an incredible extent, and therefore were comparable to cleft palate, in which there was almost complete suppression of the palatal processes. Fusion might occur to some extent, and nothing more than a canal or tract might be left behind, through which a small hernia could take place. Another cause which Professor Keith suggested was the pressure of ingrowing viscera. If the various diaphragmatic elements failed to fuse, the abdominal pressure exerted upon the thorax would tend to increase the size of the opening and press the rudimentary buds of diaphragm back against the chest wall, and lead to their complete atrophy. That seemed a sound explanation. Diaphragmatic herniæ were not rare: Paillard had collected records of 400 cases, and they occurred chiefly on the left side of the diaphragm. One case, in the London Hospital, was associated with congenital dilatation of the colon, there being a hernia through the œsophageal opening of the diaphragm of a portion of the stomach. The question was whether there was a rudimentary diaphragm at the top of the thorax in this case: he thought, from looking at the skiagrams, that there might be, but from what Professor Keith told him he did not think it was likely.

Dr. HARRISON ORTON said it was first thought that the case might be one of hydronephrosis, but the X-ray examination enabled them to exclude that, as on the skiagram both kidneys were seen to be about normal in size and position. The next skiagram showed the shadow at the left base, with the sickle-shaped upper margin, similar to that seen in ordinary pleural effusion. Above this was a bright area, and above this again a very thin line. The bright area was characteristic of air, and it was possible the thin line was due to a rudimentary diaphragm. It was evident there was not a complete pneumothorax, as the fluid line was not horizontal. Dr. Harris suggested that there should be a further X-ray examination after a bismuth meal, and the next skiagram showed the appearances. The bright area seen in the previous skiagram was now seen to be occupied by a dense shadow, cast by the bismuth. This left no doubt that there was intestine in the chest, and a diagnosis of diaphragmatic hernia was made.

Clinical Section.

March 8, 1912.

Mr. WALTER G. SPENCER, Vice-President of the Section, in the Chair.

Case of Progressive Muscular Dystrophy.

By JAMES GALLOWAY, M.D.

THE patient, J. D., a man, aged 31, presents the characteristic features of the better known types of this disease. He was born in Ireland in County Clare. His father and mother are both of Irish birth; there is no history of any similar affection on the mother's or on the father's side. He has two sisters, one of them married, with two healthy children. The patient gives no history of serious disease preceding the development of his present condition. He is not certain if he had measles, scarlatina, or other infectious disease of childhood. There is no history or evidence of syphilis.

He was educated at a school in the town of Ennis. As a lad he took much interest in gymnastics, and by the age of 15 he had become so good a gymnast that he was encouraged to continue his training. He was then sent to a school in Dublin, continuing his training in gymnastics with the idea of becoming an instructor. At the age of 18 he was an expert gymnast, and was employed in training the lads at the school. At the age of 19 he received an appointment as gymnastic instructor in a town in the North of England. He was able to continue his duties for some time. At about the age of 20 he first noticed that he was not able to perform gymnastic exercises with his pupils so well as had been his custom, especially those involving the upper limbs. He found that he had difficulty in such movements as raising his arms to push them through the sleeves of his coat, or to brush his hair. His muscles were unusually well developed as the result of his training, and he says that he then thought that the muscles of the shoulder were too big, and had become "heavy," and

consequently weak in their movements. In a short time he found that he could not continue his occupation as a gymnastic instructor, and obtained at first a month's leave of absence. He rested, performing simple, free gymnastic movements, but was unable to perform these so freely as he should. He then resigned his appointment, and came to live in Woolwich where his parents were settled. Finding himself no stronger, he came into Charing Cross Hospital for the first time about seven years ago under the care of Dr. Montague Murray. He stopped in the hospital for about three months, resting and receiving treatment by means of massage and electricity. As the result of this he felt himself stronger, and returned to Woolwich, where he obtained a

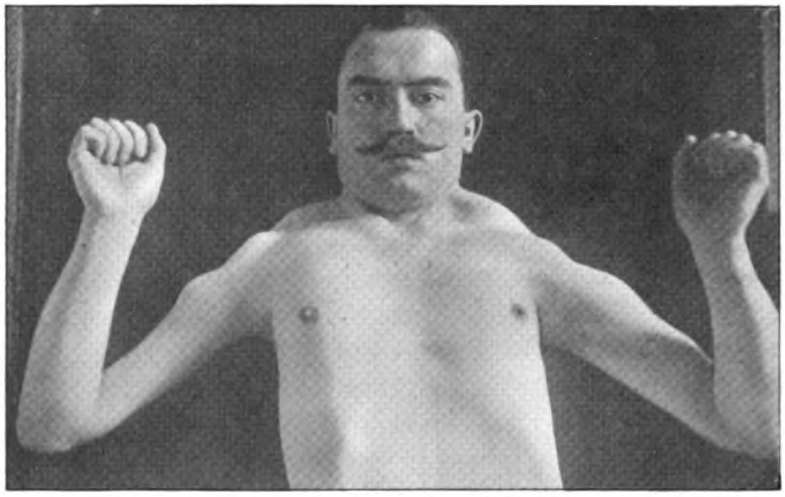


FIG. 1.

Showing the attitude and condition of the muscles while attempting to raise the hands and arms upwards.

situation as collector for an insurance company. He has been able to do this work till recently, though it involved a considerable amount of exertion. He walked about six miles daily on his rounds.

Shortly before Christmas, 1911, he had what he calls a cold and cough, and went to bed for a fortnight, taking no exercise. During that time he felt that he became much weaker, and especially experienced great difficulty in getting in and out of bed, and getting off the floor. Shortly afterwards he returned to Charing Cross Hospital.

With the exception of the condition of his muscles the patient is now in good health, and there is no evidence of disease of internal



FIG. 2.



FIG. 3.

Figs. 2 to 6 show the positions taken while attempting to assume the erect attitude from recumbency. The patient cannot now perform the muscular movements necessary to attain the positions shown in figs. 4 and 5 without being assisted.

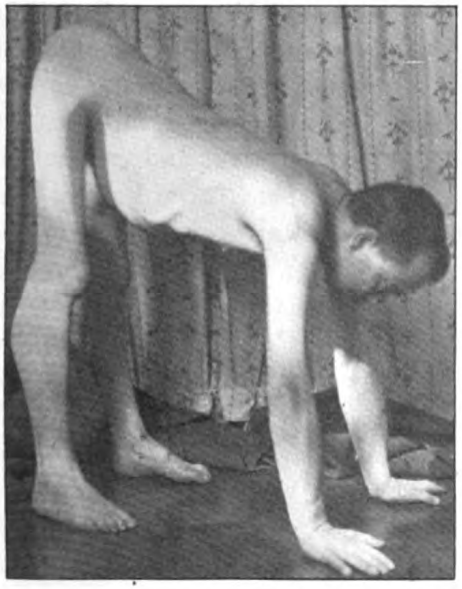


FIG. 4.

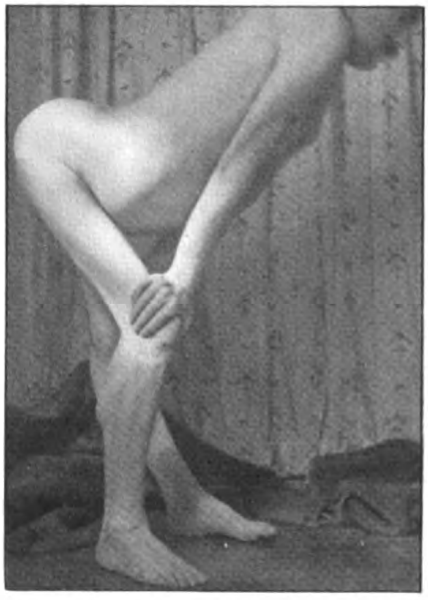


FIG 5.

viscera. His muscular system, however, shows the well-marked changes of widespread progressive muscular dystrophy.

The disease in this case appears to have commenced by affecting the muscles of the face, shoulders and arms (the facial-scapular-humeral type), but is now complicated with other features resembling the pseudo-hypertrophic variety of the disease. The face is little altered in appearance; weakness of the facial muscles has been of recent development only, but there is now to be noticed some feebleness of the muscles of the eyelid, especially in the movements required to keep the eyelids firmly closed. He is not able to whistle as well as formerly, owing to weakness of the cheek muscles, a difficulty he observed shortly before Christmas, 1911, but he is able to clench the jaws and to hold them firmly in this position. Speech is unaffected, and there is no tremor or wasting of the tongue. There is marked wasting and loss of power of most of the muscles of the shoulder girdle on both sides. This is especially noticeable in both deltoids, the latissimus dorsi, the anterior portion of the trapezius, the rhomboids, the upper part of the pectoralis major, but increase in the substance of the supra-spinatus and infra-spinatus muscles is noticeable. A lump of muscle substance remains in both deltoids, at their lower portions just above the attachment. There is marked looseness of both scapulæ, the "winged" attitude being readily assumed. On attempting to raise the forearms the heaping up of the muscular tissues over both shoulders is well noticed, caused by the want of fixation of the scapulæ. Both scapulæ are elevated above the general level of the shoulder on attempting to raise the arms above the horizontal level. The muscles of the arm are greatly wasted, flexion at the elbow is very defective, and can only be obtained by first flexing the wrists, rotating them inwards, and then slowly flexing the elbow. The forearm muscles are much less affected. He may be considered to have strong muscles of the forearm, and good grip of both hands. He has recently noticed that he cannot move his middle finger so strongly to the right and left of the middle line as formerly. On the whole there is more power and muscular substance remaining in the left arm than in the right.

The intercostal muscles and the diaphragm appear not to be affected and breathing goes on easily. There is great weakness of the erector spinæ mass of muscle tissue; in consequence, the position of marked lordosis of the lumbar spine has to be assumed to maintain the erect attitude, either on standing or on sitting. If the patient is allowed to bend forward while sitting on a chair, so as to arch his back beyond

a certain point with the head downwards, he is quite unable to re-assume the sitting posture, and if his chest were not held up he would tumble forwards off the chair, head first. The abdominal muscles seem well developed, with no marked wasting. The gluteal muscles are much diminished in size, and are soft and flabby, and there is marked wasting of all the muscles of the thigh, so that the movements of adduction, abduction and rotation of the hip are very feeble, as are also the movements of extension and flexion of the knee. The calf muscles remain



FIG. 6.

large, but are not so powerful as their bulk would suggest. The anterior muscles of the leg, on the other hand, are wasted. In consequence there is marked talipes equinus.

On standing the patient has the characteristic attitude produced by marked lordosis of the lumbo-dorsal spine, with prominence of the abdomen, and compensatory kyphosis of the dorso-cervical spine. He can walk without support for a short distance, but has to swing his legs outwards and forwards and to lean well back. He says he never falls forwards while walking, but tends to fall backwards. When on the

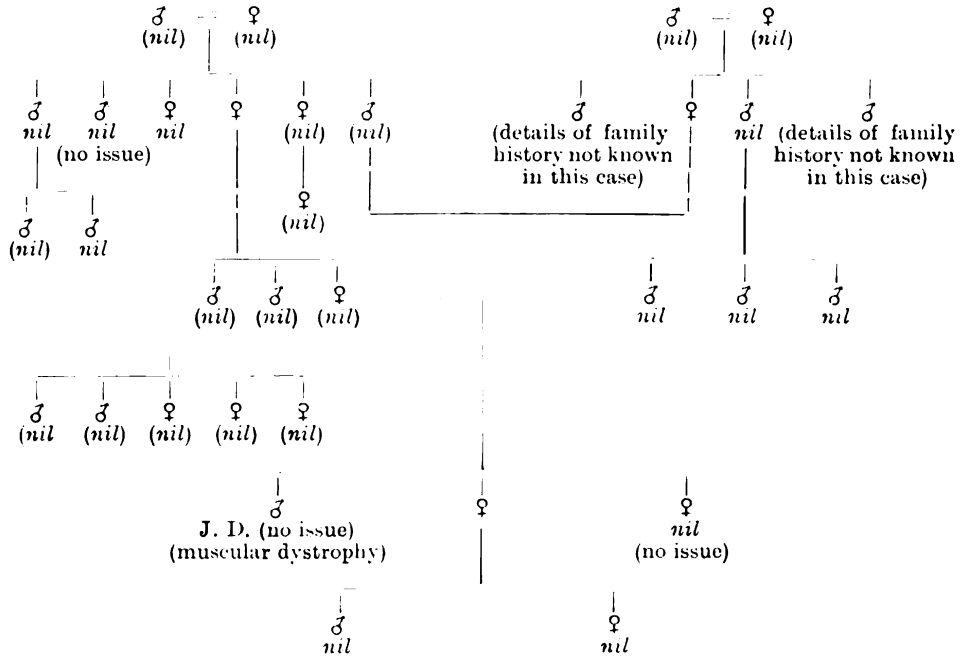
ground he has the greatest of difficulty in raising himself, and now requires help or support to perform the final movements involving the action of the erector spinæ and gluteal muscles. Till lately he was able to get up by placing his hands on his knees, but is now unable to execute this movement.

The case is noteworthy on account of the want of evidence of heredity, the late onset of symptoms, the possible influence of severe muscular exertion, the arrest or retardation of the disease for some years, and the advance of symptoms when, in consequence of a transient illness, he had to give up exercise and to remain in bed for three weeks.

DISCUSSION.

Dr. A. M. GOSSAGE asked whether the patient's mother or his grandmother had any brothers. If it were a sex-limited condition it might go through many generations without showing itself at all, providing there were very few males in the families.

Dr. GALLOWAY, in reply, said that no evidence could be obtained of examples of the disease occurring in the patient's family. But in such a disease as this, in which the defect appeared to descend through the females, it was difficult to obtain a complete record of the male relatives on the female side. The appended family tree gives the information obtainable and is



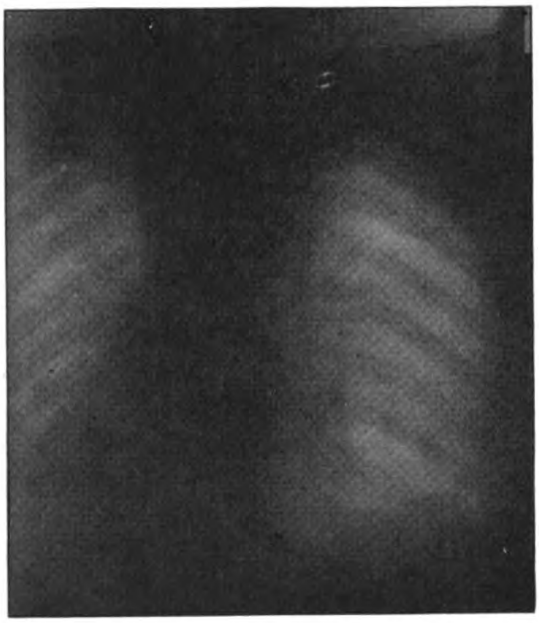
of some importance on account of its negative evidence. Dr. Galloway remarked also on the absence of any changes in the central or peripheral nervous system sufficient to account for the disease. The disease seemed to be a primary degenerative condition affecting the muscles.

Paralysis of Right Vocal Cord, Obstruction to Superior Vena Cava, and Partial Obliteration of Right Radial Pulse from Mediastinal Fibrosis, probably Syphilitic.

By HERBERT FRENCH, M.D.

THE patient, a working man, aged 36, began to suffer from fullness in the head and from congestion in his upper extremities, with consequent difficulty in doing his work, two years ago. The symptoms increased rapidly, and for upwards of a year he was unable to do any work; all the veins of his forehead, face, neck and arms standing out like cords on the least exertion, and his facies presenting the suffused and livid appearance of considerable venous obstruction. During the last nine months these congestive symptoms have diminished sufficiently to allow him to do light work, probably as the result of the development of a deep collateral circulation. There is not any marked degree of fullness of his superficial thoracic veins when he does not work or when the weather is cool, but on working, or when the weather is hot, many dilated superficial veins stand out upon his chest wall, his arms, neck, and forehead. The upper part of his right chest and shoulder are filled out as though by a tumour, especially at the root of the neck above the right clavicle. The patient has a bovine cough and there is paralysis of the right vocal cord. The right radial pulse is hardly perceptible, whilst the left is strong. There is well-marked tracheal tugging. The pupils are unequal, the right being larger than the left. The first diagnoses that the case suggests are mediastinal new growth or aortic aneurysm—the latter particularly, on account of the unequal radial pulses. X-ray examination, however, shows no bulging of the aorta but a dense shadow in the superior mediastinum (*see* figure), and particularly upon the right side, corresponding with the fullness that can be felt there. Malignant neoplasm would seem to be excluded by the facts that there has been improvement during the last year, and that the duration has already been two years without any loss of

weight. Benign neoplasm within the thorax is a possibility; but the patient when a young man had syphilis and gave a positive Wassermann reaction when he first came under observation, so that it is thought that the lesion is mediastinal fibrosis from the healing of former gummata within the thorax, leading to stenosis of the superior vena cava, stenosis of the innominate artery or right subclavian artery, paralysis of the right recurrent laryngeal nerve, and interference with the upper lobe of the right lung. There is apparently



Skiagram (by Dr. A. C. Jordan) showing opacity in the upper part of the chest due to syphilitic fibrosis of the superior mediastinum in a case clinically resembling aortic aneurysm.

no interference with the nerves of the brachial plexus. The interest of the case lies in the remarkable degree to which the effects of mediastinal fibrosis here resemble those of aortic aneurysm, even to producing tracheal tugging; the latter is doubtless the result of both the arch of the aorta and the lower part of the trachea being bound together in the same dense fibrous tissue.

DISCUSSION.

Dr. FRENCH added that it was clearly of very great importance to the patient that the diagnosis should be correct, because if it was an aortic aneurysm it would obviously be dangerous for him to continue with his labouring work, whereas if it was not an aneurysm, but mediastinal fibrosis, even laborious work would not be dangerous, but would assist in bringing about relief by establishing a better collateral venous circulation. In either case mercury and iodide of potassium were the drugs that seemed to be indicated.

Dr. DE HAVILLAND HALL said that in the days before X-rays examinations he believed all physicians would have considered it to be an aneurysm, probably of the innominate artery, and would have treated it by rest, a modified diet, and the administration of iodide of potassium. But as a result of the skiagram he thought this should be put out of court, and Dr. French's view agreed with. The man should be rather encouraged to exercise himself than to keep quiet. A point which Dr. French did not mention was, that the aortic second sound was considerably accentuated, which was one of the points in favour of it being an aneurysm.

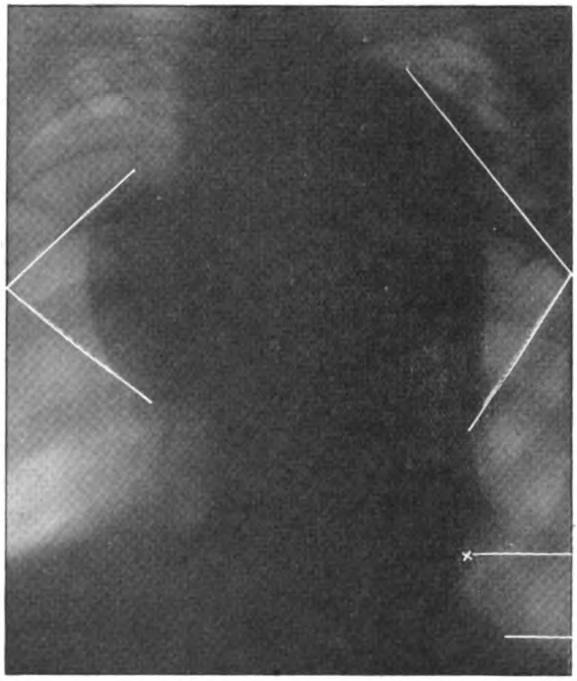
Dr. F. J. POYNTON said he considered there was also a slight diastolic shock, but that might not be held to weigh much against the skiagram. It all turned on the interpretation of this skiagram, and whether there was any pulsation in the shadow. One was largely in the hands of the radiographer for the diagnosis, which was very important to the patient. He had shown at the Medical Society of London a gentleman, aged 80, who had mediastinitis of syphilitic origin. The patient was known in most of the London hospitals, and was still living, though his inferior vena cava was completely obliterated, as far as could be made out.

A Case in which there were two separate large Thoracic Aneurysms.

By HERBERT FRENCH, M.D.

THE patient, a man, aged 61, was formerly in the 10th Hussars, and while in that regiment had had syphilis—namely, in 1871—and was treated for it for two months only. Since that time he had had various occupations, mostly of a labouring character, in the docks and on the streets, and was not aware of anything wrong until a year ago, when

he began to suffer from a cough, and very shortly afterwards he noticed a swelling in the right side of the thorax. He was laid up for a time, but became so much better that he returned to work for four months. Now, however, for several months he had not been able to work on account of increasing dyspnœa and complete orthopnœa. He had to lie propped up with pillows at night, and his breathing was troublesome on the least exertion. His cough was of the "brassy" type, he had a large aneurysmal dilatation of the aorta protruding from the right



Skiagram (by Dr. A. C. Jordan) showing two very large thoracic aneurysms.

side of his thorax and visibly extending from the clavicle above to the fifth right intercostal space below, and reaching out laterally an inch beyond the right nipple line. The heart was not much enlarged. The pupils were unequal, the right being smaller than the left. The left radial pulse was much smaller than the right, and there was paralysis of the left vocal cord. It was difficult to explain this smallness of the *left* radial pulse and paralysis of the *left* vocal cord as the effect of an aneurysm of the ascending aorta bulging to the right, and the signs suggested that there was a second aneurysm further on. This was confirmed by X-ray examination, there being one very large aneurysm

of the ascending aorta, saccular and bulging to the right; and a second and larger aneurysm filling up both the superior and the posterior mediastinum, and reaching down to the sixth or seventh dorsal vertebra (*see* figure). Second aneurysms were not uncommon. There was also an unusual degree of obstruction of the superior vena cava with collateral circulation in distended superficial and abdominal veins, the blood-current in these being from above downwards. There was tracheal tugging. There was also considerable obstruction to the œsophagus, the patient being able to swallow liquids only, and even these causing a sense of choking as they passed the root of the neck. The Wassermann reaction was positive. Large doses of iodide of potassium and of mercury had been given, but with no benefit so far. Treatment by gelatine injections had not been tried. Dr. French had employed it in several other cases, and though he had had no catastrophes in them, such as tetanus, he did not think there was much material benefit to the aneurysm. In this particular case there was no obvious affection of the heart itself, and most of the symptoms were due to the mechanical effects of the two huge aneurysms within the confined space of the thorax, so that if he had been a younger man, so as to make the risk worth while, it might have been a good thing if more room could have been given for the mechanical accommodation of the aneurysms, for instance by rib resection, so as to relieve the tension in the chest: but in the present instance that seemed to be out of the question.

DISCUSSION.

Dr. POYNTON raised the question whether any real good had been seen from the treatment of these bad cases by prolonged rest, starvation and iodides. Often in hospitals the treatment was persisted in for a time, and then the patient went out very much as he was before. He would be very glad to know whether some of the senior physicians present had seen any really good results from the treatment. It was important to know whether one might expect some reward at the end of such a long course of partial starvation and rest, for one needed a true faith to keep alive the courage of the patient.

Dr. DE HAVILLAND HALL said that comparatively recently he had had under care at the Westminster Hospital a man who was a wire-drawer by trade, and whom he showed once or twice at the Medical Society of London. He came in suffering from pain and dyspnœa, and the aneurysm seemed to be advancing. For four or five months he was kept at rest and given iodide of

potassium and a low diet, and he went out so much better that he thought himself cured, but there was still some pulsation. He continued at work for two or three years, and then came in again; after a month's rest be returned to his work again, and had it not been for the Employers' Liability Act he would still have been at work. But the employer got to know that there was something the matter with him, and that he might die from aneurysm, and so told him he could not keep him longer, though he had been twenty-three years at his work. He had also seen other cases benefited, but the difficulty was to follow them up afterwards. He remembered one case, recorded by Dr. Paulin Martin, near Newbury, which figured in the *St. Bartholomew's Hospital Reports*, where, after prolonged treatment, the man hunted and led the life of a country gentleman. He thought such treatment was worth trying for six months, but one must choose the case and the type of patient. He must be of a placid temperament, and must have no valvular complication. An irritable person would not stand the treatment.

Dr. GOSSAGE said Dr. de Havilland Hall would also remember a case which was in the Westminster Hospital for many years, and was exhibited at societies as an example of benefit from treatment by rest and low diet. Subsequently the aneurysm ruptured, and though he had lived for many years in fair comfort, there was no sign post mortem of any repair having taken place in the aneurysm, although admittedly it had not extended.

Dr. FRENCH, in reply, said that absolute cure of thoracic aneurysm was not an impossibility, and he had recorded a case in the *Lancet* for July 10, 1909. The aneurysm there was saccular and as large as a big golf ball; it was filled completely up to the level of the aorta with fibrous tissue, which had been produced in aneurysmal clot. The patient died of uræmia and granular kidneys. He thought if treatment by absolute rest, iodides and restricted diet were to be undertaken with the view of accomplishing a permanent cure it must necessarily be persisted with for even a much longer time than Dr. de Havilland Hall had said; this was obvious if only one thought for a moment of the various processes that had to be gone through before cure was complete. First there had to be arrest of the process of progressive dilatation; this might take weeks. Then there was the production of clot inside the aneurysm; every disturbance of the clot by the movements of the patient had to be made good, therefore clot upon clot was needed, and the process required a long time—many months. Then the clot needed to be organized, and for this there must be budding out of small capillaries from the relatively avascular aorta wall into the clot; those capillaries that were first formed were constantly being broken through, and repeated fresh starts had to be made. Small round cells had to be thrown out when the capillary network had been established in the clot, and these small round cells in turn had to produce fibrous tissue. To lay oneself out to bring about cure of aneurysm by rest, it should be continued for three or four years at least. Therefore it was seldom worth while, especially in a patient getting on in life, though it might be for a man aged 30 or so.

Sarcoma of the Ilium treated by Coley's Fluid.

By ALBERT CARLESS, M.S.

THIS patient, G. C., aged 49, was shown before the Clinical Section in February, 1910.[1] He was admitted to hospital in October, 1909, and his symptoms existed for eighteen months before that date. At first he complained of pain in the buttocks and calf, which were supposed to be sciatica, and the tumour was not noticed till just before his admission. It then extended from the right iliac fossa upwards as far as the rib margin and back to the spine, filling the right lumbar region. It was hard and immobile, fixed firmly to the innominate bone and to the lumbar spine. At first the pain was so severe that morphia was required, even when he lay in bed. Operation being hopeless, treatment by Coley's fluid was undertaken. When exhibited in February, 1910, his condition was much improved, pain was absent, and the swelling had decreased in size. Since that date several courses of Coley's fluid have been administered, and the swelling has gradually shrunk, the patient being able to get about and complaining of no pain. Nothing has been undertaken in the shape of treatment for six or eight months.

DISCUSSION.

Mr. CARLESS added that sufficient reaction was produced by comparatively small doses, and although at times there was no pyrexia or rigor the injections made the patient feel very ill. A skiagram indicated that the mass sprang from the iliac fossa and projected upwards along the side of the spine. It was absolutely fixed and very hard. The treatment had proved so successful that doubts as to the correctness of the diagnosis had presented themselves frequently, and yet the man was so ill when first he appeared and the picture so characteristic that no one could then have questioned its nature, especially since puncture, frequently repeated, only produced a blood-stained fluid.

Mr. KELLOCK asked upon what grounds, except the clinical evidence, the condition was believed to be sarcoma. The cases which were reported as being benefited by Coley's fluid seemed to be almost invariably those hidden at a depth. For a visible fungating sarcoma the fluid did not seem to do much good. If there was any doubt about the nature of this growth it would be a

[1] See *Proceedings*, 1910, iii, p. 113.

pity to attribute too much to Coley's fluid, for one might get a false impression of the value of this remedy. A patient was in the cancer wards of Middlesex Hospital for twenty-four years with what was originally diagnosed as sarcoma of the ilium. She died of an intercurrent disease, and the tumour was found to be almost entirely cartilaginous. Possibly in this present case the tumour might be a chondro-sarcoma, which was a very slow growing variety, although it felt too soft for such a diagnosis; in fact, it was almost fluctuating at one spot in front. If the man would permit it, it would be interesting to examine a small portion microscopically before concluding how much was due to Coley's fluid and how much to natural arrest of the growth.

Mr. W. H. BATTLE expressed the hope that the Section might be informed of the ultimate progress of the case, and its final ending. His experience of Coley's fluid, which extended over many years, had not been satisfactory, although he had tried some varieties of it, each of which was said to be better than the last. Years ago he showed a man at the Medical Society who had a tumour in his chest, which microscopically was said to be a spindle-celled sarcoma. It did not improve under iodide of potassium, and he put the patient upon Coley's fluid, and the tumour disappeared. Sections of the growth were made, and a committee which investigated them concluded that there was not sufficient proof that it was sarcomatous, but they thought that it was inflammatory in character. Not one of the cases of sarcomata, either spindle-celled or round-celled, had shown any satisfactory improvement from Coley's fluid. Moreover, patients soon got tired of it owing to the amount of illness it produced. He therefore began to take a very unfavourable view of the fluid. He had had a letter from a doctor at Kensington telling him of a wonderful case which had been under his observation, a patient with a large growth of the ilium. She went to America, and the tumour disappeared under Dr. Coley's treatment. He (Mr. Battle) heard from the family that not long after the patient returned to London the growth came on again and proved fatal in a few weeks.

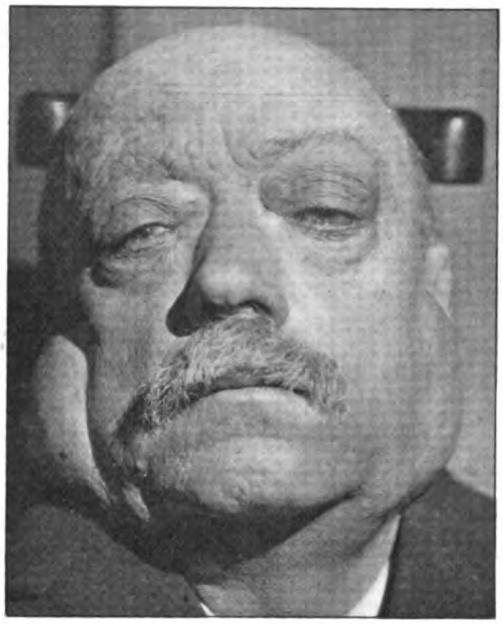
Dr. POYNTON wished to know if, supposing Coley's fluid did produce an arrest of the growth of a tumour, the pathologist would be able to decide upon its nature, on examination after the tumour had ceased to be active.

Mr. CARLESS, in reply, said he quite realized the possibility of doubt as to diagnosis, since the growth had not been examined microscopically. He could not accept the idea that it was a slowly growing chondro-sarcoma. The man was so very ill on admission, and the case had been progressing so rapidly, that it did not at all accord with the clinical picture of chronic chondro-sarcoma. The ultimate result, if obtainable, should be presented to the Section.

A Case of Mikulicz's Disease.

By W. H. Battle, F.R.C.S.

The patient, aged 61, complaining of the change in his appearance produced by swellings in the cheeks, and of watering of the eyes, was under my care in St. Thomas's Hospital, having been sent to me by



Case of Mikulicz's disease.

Dr. Bowen Williams, from July 20 to August 9, 1910. At that time there was a history of swellings on the sides of the face which began about the same time ten months before and had increased slowly in size, without any pain. There was nothing abnormal about the condition of the mouth, and certainly no xerostomia, or blocking of the salivary ducts.

The swellings were quite symmetrical, firm, ovoid in shape, and not attached to the deeper structures or to the skin. Situated in front of the ears, they measured about 3 in. from above downwards and 2 in. from before backwards, appeared lobulated, and were without tenderness.

The submaxillary glands could be felt. The urine was normal, and the patient presented no other evidence of disease.

Regarding the case as one of chronic parotitis, I requested the Director of the Clinical Laboratory to examine a piece of one of these glands microscopically, and for the presence of bacteria. The report returned was that the structure of the gland was that of a round-celled sarcoma invading the parotid gland, also that no culture could be grown from the specimen. When he left the hospital the submaxillary glands were rather larger than on admission. Coley's fluid was tried and continued for some time after he left the hospital, without apparent benefit.

A fortnight ago he came to see me again, and now complained of watering of the eyes which caused him to use his handkerchief constantly. There was a distinct change in his appearance; the swellings in the parotid and submaxillary regions were somewhat larger, and the appearance of the eyes was different, both orbits appearing full, the lids too prominent, and the left eye pushed forward. Both orbits appeared full of growth of some kind; this was lobulated (coarsely lobulated) and tending to overlap the globe, making it difficult for him to lift the left upper lid. The eyeball seemed to be almost surrounded by this, more so on the left than on the right side. About the right inner canthus the growth was less movable than in other parts. Mr. Lawford kindly examined him for me and wrote: "The man —— has no ophthalmoscopic evidence of involvement of his optic nerves. He has some choroidal changes in each eye in the macular region, but they are of a type which is quite common in elderly people, and which I think can hardly be directly connected with the disease from which he is suffering. Although there is marked proptosis of the left eye, I do not think the movements of that eye are interfered with, and there have not been any subjective symptoms of this, such as diplopia."

He had an attack of malaria seven years ago, and after leaving the hospital in 1910, an acute inflammatory attack which affected the right side of the scalp and caused a certain amount of scarring of the scalp. He is under the impression that this was erysipelas, but the fact that it was definitely limited to the right side of the middle line, &c., suggests the possibility of an attack of herpes. He was being treated with Coley's fluid at the time. The spleen can be felt, and the lymphatic glands in groins and axillæ. He has not discontinued his work, and feels quite well excepting for the weeping of the eyes and the knowledge of his strange appearance.

To this a further note must be added. March 2, 1912: He was unable to attend on the last Clinical evening, owing to an attack of acute pain in the jaw, for which a number of teeth were extracted. He was very ill, and looks somewhat cachectic now. He has had three applications of X-rays to the right parotid, and it is much smaller. He is also taking arsenic.[1]

DISCUSSION.

The CHAIRMAN (Mr. W. G. Spencer) mentioned that Mikulicz's first case was described as a lympho-sarcoma from examination of a small piece. The patient, however, had later acute peritonitis and, after nine days, died. Meantime the glands shrank so that they could hardly be found post mortem. Mr. Snell, the ophthalmic surgeon, described an earlier case than Mikulicz's, but that was of the opposite kind—malignant disease of the parotids and lachrymal glands ending in fungation. There had been other cases which arose from chronic conjunctivitis. In some there was irritation from working in a cigar manufactory. The chronic conjunctivitis started inflammation of the lachrymal glands which spread to the parotids and submaxillary. Physicians had reported that in mumps these sets of glands were simultaneously involved, as also in leukæmia. Some cases seemed to have cleared up on arsenic, or iron and iodides.

Dr. F. PARKES WEBER remarked that at present there was symmetrical enlargement of both the inguinal and the axillary lymphatic glands, and he asked whether that enlargement was present when last Mr. Battle examined the case. As those glands, together with the parotid submaxillary and lachrymal glands, were enlarged on both sides of the body, and as the spleen could be felt, it seemed that the condition was almost certainly one of lymphatic leukæmia, although the blood had not yet been examined to confirm the diagnosis. Mikulicz's disease was after all only a symptom-complex, like so many other so-called "diseases." Examination of the blood was necessary in all such cases for establishing a diagnosis as to the exact nature of the glandular enlargement. The glandular swellings in the present case were more likely to be due to leukæmia than Hodgkin's disease (lymphadenoma), because in Hodgkin's disease (at least during the early stages) one set of glands was almost invariably much more enlarged than the others.

[1] We are indebted to Dr. Bowen Williams for a further report on this patient: After he was shown to the Members of the Society "he became very anæmic, with dyspnœa on exertion. There was cardiac weakness, feebleness of sounds, but no murmurs or signs of dilatation. The parotid swellings became very much diminished in size, and the right one had almost disappeared. On April 6 he felt very ill, and had a temperature of 102° F.; this persisted (with one exception when it was normal) until his death on April 14. Death was preceded by coma for some hours and was rather sudden at the last, probably due to some embolus lodging in the brain." There was no necropsy.

Dr. HERBERT FRENCH asked, as there was a well-known relationship between the parotid glands and the pancreas, whether there had been a tendency to glycosuria in this patient, either as the result of normal diet or in connexion with any special diets.

Mr. BATTLE, in reply, said the only information which was supplied about the urine was that it was normal. With regard to examination of the blood, he sent the patient to the clinical laboratory for that purpose, but they reported that the patient would not part with sufficient blood to make a proper examination. The condition the patient now had was recognized as a late stage of Mikulicz's disease; he supposed it was practically splenic leukæmia. But the main question was what it was best to do with the patient. No doubt X-rays were satisfactory, as that treatment had caused diminution in the swellings in the face, but whether the same could be applied to some of the other enlargements was another matter. He thought medical measures would have to be relied upon there.

Rupture of Extensor Tendon of Terminal Phalanx of Finger.

By WILFRED TROTTER, M.S.

A. K., MALE, aged 14. Patient first attended at hospital on February 26. Five weeks earlier, in attempting to hold a piano which was falling over, the patient had had his hand caught between a heavy weight and the ground. The finger was bruised and the patient noticed that he could not extend the terminal phalanx. The finger was fixed on a straight palmar splint for some weeks before the patient's first attendance at the hospital. No improvement whatever had occurred, and the case showed the three physical signs characteristic of the lesion —viz., drooping of the terminal phalanx and inability to extend it, swelling over the dorsum of the second phalanx, and *hyperextension of the joint between first and second phalanges.* The last-named sign was unusually well marked. The case is shown to illustrate this feature, which makes it clear that if the injury is treated with the finger in the straight position the gap in the tendon is widened rather than narrowed. It is obvious from a consideration of the anatomy of the extensor tendon that in order to promote union of a rupture at the terminal joint, the finger must be put up with the middle joint strongly flexed. This view is confirmed by the results of treatment by this method.

Rupture of Extensor Tendon of Terminal Phalanx of Finger. Treatment by Fixation, with Middle Joint Flexed; Cure.

By WILFRED TROTTER, M.S.

F. B., FEMALE, aged 7. Patient first attended as out-patient at hospital on January 22. Some weeks earlier she had struck the end of her finger against a wall. Since then the last joint had drooped. Fixation in straight position had been tried without success. There was complete inability to extend the terminal phalanx, some thickening over second phalanx, and hyperextension of middle joint. The finger was fixed with the middle joint flexed, and in four weeks complete recovery of power of extension had occurred. The case is shown merely to demonstrate the efficiency of this treatment.

A Case of Hyperostosis Cranii.

By E. B. WAGGETT, M.B., and EDWARD D. DAVIS.

A NEWSPAPER-SELLER, aged 22, complained of swellings of the face and of nasal obstruction. He first noticed the swellings at about the age of 16, one month after receiving "a punch on the nose." He has never been abroad, and none of his family are similarly affected.

He has been given a mixture of mercury and potassium iodide for six weeks, and no change in his condition has been noted. The patient is of poor physique, but well nourished, and has well-marked symmetrical osseous swellings involving the nasal processes of the maxillæ, and extending on to the facial surfaces of the bodies of the maxillæ. The maxillary antra are opaque to transillumination, but the frontal sinuses are normal. The nasal bones are unaffected, but both sides of the nose are obstructed by the hard osseous swellings, which can both be felt and seen within the nostrils. The infra-orbital margins are involved and there is lachrymal obstruction on the right side, but in other respects the orbits are apparently normal. The optic disks are normal. In

addition, there is a diffuse smooth swelling of the body of the mandible, to the right of the mental eminence, and surrounding the mental foramen. The teeth are carious, and there is considerable oral sepsis. The left ear is normal, but he has had otitis media on the right. There are neither signs nor history of syphilis, but the Wassermann reaction is positive.

Skiagrams show both maxillæ occupied by dense masses of bone. The rest of the skeleton is normal.

DISCUSSION.

Mr. KELLOCK referred to the thickening on the lower jaw, and thought the three conditions were part of a general disease. The symmetry of the two swellings on either side of the nose was so marked in this patient, and so like that seen in negroes, that it was difficult to account for the swelling on the lower jaw. It might possibly be syphilitic osteitis, and he thought it would be wise at any rate to try the effect of a course of treatment with iodide of potassium.

The CHAIRMAN said he had a case in a farm-boy, and in him there was no question of syphilis. He was begged to do something for the patient, so as to give him a chance of working and breathing through his nose. He used a burr and gouge, like an auger, and bored through to the nasopharynx. He left him with a good channel. He went back to his farm work, and a report was received from the doctor some years afterwards that he continued able to breathe through his nose. But, as Mr. Kellock said, the present case might be proceeding on to leontiasis ossea.

A Family with Membranous Discharge from the Nose.

By A. M. GOSSAGE, M.D.

FIBRINOUS rhinitis is not very uncommon in children. According to Lambert Lack [3] it is commonest in early childhood and is ushered in by slight malaise which is not sufficient to cause the child to lie up and only lasts for a day or two. There is a nasal discharge associated with fibrinous or membranous exudation on the nasal mucous membrane which persists for six to eight weeks and then clears up, leaving no

sequelæ. In nearly all the cases bacilli indistinguishable from the Klebs-Loeffler bacillus can be found, but although the disease is infectious it does not seem to give rise to true clinical diphtheria. True diphtheria, of course, invades the nostrils not infrequently, and gives rise to a severe illness with formation of membrane in the nose.

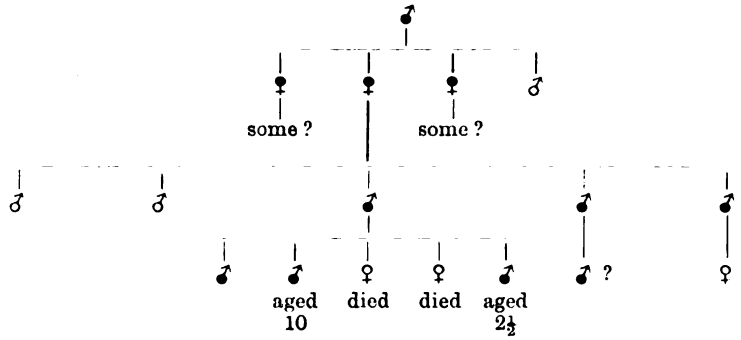
A further type of membranous formation in the nose has been described by Baumgarten [1]. A strong baby girl was noticed from birth to have crusts about the nose. With a probe and wool a long thin tube, like parchment, could be obtained from the nostrils which took three to four days to re-form. At the age of 3 years a bad smell was noticed, and after 4 years typical ozæna developed. When the child was 2 months old the nose was washed out with iodoglycerine and after this no more tubes were formed, but occasionally there were slight crusts: later even these disappeared. The mother suffered from ozæna, but there was no other abnormality in the family: twin sister was quite normal. Baumgarten further states that he has met with two other similar cases: tube formation in early infancy and development of ozæna about the fifth year of life. He considers that ozæna is frequently inherited chiefly from mother to daughter, and is much commoner in women.

I have recently come across a remarkable family, several members of which have a persistent membranous discharge from the nose. The condition is first noticed at birth and apparently persists throughout life. It causes no impairment of health; the bronchitis from which the first member I saw was suffering being probably an accidental concomitant. There is only very slight discomfort, though there is a tendency to the development of a bad smell if the nostrils are not kept clear. In no case, however, is there any sign of ozæna. Specimens of the discharge, which usually took about twelve hours to re-form, were obtained from the father and two of the children, and in all cases were found to be a more or less complete fibrinous cast of the nostrils. Dr. Ross, the pathologist at the East London Hospital for Children, kindly examined the discharge for me and reported as follows: Film preparations showed polymorphonuclear cells embedded in a network of fibrinous matter. The organisms present were bacilli, diplococci and short diplobacilli; no evidence of *Bacillus diphtheriæ* or *Bacillus xerosis*. Attempts to embed in paraffin and cut sections failed owing to the friability of the material.

One of the boys was taken into the hospital for twenty-four hours but, unfortunately, had no discharge during that time. The nostrils

were examined on several occasions and nothing abnormal could be seen except once when some fibrinous exudation was found over one lower turbinate bone.

In the appended genealogical tree the affected individuals are black and the normal white. It can be seen that at least four generations



have been affected and that males and females are attacked equally. Of the children of affected persons with normal mates roughly half are affected and half normal. This suggests that the inheritance affords another example of Mendelism in human beings and that the abnormal condition is dominant to the normal [2]. Unfortunately, there is no record of any offspring from the normal members of the family, so that it is impossible to say whether all their children are normal, as would be expected.

It is a matter of great interest to find a fibrinous exudation on a mucous membrane resulting from a congenital abnormality and not from some infective inflammatory process. I have not been able to find any record of a similar condition in medical literature.

REFERENCES.

[1] Baumgarten. *Archiv. f. Laryngol. u. Rhinol.*, 1909, xxii, p. 492.
[2] Gossage. *Quart. Journ. Med.*, 1908, i, p. 331.
[3] Lambert Lack. *Med.-Chir. Trans.*, 1899, lxxxii, p. 1.

DISCUSSION.

Dr. GALLOWAY remarked on the possible relationship of plastic or fibrinous bronchitis to the form of disease described by Dr. Gossage.

Dr. HERBERT FRENCH asked whether any chemical analysis of the material had been made to see if it contained any unusual metabolic product.

Dr. GOSSAGE replied that only microscopical examination had been made.

Note on a Case of Ruptured Intestine, with Especial Reference to the Mode of Production of the Lesion.

By WILFRED TROTTER, M.S.

THE case to which I propose to call your attention is one in which the diagnosis, treatment, and result were so strictly ordinary that I should not venture to bring it before you if it did not appear to throw some light upon a subject about which there has been some controversy —viz., the mechanics of the production of traumatic rupture of the intestine.

The patient, a youth, aged 18, was admitted to the University College Hospital during the evening of August 30, 1910. Some hours earlier, in the course of his work as a navvy, he was caught between the buffers of two ballast wagons and severely crushed. The wagons being a good deal lower than ordinary railway trucks the buffers caught him over the lower half of the abdomen. He was held by them for a few moments under severe pressure.

At the time of admission to the hospital he complained of considerable abdominal pain and of tenderness over the left lower part of the abdominal wall, where a hæmatoma had already begun to form. The pulse was 80 per minute and of good quality. There were no signs of free fluid in the abdomen, the urine was normal, and there was no vomiting. Such pain and tenderness as he had seemed clearly to be associated with the bruising of the abdominal wall. It was already some hours after the accident, and the absence of symptoms was so complete that the house surgeon did not think it necessary for me to see the patient until the morning. Instructions were given that the pulse-rate should be taken and recorded every hour during the night.

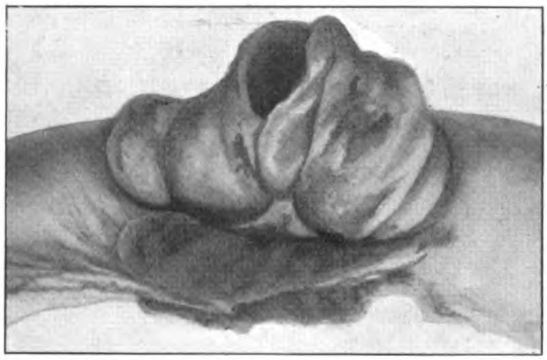
The next morning the diagnosis of an acute intra-abdominal lesion was quite clear. The pulse had risen steadily to over 100, the patient had vomited, and the tongue was becoming dry. The abdomen was rigid, slightly retracted, and respiratory movements were visible only in the upper part. Operation was undertaken somewhat less than twenty-four hours after the accident. A median incision was made below the umbilicus. The intestinal coils in the lower half of the abdomen were found to be inflamed and somewhat matted together. Amongst them was turbid fluid mixed with a considerable quantity of fæces. Separation of the coils revealed a segment of ileum lying to the left of the spine against the posterior abdominal wall and showing an opening half an inch across, which was discharging semisolid fæces of a green colour.

Examination of the damaged loop showed at once that suture was not possible. Enterectomy was therefore done and the bowel united end to end. The pus and fæces were mopped out of the abdominal cavity and a drainage-tube put into the pelvis. Convalescence was uneventful except for a slowly forming pleural effusion which had to be tapped a fortnight after the operation. The wound had to be drained for several weeks.

It is the form of the lesion in the bowel which constitutes my excuse for bringing the case before you. The features of the injury can be well seen in the specimen which preserves most of the appearances visible at the operation and in the drawing very kindly made for me by Mr. Lawrence (*see* figure). For a distance of about 2 in. the peritoneum of the bowel has been torn through and has shrunk back. Through this opening the tube of mucous membrane has protruded after being torn away from the outer coats. In the most prominent part of the prolapsed mucous membrane is a round opening, the size of a sixpence. The appearances seem to me to show that the lesion was produced by a true bursting of the intestine.

Traumatic rupture of the intestine has been attributed, and no doubt is due to the action of one or other of three different mechanisms —viz., crushing, laceration, and bursting. There is no doubt that one or other of the two former causes the lesion of the bowel in many cases. Upon the third, however, a good deal of doubt has been thrown, and some authorities are inclined to question whether it ever is a cause of rupture. Experiments have shown that when the abdomen is compressed the pressure within the bowel, as might have been expected, is not increased above the general intra-abdominal pressure, and that

therefore there is no tendency for bursting of the bowel to occur. When, however, a loop of bowel is not supported by the general intra-abdominal pressure—when, that is to say, it lies at a hernial opening or within a hernial sac—rupture is likely to occur if the abdomen is violently compressed. It has been shown comparatively recently that the influence of a hernia in predisposing to the occurrence of rupture is an important one. While admitting this, one is not prepared to agree that bursting can occur only in presence of a hernia. None such was discoverable in my case. It seems, however, that bursting might be brought about in another way. If a loop of bowel were caught between the compressing force and the spine in such a way that both limbs of the



Case of ruptured intestine.

loop were occluded, the contents of the latter might be put under such pressure that bursting occurred. Some such mechanism as this appears to me to be the explanation of the state of affairs found in this specimen.

DISCUSSION.

Dr. HERBERT FRENCH said he took it that Mr. Trotter meant by "rupture" here that the aperture through the mucous membrane into the peritoneal cavity was produced actually at the time of the buffer accident. He thought, however, that precisely similar appearances might have resulted if the injury had led first to the formation of a hæmatoma of the intestinal wall, the hole itself not occurring at that time, but only later as the result of the progressive softening and digestion of the most injured part. It was possible that the actual rupture did not take place until three or four hours after the accident; at least it would be difficult to disprove that view either from the clinical history or from the appearances of the specimen.

Mr. C. H. FAGGE said he had no valid argument against Mr. Trotter's view as to the method by which this intestinal rupture had been brought about, except that it seemed to demand simultaneous and complete closure of both ends of the intestinal loop; as an alternative he had wondered whether hæmorrhage into the mesenteric border might be regarded as the primary lesion by which the peritoneum at the antimesenteric border was stretched and finally split, as it will often do in the forcible reduction of an intussusception. If this were admitted, it would be easy to account for the hole in the mucosa by supposing that it had been lacerated between the buffer and the spine.

Clinical Section.[1]

March 20, 1912.

Dr. SAMUEL WEST, Vice-President of the Section, in the Chair.

Observations of the Movements of the Heart by means of Electrocardiograms.

Introduced by W. EINTHOVEN, M.D. (Leyden).

PROFESSOR EINTHOVEN said he would like to discuss the question whether it was possible to judge of the strength of a heart contraction by means of the electrocardiogram.

The muscle contraction is quite a different thing from the action current of the muscle; the former is a mechanical act, a shortening of the muscle-fibres, whereas the action current is an electrical phenomenon. The phenomena are connected with each other, but, generally speaking, it is not feasible to measure the strength of a contraction by the strength of the action current produced. Nevertheless, we may in many cases obtain valuable information as to the strength of the systole by means of the electrocardiogram.

The magnitude of the pulse is often a misleading measure of the strength of the ventricular systole, for the pulse is influenced not only by the strength of the ventricular contraction, but also by the blood-pressure in the aorta and by the amount of blood present in the ventricular chambers of the heart at the moment the systole begins. On the other hand, if one examines the same person under similar conditions and observes the production of two electrocardiograms, the form as well as the absolute dimensions of which are fully equal, one has no reason

[1] Special meeting of the Section, held at the University College Hospital Medical School.

to suppose that the systole which produces the first curve is different from the systole which produces the second one.

So if one observes two different pulses corresponding to two equal electrocardiograms one may generally conclude that the heart's action has been equal, and that a change in the other conditions of the circulation has taken place: either the arterial blood-pressure or the blood-content of the heart, or both conditions together, must have changed.

Dr. Einthoven showed a number of electrocardiograms to support this assertion. In the first series of curves he showed some atypical premature electrocardiograms. They all had practically the same form and dimensions, but they corresponded to a large arterial pulse when they were but little premature, whereas they corresponded to no pulse whatever, or to a negative one, when they came much too early, due to the fact that there had been no time to fill the ventricular chambers with blood from the auricles.

The marked influence which a preceding auricular contraction exerts upon the magnitude of the pulse was shown in a second series of curves. With a preceding auricular contraction the pulse was large, without it the pulse was much smaller. The electrocardiogram gave evidence of the fact that the ventricular contraction was equal in both cases.

A third series of photograms dealt with the continuously irregular pulse of auricular fibrillation. Here again the electrocardiogram proved that the differences in the magnitude of the pulse were not caused by variations in the ventricular action, but by the irregularities of the auricles.

Dr. Einthoven next discussed the causes of pulsus alternans and of bigeminy. By means of the electrocardiogram it could be shown that the changes in the pulses were mainly due to changes in blood-pressure, to changes in the duration of the diastole, and to a weakened heart muscle in general, whereas the strength of the heart contractions did not, or practically did not, alternate.

Dr. Einthoven then demonstrated five curves from a remarkable patient, who had an injury of the right branch of the auriculo-ventricular bundle, combined with auricular fibrillation. In paroxysms of tachycardia the patient had 250 heart-beats per minute. At the same time the pulse-rate was comparatively infrequent. Many of the heart-beats, as shown by the electrocardiogram, did not produce a pulse. The absence of the pulse was to be explained in this case not so much by a decrease in the strength of the ventricular systole as by the lack of filling of the ventricles with blood.

In conclusion, four photograms were shown exhibiting the results of a series of experiments on dogs. Four curves were displayed on each photogram: (1) The intraventricular pressure curve; (2) the sphygmogram of the femoral artery; (3) the electrocardiogram, and (4) the venous pulse with the well-known *a*, *c* and *v* waves of Mackenzie. By means of aconitine a pulsus alternans was produced in the animal. The sphygmogram of the femoral artery showed the alternation very strikingly, so that every second pulse almost entirely disappeared. Nevertheless, the electrocardiograms retained about the same form and dimensions. The intraventricular pressure curve also gave evidence of about equal force in the ventricular contractions, and thus was in accordance with the electrocardiogram. A slight difference in the intraventricular pressure was obviously sufficient to produce a great difference in the magnitude of the pulse. If in the first heart-beat the pressure in the left ventricle was a little higher than the pressure in the aorta, the pulse was large; if in the second heart-beat the intraventricular pressure was only a very little lower than the blood-pressure, there was no pulse at all produced.

The last photogram showed premature heart contractions in the dog. The intraventricular pressure curve was no longer in accordance with the electrocardiograms. If the heart-beat was much too early the intraventricular pressure curve showed hardly any rise, whereas the electrocardiographic curve showed large peaks. Dr. Einthoven explained this phenomenon by pointing out that there is a difference between the relaxation and the lengthening of a muscle-fibre. When an ordinary systole has finished the fibres of the heart musculature relax, and the intraventricular pressure is reduced to zero. But before there has been time to permit of perceptible lengthening of the fibres after their relaxation the second heart-beat sets in. If the same maximum of shortening of the fibres is now not reached as in the first systole, there cannot be produced a new wave in the intraventricular pressure curve. Under these special conditions the force produced by the heart contraction is even better judged of by means of the electrocardiogram than by means of the intraventricular pressure curve.

Many of the curves shown were recorded by his pupils.

Dr. FLORENCE BUCHANAN dealt with the experimental aspect of the subject and showed, by means of electrocardiograms, the dissociation of the auricles and ventricles in hibernating animals. The instrument with which they had been taken differed from that of

Professor Einthoven; it was a capillary electrometer, and what was photographed was the movement of the meniscus of a column of mercury in a capillary tube containing dilute sulphuric acid. A human electrocardiogram taken with this instrument was first shown for comparison with those taken with Professor Einthoven's more delicate and more sensitive instrument. The investigations referred to dormice and rats, but electrocardiograms of dormice only would be shown.

The first was that of a dormouse awake and warm, taken on a plate moving at the same rate as the last one. It showed that the frequency of the ventricular beat was 700 a minute. The next was one taken half an hour earlier with the same animal when it was asleep and cold, showing that the frequency of the ventricular beats was 104 per minute. A series of smaller effects, the auricular effects, were also seen. Their frequency was 114 per minute, quicker than the ventricular. When the animal was awake and warm the auricular effects, when visible, always had the same frequency as the ventricular. Those that occur with an independent frequency when the animal is cold do not correspond to ordinary auricular systoles, since the electrical counterparts of these may occur side by side with them, both late in the season and early in the season of hibernation. The auricular effects, which correspond to Professor Einthoven's "P," Dr. Buchanan called *a* effects, and those of the independent series, which were those she would speak about chiefly and which were much longer lasting effects, she called *b* effects. They represented extra-auricular beats. The next photograph shown was taken with another dormouse in the same stage of hibernation as the last, but on a much slower rate of plate. A metronome had been used to cut off the light once a second. It inscribed vertical lines on the plate. The auricular effects had a frequency of about 110, the ventricular effects one of about 90 a minute. Next was a record taken with the same dormouse about an hour earlier, showing that the auricular effects were then hardly faster than the ventricular, the ventricular themselves being about the same. The next record, taken fifteen minutes earlier, showed that the auricles were then beating more slowly than the ventricles, the frequency of which was about 70 a minute.

A series of records, taken with another dormouse, at 2° C., were then shown in the order in which they were taken. The first was when it was in a condition of extreme torpor and very cold. The ventricles were beating 35 times per minute, the auricles not at all.

When an animal was hibernating most completely no respiratory movements could be detected, often for several minutes at a time, but every now and then the animal took a few breaths, and then went back into a condition of apnœa. Whenever it was in complete apnœa no auricular *b* effects were ever to be seen. Here and there, however, there were indications between two ventricular effects of a large number of quick auricular effects like those seen in cases of auricular fibrillation in man.

The next records, taken with the same dormouse at intervals of about five minutes, showed that the auricular *b* effects appeared when the breathing began. They were of longer duration than the ventricular effects, but of slower frequency and less regular. The animal was waking up quickly in spite of the low temperature of the room. The frequencies of both auricles and ventricles increased, but in this dormouse, on this occasion, the rate of the auricular beat was still behind the ventricular when this had reached 300 a minute. Usually the auricles had overtaken the ventricles before the rate of the latter became 100 a minute. While being overtaken the ventricular beats were often dropped. The extra-auricular systoles appear, therefore, to be dependent on some condition of the blood, which also affects the respiratory centre. While the animal is waking Cheyne-Stokes respiration generally occurs. When this happens the auricular *b* effects disappear immediately before each period of apnœa, and reappear immediately before each period of breathing. This was illustrated by three records, taken with different dormice.

Another record taken at the end of March, near the conclusion of the hibernating period, showed the *a* auricular effects as well as the *b* ones. These did not cease, as the *b* effects did, when the breathing was suspended. Towards the end of the season the block, which had apparently been complete before, seemed to be giving way, but the ordinary auricular systoles, when they appeared, took on the frequency of the ventricles and did not set a new pace. Their electrical counterparts, the *a* effects, sometimes occurred almost at the same moment as the *b* effects. In such case there would be but one mechanical effect. It is probable that the extra-auricular effects cease to occur as soon as the ordinary auricular effects have become thoroughly established. Records taken with the string galvanometer would serve to elucidate this matter better than those taken with the capillary electrometer.

Dr. JAMES MACKENZIE said that it was essential to the physician who would treat his patient intelligently to make a diagnosis precise and definite. The introduction of the electrocardiograph had undoubtedly been of the greatest assistance in achieving this object, and he personally felt greatly indebted to Professor Einthoven for giving us this valuable method of diagnosis.

It was in 1906 that he first read the description of the electrocardiograph, and he perceived how valuable it would be in carrying out an investigation into the abnormal action of the heart. He had been able to clear up a good many obscurities by means of graphic records, but was continually meeting with cases which he failed to understand.

The great group of heart irregularities which we now recognized as being due to auricular fibrillation interested him very much. He had, indeed, recognized and demonstrated that the normal auricular activity had ceased, although at that time few clinicians accepted his description. He tried to get records of the irregular hearts in which he had observed this cessation of auricular activity, without success in this country. Eventually Dr. Lewis began his investigations, and when electrocardiograms of these cases were taken a complete corroboration of his interpretation of the tracings was obtained and established, in so far as the electrocardiograph showed that the auricle was not acting in its normal manner.

He was baffled in his attempts to solve another group of cases. These were cases of tachycardias, persistent and paroxysmal. He could recognize a few, but it was evident from the tracings he obtained that there were different forms. Thus, in some cases the pulse-rate was persistently over 130 beats per minute. In one case the rate of the ventricle occasionally attained a speed of nearly 300 beats per minute. An electrocardiogram showed that in such cases the auricle was beating at a rate of from 250 to 300 per minute, and that usually the ventricle responded only to every second beat of the auricle. He had never suspected such a condition of affairs.

In pursuing, with his colleagues, an inquiry into the action of digitalis and into the treatment of heart affections generally, they had been greatly assisted in the interpretation of many obscure phenomena by Dr. Lewis taking and interpreting electrocardiograms, and by this and other means they had been able to obtain results of such accuracy as hitherto had not been possible.

Dr. THOMAS LEWIS said that if he might speak for a moment from the point of view of the School, he would like to express how cordially University College Hospital Medical School welcomed Professor Einthoven, and how much the meeting appreciated his splendid demonstration. The School was proud to entertain such a man, a worker whose name was for ever stamped in the annals of clinical medicine. The advances in our knowledge came, as a rule, through the invention and introduction of new methods. It was given to few men to make these discoveries; it had been given to Einthoven. Such men were incalculable benefactors of the human race. The Professor had given the profession a perfected instrument, and a method of unrivalled precision. But he wished to speak more particularly from the point of view of electrocardiographic work, especially on the hearts affected by various disorders of rhythm. It would take too long to mention in detail the various ways in which the method had been of assistance, and he would confine himself to the chief directions in which its benefits had been felt by his fellow-workers and himself. In the first place, he would put the corroboration of the results obtained by another new method, that of Dr. James Mackenzie —namely, the analysis of the venous pulse curves. He wished to lay special emphasis on the corroboration which the electrocardiograph had given of the venous pulse records, because the polygraph was an instrument capable of use by all practitioners. The chapter of the analysis of cardiac irregularities was fast closing. The electrocardiograph gave an immediate and decisive answer to the question as to the nature of a given irregularity in all but exceptional instances. He wished to illustrate that statement by exhibiting one or two curves.

The first slide showed an electrocardiogram from a patient whose heart beat irregularly. One saw the normal beats consisting of the usual peaks, termed by Professor Einthoven *p*, *q*, *r*, *s*, *t*. Those curves, normal cycles following upon each other regularly, were here and there interrupted by what the Professor termed atypical curves, corresponding to premature contractions. Those beats were very striking in electrocardiograms, and many had been shown that evening. We knew from venous pulse work that such beats might arise in auricle or ventricle, and the galvanometer had taken us a step further, as it told approximately the point from which such beats arose, not only that they arose from the ventricle, but in which part of it. Another disorder of the heart's action which was beautifully illustrated by the electrocardiographic method was one in which there were disturbances of conduction

in the passage of impulses from the auricle to the ventricle. The slide was from a case of acute infection, and it would be seen that there was a regular heart-beat, consisting of auricular contraction and ventricular contraction of the usual type. But there was an increase in the interval between the contraction of the auricle and the contraction of the ventricle, indicating the commencement of a deficiency in the conduction in the strand of tissue to which he had referred. At times there was an absence of response, and the ventricular beat was missed. The next slide was an example of the Stokes-Adams syndrome, due to a complete dissociation of auricle and ventricle. The first curves of that nature were published by Professor Einthoven in his first paper. Often they were very striking. For instance, one saw a very slow action of the ventricle occurring regularly, each ventricular contraction being represented by a peak—namely, *q*, *r*, *s*, and just a small *t*. One could see quite regular peaks running throughout the whole curve, and falling with haphazard relations to the ventricular contractions; they were auricular contractions. These three electrocardiograms were taken from the same patient. The difference in the outlines of the curves was due to the alteration of the leads, those being what the Professor called 1, 2 and 3. Perhaps dissociation was shown most perfectly in the second lead.

He next wished to refer to what the galvanometer had done for them as students of cardiac pathology and physiology—namely, in guiding them as to where the heart-beat started. This work had been largely carried out with Professor Einthoven's galvanometer. The isolation of the site at which the heart-beat started was of considerable importance, because the galvanometer also showed, in pathological cases, that a new impulse formation might arise in other parts of both auricle and ventricle. The galvanometer had split up abnormal contractions, and placed them into groups according to the points of origin of such contractions, and it had shown that the normal point at which the impulse was generated—the sino-auricular node—might be subservient, for more or less long periods, to other centres. He exhibited the curve from a case of paroxysmal tachycardia. The typical electrocardiogram for that case consisted of *p*, *r* and *t* waves, *p* being upright, as usual. One could see a number of beats running faster, which, in their general outline, and so far as the ventricular portion was concerned, had the general shape of the beats of the slower rhythm. But they were characterized by the abnormality of the auricular representative. It showed that a beat had arisen in a different auricular focus. Dr. Mackenzie

had mentioned that it was through the work done with the galvanometer that we now knew clinically of the condition of auricular fibrillation. The next slide showed fibrillation in a dog, and one could see the ventricular peaks occurring at irregular intervals, consisting of sharp peaks *r* and *t*, the latter being buried in the remainder of the curve, which consisted of a number of irregular oscillations. The next was a curve from a patient in whom the heart was affected in a similar manner, and it had occurred spontaneously; in the case of the dog it had resulted from experimental interference. The next slide was from a case recently in the wards of University College Hospital. The patient, when admitted, had a pulse of 150, and for some time the electrocardiograms were obscure, and at this stage it was difficult to understand them. The patient was put on digitalis, and some heart-block resulted, and it was apparent that the auricle was beating four times as fast as the ventricle, the auricles beating at 300 per minute. Those were the cases which Dr. Mackenzie had just alluded to, in which there was an extraordinarily rapid action of the auricles, so that the curves almost resembled those he (the speaker) had shown illustrating fibrillation, but were distinguished from them by the regular incidence of the several auricular beats. One could see the ventricular peaks occurring at regular intervals, each portion of the curve between the ventricular peaks resembling the next and corresponding strip, and the mechanism was constant from ventricular cycle to ventricular cycle. One could show the nature of the auricular activity in a still more striking manner by pressing lightly on the carotid sheath to irritate the vagus, then what was 4 : 1 heart-block before gave place to a prolonged stoppage of the ventricle, the auricular beats continuing to form regular oscillations.

One of the most notable things in Professor Einthoven's communication was his conception that electrocardiograms might give hints as to the relative amount of hypertrophy of the two ventricles. Dr. Lewis showed a curve from a case of mitral stenosis, which could be regarded from that point of view. For the sake of brevity he would speak only of the right ventricle. In right ventricle hypertrophy Professor Einthoven held that in lead 1 a small peak *r* and a deep depression *s*, and in lead 3 a large and exaggerated peak *r* occurred. He desired to bring some corroborative evidence of this conception of the Professor's. He showed a slide of a case of congenital pulmonary stenosis (congenital pulmonary stenosis so far as it is possible to diagnose it). It was a fairly characteristic case of congenital stenosis,

with a thrill over the second left cartilage. The variation in the peaks was shown in a very striking form. With Dr. Clifford White he had recently been examining some newly born children from the same point of view. Relatively, the right ventricle in the new-born child was strong, and it gave the curves which were anticipated from Professor Einthoven's original suggestion. There was a small *r* and deep *s* in lead 1, and in lead 3 *r* was exaggerated. He would have liked to have mentioned the wonders of Professor Einthoven's laboratory, of his unique instrument, beside which their own instrument was a toy. They could not sufficiently thank him for the great gift he had offered, nor emphasize enough their admiration of the patient endeavour which had carried him to the end of the task which he had set himself to perform.

Clinical Section.

May 31, 1912.

Sir Wm. Osler, Bt., F.R.S., President of the Section, in the Chair.

A Case of (?) Acromegaly.

By Rupert Farrant, F.R.C.S.

E. G., aged 21, greengrocer. Past history: He has never been robust, but beyond an attack of jaundice at the age of 10 he has had no illnesses. Family history: He can trace his ancestry back through two or three generations of Dorset peasants. No history of tubercle or syphilis.

History of present illness: In January, 1907, a cart-wheel passed over his right calf; this laid him up in bed for six weeks. Shortly after he found that he was unable to fully bend his hip; this has been gradually getting worse.

Present condition: The skin is pigmented in the axillæ, &c.; there is marked muscular wasting. The knee-jerks are increased and the right plantar reflex is extensor. The blood-pressure is 110. The forehead recedes, the lips and tongue are enlarged, the speech is somewhat blurred. The lower jaw and the malar bones are prominent, to a lesser extent the mastoid processes and the external occipital protuberance. A slight difference is to be noted in his two hands, the right index finger being thicker than the left. The femora are bowed outwards, the necks thickened and bent and the heads enlarged, the articular surfaces extend down on to the neck as shown in the prints. These changes in the femora cause much limitation of movement, and the limbs lie in a position of external rotation—the left 75°, the right 15°. The X-ray print of the skull shows some widening of the pituitary fossa. The eyes are markedly myopic, but there are no changes in the fundi or in the fields of vision. There is no polyuria. The question that enters

my mind is whether this can be a case of early acromegaly combined with changes in the upper ends of the femora. I am indebted to Mr. Tubby for showing this case.

Mr. FARRANT, in reply to the President (Sir William Osler), said that there was no sugar in the urine.

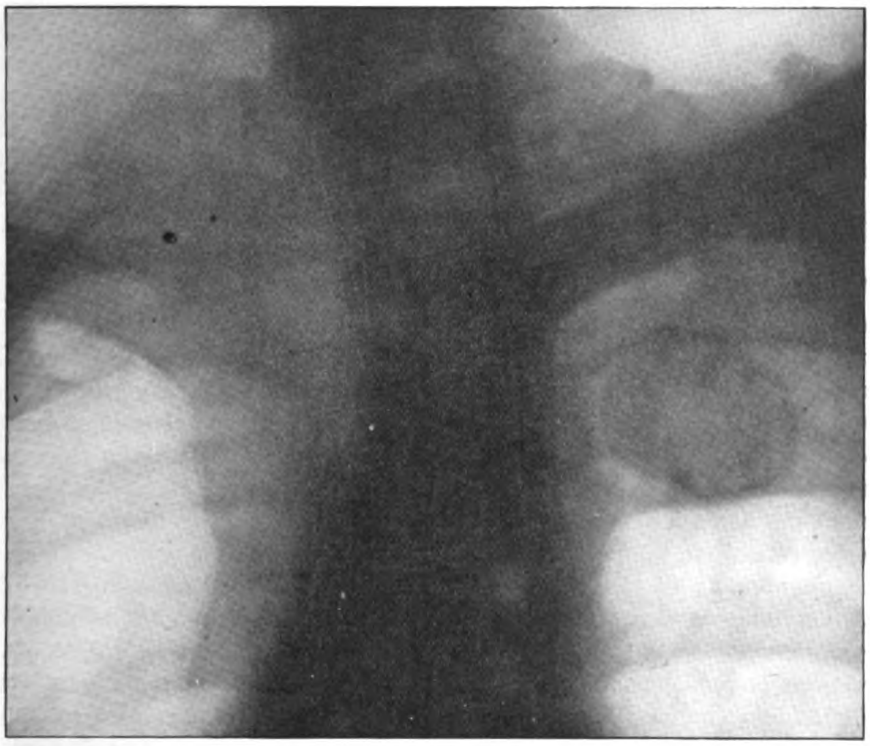
A Case of Thoracic Aneurysm not connected with the Aorta.

By FREDERICK LANGMEAD, M.D.

THE patient is a married woman, aged 55, and the mother of thirteen children, twelve of whom are alive and well. There have been no miscarriages. The first symptoms of disease occurred in September, 1909. While at work at home she suddenly lost the use of her right hand, and in a few minutes the entire right side was paralysed and she could not speak. She did not lose consciousness, however, and during the progress of the paralysis was able to struggle down four flights of stairs. She was taken to the Infirmary, where she stayed for a month. During the first thirteen days she was semi-conscious. The power in the right side and speech gradually recovered, and now on examination there remains only slight rigidity and an extensor plantar response on that side.

She attended the Seamen's Hospital for the first time on June 5, 1911, and complained that for about three months she had been suffering from shortness of breath and pain in the chest and abdomen. On several occasions she has vomited a drachm or so of blood after meals. She has evidently lost a great deal of flesh. Over the back of the thorax, especially on the left side, are several tortuous and pulsating vessels, which communicate with others in the left axilla; they are apparently arteries, and probably represent a secondary circulation. To the left of the second and third dorsal vertebræ is a slightly protruding area, about the size of half-a-crown. It pulsates slightly, and over it a thrill can be felt and a loud systolic bruit heard. A somewhat more extensive area of dullness is obtainable, reaching from the seventh cervical vertebra above to the third dorsal below, and extending to just beyond the spine to the right. The bruit can be heard most loudly opposite second dorsal vertebra on the left side, and is traceable across the spine

and downwards for three or four vertebræ. If the tortuous superficial arteries be compressed the pitch of the bruit heard over the pulsating tumour alters. There is no evidence of obstruction to, or delay of, the flow of blood in the thoracic aorta; the abdominal aorta and femoral arteries pulsate normally. Forcible pulsation occurs in the carotids. The heart is not enlarged, but a definite systolic murmur is heard over the aortic area, and is conducted into the vessels of the neck. No



Thoracic aneurysm not connected with the aorta.

abnormal physical signs can be detected in the lungs, except slightly deficient air entry over the right upper lobe. There is a movable tumour in the abdomen; this was at first situated in the right hypochondrium, but during the last few months has become more mobile, and now can be felt sometimes as low as the hypogastric region. It is probably a movable kidney. Wassermann's reaction is negative.

Skiagrams show a very defined, circumscribed, rounded shadow, situated between the third and fourth ribs, posteriorly near their junction with the vertebral column. It is obviously a structure with

dense walls, and contains some opaque material. The heart is seen to be normal in size and position, and the aorta is normal. The right lung is dimmer than the left, and does not brighten up on deep inspiration. There is some opacity in the region of the root of the right lung; the appearance of the right lung suggests tuberculosis. There is no evidence of growth in the mediastinum.

No history of syphilis is obtainable. Her family and "several members of her father's family" died of consumption.

The case is shown to elicit opinions as to diagnosis. The view suggested is that it is an aneurysm of an intercostal artery, which is dense-walled, and contains a considerable amount of clot. The large superficial vessels probably constitute a secondary circulation.

DISCUSSION.

Dr. LANGMEAD added that he had used the term "thoracic aneurysm" in order to elicit a discussion. He had considered the question of coarctation of the aorta, of which Dr. Parkes Weber thought this was an instance. Still, there were some difficulties to be explained. There was a very opaque shadow on the left side of the chest in the upper part, which stereoscopic examination placed at the back of the thorax, lying against the body of the third dorsal vertebra and to its left, near the costal junction. It was above the aorta, and he was puzzled as to what the shadow represented. The fact that it corresponded roughly to the position of dullness, to an area of deep pulsation and the greatest intensity of the murmur, suggested it might be an aneurysm of the third left intercostal artery. But that would scarcely explain the presence of the very large vessels over the back. Possibly there might be a double lesion —namely, coarctation of the aorta, and secondarily, aneurysm. In reply to the President, he said the abdominal aorta and the femoral artery pulsated well.

Dr. F. PARKES WEBER said he did not know any condition other than coarctation of the aorta in which there was such an extensive collateral circulation by means of superficial arteries on both sides of the back of the thorax. The only way to account for the enlargement of the superficial arteries on both sides of the spine was to assume that they formed part of an elaborate collateral circulation to compensate for stenosis, or possibly complete occlusion of the thoracic aorta just beyond the origin of the left subclavian artery. In some of these cases aneurysms had been found. In a case of a woman, aged 56, whom he saw in 1910 at the Mount Vernon Hospital, there was a small aneurysm on the right side of the neck. After the patient left the hospital this aneurysm enlarged and caused her death by bursting externally. He thought the President, more than anyone else in the

room, could tell the meeting about cases of the kind, because he had studied the condition long ago. In Canada there existed an old account in Sir William Osler's handwriting of the post-mortem findings in such a case,

The PRESIDENT said the case referred to by Dr. Weber was the only one he had ever seen. In the case now shown he thought the only other possibility was the presence of a subcutaneous cirsoid aneurysm. He had had a case of multiple cirsoid aneurysm, one of the groups being on the back, and a very intense systolic murmur was audible over it. It seemed to him that a point against coarctation in this case was the fact the patient had pulsation in her abdominal and femoral arteries.

Dr. PARKES WEBER, in further comment, expressed the opinion that pulsation could probably be felt in the abdominal aorta in cases of coarctation. With regard to cirsoid aneurysm, he did not see how that would account for the enlarged arteries under the skin on both sides of the patient's spinal column.

Relief following Bilateral Nephrotomy and Drainage for Acute Nephritis attended by Suppression of Urine and Uræmic Convulsions.

By W. G. SPENCER, M.S.

AN engineer, aged 36, was admitted to the Westminster Hospital for the radical treatment of a right inguinal bubonocele. The urine was noted as containing a trace of albumin. He gave no history of any previous illness, although later, when pressed, said he had had pains in the loins before.

On March 28, the twentieth day after the operation, he was up and about the ward, preparatory to leaving on the following day. About mid-day he was attacked by a dull aching pain in the loins, scalding micturition, and he vomited twice. The pain was relieved by aspirin and counter-irritation to the loins. On the next day, March 29, he passed 15 oz. of urine and vomited five times. He was made to sweat repeatedly, purged, and kept on a milk diet. On March 30 he passed 12 oz. of urine, on March 31, 10 oz., and on April 1, 8 oz. The urine contained not more than a cloud of albumin, red corpuscles, white corpuscles or pus cells, oxalate crystals and epithelium, but practically no casts. His blood contained 25,300 white blood corpuscles per cubic millimetre, 92 per cent. being polymorphonuclear. He was examined twice by the X-rays without finding any sign of stone in the pelvis or

ureter of either kidney. On April 2, at midday, five days from the commencement of the attack, he had a uræmic convulsion lasting ten minutes. For the previous thirty-six hours no urine had passed into the bladder. At 1 p.m. he had a second uræmic convulsion lasting one and a half minutes. At 2 p.m. he had a third convulsion, and at 2.30 p.m., when the operation was begun, he was still partially unconscious and his breath had a urinous odour.

The left kidney was exposed through a lumbar incision and found about double the normal size, much congested, and its capsule inflamed. Forceps were pushed from the convex border through the very friable kidney substance into the pelvis of the kidney, and this was followed up by one finger. The pelvis and calices were not dilated at all. There was no urine in the pelvis. The palpation of the kidney and the probing of the ureter failed to discover any stone or other cause of obstruction. A drainage-tube was fixed in the kidney pelvis and the wound not sutured. The operation was repeated on the right side, and the observations made were similar to those on the left. There was no excessive hæmorrhage.

After the operation saline infusions, hot-air baths, aperients and a milk diet formed the course of treatment, practically the same as that before the operation. He recovered from the effects of the operation without any complications. Urine began to escape from both drainage-tubes very soon; before the dressings were changed, seventeen hours after the operation, they had to be packed five times. There was no clot under the dressings when removed, and the urine soon became free from blood.

On April 3, 8 oz. of blood-stained urine passed by the bladder; on April 4, 45 oz. were so passed, and this continued for some days, whilst urine escaped freely from the wounds. On April 5 the drainage-tubes were removed and the quantity of urine passed by the bladder gradually increased to 80 oz. *per diem*. On April 30 the patient got up; on May 2 the right lumbar wound ceased to leak urine; on May 23 the left wound ceased to leak, and healing of both wounds occurred within two months of the operation.

The examination of the eyes disclosed no pathological changes.

The estimation of the blood-pressure in the radial artery by Dr. Gossage yielded a maximum of 145 mm. Hg. and a minimum of 100. After the healing of the wound the urine passed averaged about 80 oz. *per diem*. The specific gravity was about 1006, and it contained a cloud of albumin or 0·25 pro mille.

The patient appears to be suffering from chronic nephritis, and an intercurrent attack of acute nephritis caused congestion and arrest of the kidney secretion. The relief of the congestion by bilateral nephrotomy and drainage was followed by a return of the patient to his previous state. He expresses himself as feeling quite well. Besides the condition of the urine, the only noticeable feature is the rather hard radial artery.

DISCUSSION.

Dr. DE HAVILLAND HALL said he saw the case in the hospital when it was under Mr. Spencer's care, and his reflection was that the surgeons were gradually invading the whole domain of the physicians in medicine. He thought that at least cases of uræmic convulsions would be left to the physician, but this case showed that even in this condition surgical assistance might be desirable in extreme cases. Before Mr. Spencer operated, it seemed likely that the patient would die. Whether the benefit was due to the incision into the kidney having relieved the tension and caused some bleeding he did not know, but the result had been most satisfactory, for the patient now appeared to be in a good condition. Mr. Spencer was to be congratulated on the courage with which he attacked so grave a case.

Mr. CLAYTON-GREENE desired to add his congratulations to Mr. Spencer on the result in this case. He was himself interested in cases of the kind, having brought before the Society a year ago five cases in which suppression of urine had occurred, and a similar operation had been done.[1] One case was due to perchloride of mercury poisoning. The operation resulted in the subsequent passage of urine almost normally, though in the case of perchloride poisoning the patient died later from ulcerative colitis. He considered that there was a sphere of usefulness in operation on these cases when very acute. In severe nephritis the kidney should be incised, but one should not have recourse to the operation of stripping the capsule, which was a much more severe operation, and necessitated a larger wound. The hæmorrhage also was more severe. The operation carried out by Mr. Spencer in this case was much more simple, and there was but little bleeding. These results should encourage the practice of operating in cases of acute suppression of urine which had failed to respond to medical treatment after forty-eight hours.

Mr. SPENCER, in reply, said he had done the operation before, some years ago. The patient had had chronic Bright's disease with subacute intercurrent inflammation of the kidney, and was almost dying. He cut into both kidneys in the same way. The patient was not made worse by the operation, and commenced to secrete urine afterwards, but he died in thirty-six hours. The kidneys, however, were much more fibrotic than in the present case. A short time ago he was asked to operate in a case of malignant endocarditis with acute suppression of urine and hæmaturia, as it was thought that there might

[1] *Proceedings*, 1911, iv, pp. 161-68.

be something of the nature of renal suppuration. He cut into only one kidney and found it very vascular, but there was no sign of abscess. A little urine was subsequently passed, but the girl only lived a day or so. He believed the operation made no difference, one way or the other. Such kidneys were extremely vascular, and unless care were taken there would be considerable loss of blood.

Osteo-periostitis of Right Tibia (Congenital Syphilitic).

By PAUL B. ROTH, F.R.C.S.

H. G., AGED 8 years 10 months, attended hospital on May 17, complaining of pain and swelling in upper part of right leg, below the knee. The trouble began ten days previously. No history of injury. On examination there was observed a spindle-shaped swelling, involving the upper third of the leg. The swelling consisted of an enlargement of the shaft of the tibia. A skiagram showed that the shaft of the tibia for its upper third was increased in size by a localized osteo-periostitis. The Wassermann reaction was positive. No history of syphilis was obtained in either father or mother.

Osteo-periostitis of Left Tibia (Congenital Syphilitic).

By PAUL B. ROTH, F.R.C.S.

C. B., AGED 7, attended hospital on March 9, complaining of pain and swelling of left leg below the knee, which had been noticed for two weeks. The mother stated that the child had suffered from the same trouble in June last (?), but had recovered. Two weeks before Christmas the child had a severe blow on the left shin. On examination there was observed a spindle-shaped swelling, involving the middle two-fourths of the leg. The calf muscles were wasted. The swelling appeared to consist entirely of an enlargement of the shaft of the tibia. A skiagram showed that the shaft of the tibia for its middle three-fifths was markedly thickened by a deposition of new periosteal bone. The Wassermann reaction was positive. There was no history of syphilis in the father, but a few weeks after the birth of the patient the mother had a "rash on the arms and face and lost her front hair."

Tonic Spasm of the Muscles, chiefly of the Extremities (? Myotonia).

By JAMES GALLOWAY, M.D.

THE patient, D. L., aged 14, is said to have shown no signs of the present condition of the muscles of the left arm and leg till about Christmas, 1911. She states that she could play the pianoforte readily up to that time. It was then noted that she had difficulty in moving the muscles of the left arm and hand, a condition that has continued and increased up to the present time. There is similar difficulty in moving the muscles of the left leg and foot. The right arm, hand, and leg appear to move practically normally.

On observing the muscles of the left arm and hand, they are found to pass into a condition of spasmodic contraction on very slight exertion or stimulation; the contractions resulting relax very slowly. On clasping the hand on any object the grasp of the hand continues for a considerable period of time—many seconds—and it is only after strong voluntary effort or after waiting for some time that the grasp is relaxed. Movements bring about a similar long-lasting spasm of the muscles of the left shoulder.

Similar long-enduring spasm affects the muscles of the left leg and foot, causing some impairment of ease in walking. The toes become spread out in a spasmodic manner on attempting to extend the foot, the calf muscles, as also the extensors of the leg, become firm and remain so for an unusually long time. All movements become easier and more rapidly executed when repeated. The electrical reactions are normal, there is no alteration in sensations, no definite evidence of increase of reflexes, nor are there involuntary muscular movements. The child is well in all other respects. There is no evidence or history of injury to the head or brain, no headache, vomiting, nor sickness, the eye movements are normal, and there are no changes in the fundus oculi. The character of the movements of the muscles suggest "myotonia."

It is noteworthy that this condition is stated to have commenced about five months ago, and that no history is obtained of any other member of her family being affected in this way. The affection of the muscles is almost entirely limited to the left side, chiefly the arm and

leg. The condition of spasm, although markedly increased on voluntary movement, is probably never entirely absent, and there is a slight suspicion of increase of the tendon reflexes of the lower extremity (slight ankle clonus ?).

DISCUSSION.

Dr. GALLOWAY added that some members of the Section had formed strong opinions about this case which he hoped they would express. When the patient came first under observation he thought that the case was probably an unusual example of true myotonia, of a similar type to myotonia congenita or Thomsen's disease. The familial aspect of Thomsen's disease was well known to members, and this was entirely absent in this patient's case, but it was stated, and it seemed possible, that such cases might occur sporadically without a history of the disease in the family that could be ascertained. He had made it clear in his description that there were several points against this diagnosis. The first was the peculiarity that the hypertonicity was almost entirely on the left side. There was also in question a possible slight tendency to increase of the deep reflexes on the left side. He himself had not been able to be convinced that the deep reflexes were increased. There was no disturbance of the fundus oculi or optic nerve of any kind suggesting cerebral lesion; a small crescent existed on the inner side of both optic disks which might be regarded as a slight congenital coloboma. The possibility of the condition being of "functional" origin had been considered, but he thought that there was no evidence to support this conclusion.

Dr. F. E. BATTEN remarked that if by "myotonia" Dr. Galloway meant simply muscular hypertonia, he would agree with him. This case certainly showed hypertonia of muscles on one side. But if the term were used by the exhibitor in the sense of myotonia congenita or Thomsen's disease, he differed from him, because he did not consider that this case in any way showed the symptoms which Thomsen had and which were characteristic of the disease. The question arose whether the condition was due to organic disease, or whether it was a functional one. Judging by the reflexes, he could find no evidence of organic disease of the pyramidal path. But hypertonia might be present without involvement of the pyramidal path. Such hypertonia was present in cases recently described by Wilson, under the title "Progressive Lenticular Degeneration," and the pyramidal tract itself was not involved. The real question in this case was as to whether the condition was due to some intracranial neoplasm, or whether it was purely a functional condition? This was difficult to decide. He was in favour of regarding it as probably due to an organic lesion, but he was prepared to accept the possibility of its being functional.

Dr. HALE WHITE agreed with Dr. Batten that this case would not pass as one of the disease described by Thomsen as occurring in himself and other members of his family. The contractions were not like those occurring in Thomsen's disease. They were striking, well marked, and yet unilateral. There was also some overtone in the muscles, even when the voluntary contractions were not going on. Whatever the diagnosis might be, he thought it should be put out of the category of Thomsen's disease.

Dr. GALLOWAY, in reply, admitted the justice of the criticism as to the use of the word myotonia in describing this case, though he thought the criticisms would have been more applicable if he had suggested that the case was one of myotonia congenita or Thomsen's disease. He did not think that the patient suffered from this malady. The peculiarities which he had already pointed out were much against this diagnosis. He thanked Dr. Batten for the interest he had taken in the case, and also for his suggestion that the hypertonicity of the muscles might be due to some irritation of the pyramidal fibres owing to some obscure lesion or degeneration in some of the central ganglia. He added that he was not yet quite certain whether any atrophy of the muscles existed or not. This would be an important point for observation in the future, especially in view of the cases which had been described under the name of myotonia atrophica. The hypertonicity or spasm of the muscles was apparently absent during sleep. The patient would be kept under observation, and a further report made.

Sclerema Cutis (Adultorum).

By JAMES GALLOWAY, M.D.

THE patient, J. M., aged 54, states that his malady followed rapidly after being exposed to a foul atmosphere, the unpleasant odour of which he found difficult to get rid of for an unusually long time. The hardening of the skin followed this incident, which occurred about three months ago, and has progressed till within the last fortnight, when he has been under treatment by means of massage. The firm, swollen state of the skin is now noticeable from the face down to about the level of the loins, and is universal within this area, though worse in some parts than in others. It is especially noticeable round the neck, over the arms, and the skin of the back. The firm swelling of the skin impedes especially the movements of the neck, the shoulder, and elbow, and gives a peculiar expressionless appearance to the face. Some hardening of the skin has been noticed about the lower part of the thighs and knees, but has passed off. On the trunk a certain degree of erythema can be noted where the hardening of the skin is greatest or most recent. On the inner aspect of

the arms the hardening of the skin is seen to have a lobulated texture. There is no appearance of sclerodermia of the usual type nor of morphœa at any part of the affected area. The distribution of the affection is practically continuous within the limits of the affected region.

A careful physical examination has not revealed evidence of visceral disease, and has not suggested the cause of this unusual affection.

DISCUSSION.

Dr. GALLOWAY said he had been asked why he had not made the diagnosis of sclerodermia in this case. The reason was that he wished to emphasize the point that there was not the slightest appearance of the firm sclerotic patches of skin so often seen in cases of sclerodermia, due to condensation of the fibrous tissue of the cutis, and of which the most marked examples occurred in the type known as "morphœa." In this case there was not the slightest appearance of the parchment-like type of sclerodermia. On the other hand, the thickening seemed to involve both the skin and the subcutaneous tissues, producing the aspect of a peculiar firm œdema. The type of lesion resembled closely the type seen in newly born, or young, children, of which Dr. Garrod and others had brought forward examples from time to time from the hospitals for sick children. In some of these children a condition of shrinking and atrophy of the skin occurred and these were usually fatal, resembling the severe type of the affection to which the name of sclerema neonatorum had been given. Others, more closely resembling the patient shown this evening, suffered from a widespread, very firm, wax-like œdema of the skin and subcutaneous tissue which gradually disappeared, recovery taking place, and to these the name of œdema neonatorum had been given. The patient shown that evening had now been under treatment chiefly by means of vigorous massage, and the firmness of the skin was beginning to melt away. He could now raise and turn his neck so that he could shave himself. The firmness of the arm was beginning to break up, so that the inner surface could be seen to be pitted and dimpled. He said himself that the movements of the arm, shoulders, and neck were much more easy now than a month ago. Dr. Galloway could not make any definite suggestion as to the cause of the disease in this case.

Dr. LANGMEAD thought it was a pity to call the condition sclerema, though he knew it corresponded with sclerema as described in England. Recently he had had occasion to look up the literature, and found that the cases which had been called sclerema neonatorum in England were not similar to those which originally received that name. According to German descriptions, sclerema was almost invariably fatal, and was associated with a yellowness or whiteness of the skin which soon became almost general. It was also associated with a good deal of prostration, and led to death in a very short time. The cases

which had been so designated in England did not show those features at all. Recognizing this, Dr. John Thomson, of Edinburgh, in the last edition of his book on children's diseases had called them "pseudo-sclerema."

Dr. PARKES WEBER considered that these cases should be labelled the "deep type" of sclerodermia. Such "hide-bound" cases had, he thought, just as much claim to the name "sclerodermia" as the cases of superficial disseminated sclerodermia had.

The PRESIDENT agreed with those who regarded this case as really one of sclerodermia, of the form spoken of as generalized. In these cases the swelling and infiltration were only the early stage. He had reported many cases of sclerodermia, and in a number of them the early condition had been of exactly this nature. One such patient had considerable thickening of the skin of his chest, which remained swollen for a year or so before shrinking set in, and when the latter did occur it proceeded to mummification. He feared the outlook in the present case was the same, and he considered that generalized sclerodermia was a condition of extreme gravity, and distinct from the localized form.

Dr. GALLOWAY, in reply, said that he had employed the word sclerema to emphasize the distinction between this case and the ordinary type of sclerodermia with which they were more familiar. It resembled much more closely the forms of disease in children in which the name œdema and sclerema neonatorum had been used. He hoped that in his patient the lesions would not become atrophic, and so avoid falling into the group of severe cases of sclerema neonatorum to which Dr. Langmead had alluded. He thought that the effect of the secretions of the thyroid and pituitary glands should not be forgotten in investigating the origin of such cases nor in their treatment. He was confident that already marked improvement had taken place in this patient, and he hoped that he would be able to bring him before a subsequent meeting of the Section.

Hæmorrhagic Disease in a Child (? Scorbutic).

By G. A. SUTHERLAND, M.D.

L. C., FEMALE, aged 7. She appears to have been healthy, and in every way normal in her habits, up to August of last year, when she had an illness characterized by pain and swelling in the left hip and the left elbow, with pyrexia. This illness lasted for some weeks, and when the swelling at the hip subsided it was found that the head of the left femur was dislocated.

Admitted to Orthopædic Hospital in March for dislocation of the hip. Wasted, anæmic-looking child, with dilated veins over the body

generally. Skiagrams showed marked changes at the upper ends of both femora and both humeri, in varying extent, and consisting of rarefaction of the bone below the epiphysis, subperiosteal effusion, and apparently fresh formation of bone beneath the periosteum. Attention was first directed to the humerus by the occurrence of pain and swelling about the right shoulder. At times pain, tenderness, and swelling around the left hip. Recurrent attacks of purpura affecting the legs, feet and arms. Motions usually offensive. Presystolic and systolic murmurs at the cardiac apex, and some subcutaneous nodules over left elbow. More recently melæna has been present on two occasions. Hæmaturia occurred to a marked extent on May 23, and at other times blood cells have been present in the urine.

Blood examination showed 5,500,000 red and 6,000 white cells, with 78 per cent. hæmoglobin. No blood changes detected. Wassermann reaction negative; von Pirquet's test negative. No sponginess of the gums, or stomatitis. Urine sterile.

DISCUSSION.

Mr. W. G. SPENCER said the evidence in respect to scurvy did not seem to be strong, especially having regard to the dislocation of the hip. There was no reference in the notes to bacteria, but judging by the skiagrams it might be a rather mild form of streptococcal infection, perhaps from the throat, inducing the hæmorrhagic condition of the periosteum. If the affection persisted and the child did not get better he would suggest the preparation and injection of a streptococcic vaccine.

Mr. FAGGE asked why the case should not be regarded as one of Henoch's purpura.

Dr. SUTHERLAND replied that he did not think the course of the illness had been that of a case of Henoch's purpura. There had been no attacks of abdominal pain, which was one of the leading features of that affection. Though he had seen many cases of Henoch's purpura, he had not seen one which presented bone lesions. The points in favour of its being Henoch's purpura would be that there had been melæna, and that there was now and had been some hæmaturia. He was obliged by the suggestion about the treatment by vaccines and as to exploring the joint. They had been anxious to find some causal organism, but every test so far had proved negative. He hoped to explore the joint to see what could be done, but until a definite organism was found it would not be wise to attempt any haphazard vaccine therapy.

Fibro-caseous Tuberculosis of Glands in Neck; Caries of Dorsal Spine.

By ROBERT HUTCHISON, M.D.

F. S., AGED 46. Twenty-four years ago had large swelling in the neck which persisted for some months and then entirely disappeared. About a year ago he underwent an operation for fistula and shortly afterwards the swellings returned and have persisted since, although they vary greatly in size from time to time. His health otherwise has been good. He had never been aware of anything wrong with his back. Masses of enlarged glands in neck. (Section shows fibro-caseous tuberculosis.) No enlarged glands elsewhere. Viscera normal. Skiagram shows caries of two dorsal vertebræ.

Dr. HUTCHISON said the case was remarkable for its long duration, for the very benign course which it had run, and for the fact that the patient developed caries involving two dorsal vertebræ, without his knowing of anything wrong with the back at all. Evidently the patient had a very high degree of resistance to the tubercle bacillus.

A Case of Ununited Fracture of the Ulna treated by Bridging the Gap with a Slice sawn from the Tibia.

By C. H. FAGGE, M.S.

A. W., AGED 11, was admitted into Guy's Hospital on September 16, 1910, under Mr. Arbuthnot Lane, in whose absence he came under my care. About two months before admission he had fallen, breaking both bones of the right forearm. On admission there was marked deformity with large bony bosses at the middle of both bones and complete loss of pronation and supination. Examination under the X-ray screen showed that this was due to cross-union, with much overlapping of the fragments of both bones. On September 30 the site of the fractures was exposed, a wedge including callus was sawn from the radius and the ends joined by the application of a Lane's four-hole plate; the ulna was divided and plated in the same way.

A skiagram taken on October 14, 1910, shows a gap between the fragments of the ulna; he was discharged on October 28. Because of non-union of the ulna he was readmitted in January, 1911; a second skiagram shows firm union in the radius, but the gap in the ulna appears wider and rarefaction is taking place around the screws in this bone; the plate was removed from the ulna and the ends freshened; a third skiagram shows the condition on discharge (fig. 1).

In April, 1911, he was again operated on and the freshened ends of the ulna united with two plates after bringing them into apposition by pulling the hand inwards; a fourth skiagram shows the result to be unsatisfactory.

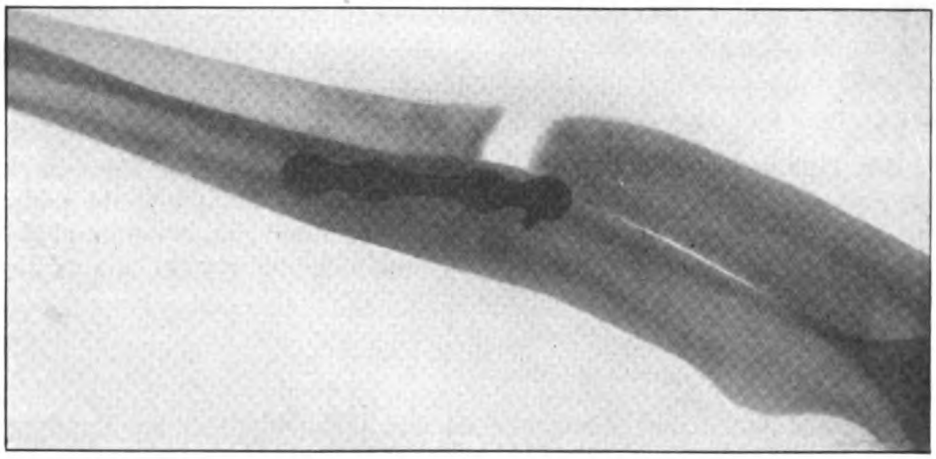


FIG. 1.

Because of pain, weakness and deformity he was again admitted in March, 1912; the deformity is most marked on full pronation and in all rotatory movements crepitus can be felt; on March 4 I cut down on the radius and removed the plate; the screws were quite firm; the ulna was then exposed and the plates removed: most of the screws lay in a granulating cavity in the bone and there was a false joint between the two ends. Callus and granulation tissue were scraped and cut away and the posterior surfaces of both fragments were flattened with a gouge; on to this surface a slice of compact bone, 4 in. long and $\frac{3}{4}$ in. wide, sawn from the inner surface of the tibia, was firmly secured by six $\frac{1}{2}$-in. screws (No. 5); a fortnight later the bone-graft did not seem secure and another skiagram suggested that the screws had come away

from the lower rarefied end of the ulna, but two months after operation union was quite firm and a skiagram (fig. 2) confirms this.

He has already regained a fair amount of power and has no pain; pronation and supination are not complete and flexion at the elbow is diminished. The chief interest in the case is the question of the fate of bone-grafts; for the idea of using this method in this case I am indebted to the ingenuity of Mr. Arbuthnot Lane.

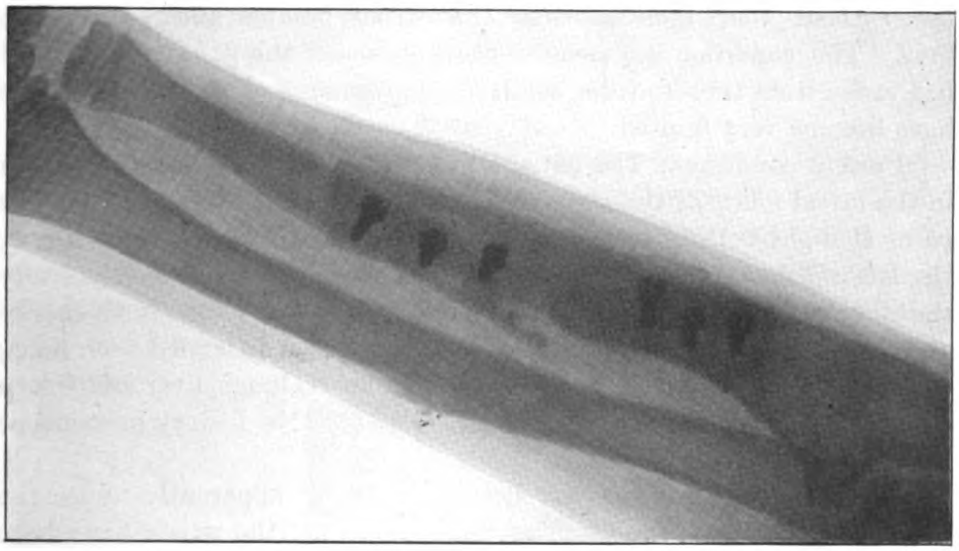


FIG. 2.

Mr. FAGGE added that it was held by some surgeons that bone transplanted from one part of the body to the other (auto-plastic transplantation), especially if devoid of its periosteum, underwent gradual absorption. Mr. Arbuthnot Lane's idea in advising him to use this method in this case was that if the piece of bone were securely screwed at the two ends into the ulna it would not undergo absorption. So far as they could be put forward in evidence, the skiagrams supported that view. It also raised a question which had recently much puzzled those interested in the operative treatment of fractures—viz., the functions of the periosteum—and this case confirmed his opinion, derived from operations on simple fractures, that the periosteum was of absolutely no value to the underlying bone, in its nutrition or in its re-formation. In this case no effort was made to transplant the periosteum nor to retain it in position on the tibia from which the bone-graft was removed.

A Case of Multiple Arthritis of Doubtful Origin.

By DUNCAN C. L. FITZWILLIAMS, F.R.C.S.

A GIRL, aged 15. History: About eighteen months ago, the tarsal joints became painful and swollen, first the right and then the left. About four months later the wrists became affected, the left first. The condition has steadily become worse, the pain has remained but varies from time to time, while the movements of the affected joints have become very limited.

Present condition: The patient limps on walking owing to the pain in the tarsal joints of the right foot; the ankle-joints are free. She has pains at night both in the foot and in the wrists. Both wrists are flexed, the left one having no movement at all, while the right one has very slight movement. The synovial membrane of the joints is thickened and the interphalangeal joints of the hands are distended with fluid; there has been no abscess formation. The heart, lungs, liver and spleen are normal, and there are no enlarged glands. No history or evidence of syphilis.

The treatment hitherto has been carried out apparently under the impression that the condition is tuberculous, for the wrists have been in plaster of Paris for three months. X-rays show very little bony change in the wrists.

The condition is thought to be due to a chronic infection and not to tubercle.

Mr. FITZWILLIAMS added that he thought tubercle could not possibly be the cause of the trouble owing to the numbers of the joints affected, and also from the fact that many of the smaller joints were infected. He thought it was a chronic infection due to the chronic absorption of infection through the mucous membranes, but at present it was very difficult to say where the source of infection had been, as it had certainly cleared up now as far as could be discovered. Before the meeting it had been his intention to break down the adhesions and to move the wrist-joints, but several members had advised him to wait and to trust a little longer to general treatment, and this advice he intended to take.

Case showing the Result of Bilateral Division of the Ramus of the Jaw for Ankylosis of both Joints (?) Nineteen Months after Operation.

By DUNCAN C. L. FITZWILLIAMS, F.R.C.S.

HISTORY (November 9, 1910): Seven years previously the patient had been laid up for six and a half months with "rheumatic fever" which came on three weeks after a premature birth. After being in bed a month her lower jaw became painful and stiff. She has never been able to open her mouth since then.

On admission: The lower jaw is completely fixed, both joints apparently being ankylosed. The upper incisors were knocked out six years ago and the space between the gum and the lower incisors is $\frac{3}{8}$ in. She can eat fish and soft food but can chew nothing. The left knee is stiff, dating from the same illness; the joints are said to be painful during damp weather.



Photograph (taken May, 1912) showing the result of bilateral division of the ramus of the jaw nineteen months after operation.

Operation (November 12). The neck of the left side of the mandible was divided with a Gigli saw and some of the bone removed. A week later the right side was similarly dealt with and most of the ascending rami of both sides were removed; no muscle-flap was made. There was bony union between the bone and the glenoid fossa, so the head of the mandible was left in place on both sides.

November, 1911: The mouth appears to be freely movable and has not shown any tendency to become stiff since the operation; the pain is no worse than it was before and she is able to chew.

May, 1912: The gape of the jaw between the canines is 2 in.

Mr. FITZWILLIAMS added that the case was shown because teaching and apparently experience were both inclined to view division of both sides of the jaw for ankylosis as an unsatisfactory procedure. If one showed these cases within three or four months of the operation, some member was sure to say that he would prefer to see the case a year later; if one waited a year one frequently lost sight of hospital patients altogether. The good result in this case depended entirely on the large amount of bone which was removed from each side.

A Case of Sarcoma of the Jaw, probably Myeloma.

By DUNCAN C. L. FITZWILLIAMS, F.R.C.S.

A GIRL, aged 10. Seen for the first time, May 22. History: An old tooth came out in bits from the upper jaw on the right side during the month of March. Since then the child has made no complaint of pain, though she says that the mouth sometimes bleeds. The mother noticed the condition by accident and took her at once to Dr. Campbell, who sent her to hospital for advice.

Present condition: The child has a dark red area about the size of a halfpenny, which extends on to the palate from the alveolar margin of the right upper jaw. The erupting tooth on that side is displaced outwards towards the cheek. The mass is sessile, attached to the bone, elastic, the red area is denuded of epithelium and has quite a smooth edge; the notch seen is where a piece has been removed for examination.

Mr. FITZWILLIAMS added that the case was diagnosed as a myeloma provisionally before the slide was cut. The section exhibited at the meeting showed that this was correct, as the field was crowded with exceptionally large multinucleated cells. He thought that the growth might quite well be removed locally with the sacrifice of three teeth and the corresponding part of the alveolar margin of the upper jaw.

Facio-scapulo-humeral Type of Muscular Dystrophy in Four Patients in Three Generations.

By H. BATTY SHAW, M.D., and P. J. EDMUNDS, M.B.

THE following cases are shown as exemplifications of the fact that cases of muscular dystrophy in whom there is no hypertrophy survive to old age. They also serve to show the existence of types of muscular dystrophy, in which the morbid change takes place in the face, trunk,

and limb muscles, combining as it were the features of distribution met with in pseudo-hypertrophic muscular dystrophy, Erb's "shoulder" type, and the "Facio-scapulo-humeral type" of Landouzy-Déjerine. So far as records are obtainable, two other members of the family probably suffered from similar disabilities—Mr. G., father of Case I, who died at the age of 77, and Lena C., daughter of Case I and half-sister to Cases II and III. The malady, therefore, appears to occur in both males and females, beginning about puberty and being transmissible by either parent.

Patient	Present age	Age of onset	Muscles showing atrophy			
			Head	Trunk	Shoulder, girdle, and arms	Legs
(I) Mrs. P.	65	14	Orbicularis oris (slight)	?	Right pectoralis major; right and left biceps and triceps: right serratus magnus; trapezii	*Nil* (?)
(II) George P., son of No. I	26	12	Orbicularis palpebrarum and orbicularis oris; left sterno-mastoid; right in part	Lower segments of recti abdominis	Pectorals, biceps, triceps, trapezii; latissimi dorsi; serrati magni; supinatores longi	Quadriceps femoris (right and left); adductores; biceps femoris (right and left); ileo-psoas (right and left); anterior muscles of legs; glutei maximi
(III) Mrs. T., daughter of No. I and sister of No. II	32	16	Orbicularis palpebrarum and orbicularis oris; left sterno-mastoid	Lower segments of recti abdominis	Pectorals, biceps, triceps, trapezii; latissimi dorsi; serrati magni; supinatores longi	Hamstring muscles (right and left): glutei maximi; ileo-psoas (right and left); anterior tibial muscles
(IV) Ada T., daughter of No. III	11	9	Orbicularis palpebrarum and orbicularis oris; left sterno-mastoid	Lower segments of recti abdominis and obliqui and transversalis	Right pectoralis major; triceps (right and left); right trapezius; left trapezius (less); right serratus magnus; left serratus magnus (less); latissimi dorsi; supinatores longi	Hamstrings (right and left); quadriceps femoris (right and left); glutei maximi; ileo-psoas (right and left); anterior tibial muscles (right and left)

DISCUSSION.

Dr. BATTY SHAW desired to record his thanks to Dr. F. E. Batten for examining the cases with him, in consequence of which he need not hesitate to call these cases examples of the Landouzy-Déjerine type of muscular dystrophy.

The hesitation in calling them by that name was due to the fact that the involvement of the face appeared to be but slight, and the involvement of the scapulo-humeral muscles very marked. Apparently, however, that did not exclude the propriety of calling these cases by the name Landouzy-Déjerine. To be able to show the condition affecting three generations was fortunate.

Dr. F. E. BATTEN said that, in his opinion, the atrophic form had nothing to do with the form here described. He considered these cases were examples of the facio-scapulo-humeral type, and they were typical of the disease as described by Landouzy and Déjerine.

Aortic Regurgitation with Extreme Pulsation of the Aortic Arch; Diastolic Shock and Diastolic Thrill over the Heart.

By F. PARKES WEBER, M.D.

THE patient, Mrs. M. I., aged 28, is an active-looking woman of rather slender build, who during the last month has suffered from severe pain in front of the chest. When aged 15 she had rheumatic fever, and her heart has been said to be "weak." Above the level of the heart to the right of the sternum there is a loud systolic murmur to be heard, and pulsation can be felt, accompanied by a marked systolic thrill. There is much pulsation in the episternal notch. The cardiac apex-beat is displaced to the left axillary region. Over the aortic base there are both loud systolic and loud diastolic murmurs. Over the mid-cardiac region there is a loud diastolic murmur, accompanied by a diastolic thrill. With the flat of the hand placed over the heart the diastolic thrill can be felt as if travelling from the aortic base towards the cardiac apex. About the nipple line, in the fifth left intercostal space, a diastolic shock can be felt, which might possibly at first be mistaken for the apex-beat. Wassermann's reaction is positive.

[*Addendum.*—Dr. Weber had at first supposed that there was an aneurysm of the first part of the aorta with aortic regurgitation, but after Dr. Hale White's remarks the patient was carefully re-examined with the help of X-rays (Dr. Finzi). The case was then clearly shown to be one of extreme pulsation of the arch of the aorta without aneurysm.]

Dr. HALE WHITE asked why the diagnosis of aneurysm was made. It seemed to him that the patient was suffering from aortic disease and that considering the improbability of an aneurysm in a woman of that age, it would be wiser not to record the case as one of aneurysm until the diagnosis had been verified by an autopsy.

Persistent Œdema of Right Hand and Forearm after Slight Traumatism.

By F. PARKES WEBER, M.D., and G. DORNER, M.D.

THE patient, J. H., aged 30, is a traveller, of German Jewish family, who has been several years resident in England. There is no history of any similar œdema in other members of his family. He denies having had any venereal disease. He has apparently enjoyed good health, but one and a half years ago his right hand and right forearm became swollen after a slight traumatism (he knocked his hand against a table).



Persistent œdema of right hand and forearm.

The swelling, which commenced the day after the injury, has persisted since then (*see* illustration), being more marked after exertion and less marked after rest and during temperate weather (the illustration shows the condition after a whole day's rest). It does not extend upwards beyond the elbow, and is best marked over the back of the hand, which "pits" considerably on pressure (there is, however, likewise some pitting on firm pressure over the forearm). There is no anæsthesia, paræsthesia, or tenderness, and, excepting at first after the injury, there has been practically no pain. One hand does not seem to be colder or hotter than the other. The blood-vessels are normal and there is no local cyanosis (as there is in cases of so-called "acrocyanosis"). Röntgen ray examination does not show anything abnormal in the bones of the affected extremity. There are no cervical ribs. There is no enlargement of

cubital or axillary or cervical lymphatic glands. The thoracic and abdominal viscera are apparently healthy. Thyroid treatment has just been begun. The present case seems to be allied to various cases of persistent œdema of the hands (chiefly of the backs of the hands) following traumatism, described on the Continent by Secrétan (1901), Borchard, R. Grünbaum, Patry, Van Trooijan (1910), &c. With these may be compared the case of a boy, aged 16, shown before the Dermatological Section of the Royal Society of Medicine on April 20, 1911,[1] under the heading "Functional Hysterical Trophœdema" (a case of persistent swelling of the right hand after an injury). Similar chronic œdema in the foot may occur after traumatism. Dr. H. G. Turney's case of chronic œdema of the right foot after an injury was perhaps analogous; it was shown before the Clinical Section on April 30, 1909,[2] under the heading of "Trophœdema following Trauma." More or less similar cases of chronic œdema in upper or lower extremities are undoubtedly met with without any history of injury. The possibility of the artificial production of œdema to simulate disease (malingering and "hysterical malingering") has always to be kept in mind and guarded against.

A Case of Actinomycotic Sinuses in the Thigh, from one of which a Concretion, probably formed in the Appendix, was discharged.

By Raymond Johnson, F.R.C.S.

Man, aged 60; was in good health until eleven months ago, when a swelling formed in the right groin, and about six weeks later opened spontaneously and discharged pus. Since that time numerous other abscesses have discharged in different parts of the upper two-thirds of the thigh. About four months ago a concretion came away from one of the sinuses. The patient's general condition is very good. The superficial tissues of the upper two-thirds of the right thigh show irregular areas of induration and softening, and numerous sinuses and the scars of sinuses which have closed.

The long history and irregular spread of the chronic inflammatory process suggested the possibility that the case was one of actinomycosis

[1] *Proceedings*, 1911, iv (Derm. Sect.), p. 84.

[2] *Proceedings*, 1909, ii (Clin. Sect.), p. 200.

—a suggestion which was readily confirmed by the naked-eye and microscopic examination of the pus from one of the sinuses.

The small concretion above mentioned shows in its broken state a laminated structure, and its characters support the view that the condition originated in the appendix. The patient states that in the early part of his illness there was some swelling in the right side of the abdomen, and at the present time slight induration can be felt in the outer part of the iliac fossa.

The man is a native of Cambridgeshire, and works on the railway. He says he has often chewed the maize with which he feeds his chickens.

A Case of Gonococcic Empyema.

By HENRI GÉRAUD, M.D. (Paris), & H. J. JOHNSTON-LAVIS, M.D.

WE consider it of sufficient interest to offer you the details of this case on account of its extreme rarity and of the special clinical evolution of this disease.

W. D., aged 19, electrician, no antecedents of importance, enjoying good health, became infected in August, 1910, with a rather acute attack of gonorrhœa. This he treated with the usual internal remedies and continued his usual avocations. While sitting out of doors on the evening of November 14, 1910, he was suddenly seized with a violent pain in the middle of the back. Dr. Marc de Lévis saw him the following day. The pain persisted, he had a high temperature, much dark urine with a hæmaturic aspect, muco-purulent deposit, and covered by a thick froth. A provisional diagnosis of gonorrhœal nephritis was made. He was then therefore admitted to the Queen Victoria Memorial Hospital, under the care of Dr. H. J. Johnston-Lavis, who found the patient suffering from anuria, which, after he had been lying on a hot-water bottle for twenty-four hours and been given Vittel water, cleared up. Urine was passed freely (21 litres, 700 c.c.), with no albumin, and on the fourth day it was quite clear. Notwithstanding this the general state of the patient remained most unsatisfactory; he was markedly depressed, plunged, as it were, in an intensely adynamic or typhoid state, which, with the absence of other signs of typhoid, made one think of acute tuberculosis. There was constipation, the abdomen distended with gas; temperature 39° to 39·5° C., pulse 92, respiration 28. At the same time the pain became marked and localized at the posterior

left base of the thorax. Upon auscultation marked pleurisy was detected, with very rapid exudation of fluid. On November 24 this had extended to the whole lower two-thirds of the left pleura at the back, and forwards to the axillary line. Almost simultaneously marked hectic fever and sweats appeared, indicating the formation of pus. The first author, Dr. H. Géraud, was asked to see the case in consultation on November 26. The effusion was then found to reach behind up to the spine of the scapula, and in front to the anterior axillary line. All this was markedly dull on percussion. Traube's space, on the contrary, retained its sonority. Respiratory movements on the affected side were abolished, as were also respiration sounds and voice resonance over the dull area. No pleuritic "souffle" was detectable. In front skodaic resonance was found on percussion under the clavicle, and rough breathing sounds on auscultation. Only at the base of the right lung were there a few sub-crepitant râles at the end of the expiratory effort. There was some subcutaneous œdema over the dull region, rather indicating empyema.

Simultaneously with the appearance of the patient's acute constitutional symptoms his urethral discharge had practically disappeared, which raised the suspicion that the infecting organism of the pleura was the gonococcus. We then made an exploratory puncture in the ninth costal interspace on the middle axillary line and withdrew about 1 c.c. of liquid having the appearance of the fluid of an ordinary serofibrinous pleurisy, but a little more cloudy. Dr. Boisseau examined this in his usual competent manner and found it to consist of a pure culture of the gonococcus.

From November 26 to November 29 all the symptoms, including intense anorexia, hectic fever, &c., rapidly increased—pulse 120, temperature between 39° and 39·5° C., respiration 30 to 40. We had no hesitation under these conditions in advising immediate operation.

Operation by Dr. H. Géraud (chloroform anæsthesia by Dr. H. J. Johnston-Lavis): Incision of thoracic walls at level of the ninth left rib and at the lowest point of dullness between anterior and posterior axillary lines; resection of 5 or 6 cm. of the fifth rib. On opening the pleura we were much surprised to see the flow of an absolutely clear serum. The finger was introduced into the wound and pushed upwards and backwards, breaking up false membranes and adhesions. Immediately there was a great flow of lumpy pus in which floated clots of yellow, glue-like mucus and pseudo-membranes. After all was evacuated and without any irrigation two No. 45 rubber drains were introduced and held by silk-gut to the skin.

The patient bore the operation well; the temperature fell in the evening to 36·5° C. The following morning temperature was 37·2° C.; general condition much better; purulent discharge very abundant, requiring two dressings daily. All pointed to rapid resolution and quick recovery, but we were doomed to disappointment. Notwithstanding good drainage of the pleura the temperature rose the third day to 39° C. This we supposed to be due to general infection. In consequence, on the fourth and ninth days 10 c.c. of electro-argol were given as an intravenous injection. Some benefit seemed to accrue, as during the next nine days the temperature remained below 39° C., but it rose again and, with slight oscillations, continued till February 21, 1912. During this time we injected on several occasions 25 c.c. of Burroughs and Wellcome's stock anti-gonococcus serum, which produced no appreciable reaction, favourable or unfavourable, of any kind. The cavity was daily irrigated with permanganate of potash and peroxide of hydrogen. Von Bier's vacuum method was tried several times to the open wound. During several days subcrepitant râles could be heard in both lungs, which, however, disappeared later on. The discharge became very fetid, and was most irregular in quantity of outflow. Each time a flush issued the condition of temperature, hectic flushes and sweats, appetite, and comfort of the patient improved, to again get worse as the flow became less. We attributed this to pockets being formed by adhesions. Altogether the patient went to the bad, steadily losing flesh and strength. New foci of pus accumulation were indicated by the extension of the dullness to a higher level behind during the month of January. In February it extended forwards at the lower part of the lung. Exploring with a spinal injection needle at different points produced no pus.

On February 21 Dr. H. Géraud introduced a Luys urethroscope along the line of the drainage-tubes for a distance of 17 cm. A small fistulous opening was detected, from which drops of thick pus oozed at about the level of the fourth rib. Through the tube and under visual control this orifice was gradually enlarged with a series of bougies of increasing dimensions, as would be done in dilating a urethra, reaching No. 40. This gave issue to a lot of creamy, thick and very fetid pus. On the following days the dilatation was continued, and then we left in place a gum elastic catheter with the end cut off. Later a rubber drain 20 cm. long of No. 30 size was introduced on a long sound. Fortunately this manœuvre was successful in avoiding a fresh operation. The upper pleural pocket was rapidly emptied and could be efficiently irrigated. In three days the temperature fell to normal, where it remained. His

appetite became excellent, weight steadily rose, in six weeks, from 60·500 kg. to 67·0 kg., his general condition rapidly improved, in fact he was a complete resurrection.

He left the Queen Victoria Memorial Hospital on May 4, returning to England, but still retaining a No. 20 drain, from which, however, very little pus oozed. Recent news from him reports that his fistula has closed and that he is in as good health as he ever enjoyed.

Here we have a patient presenting a generalized gonococcal infection of a very grave character, which went through a period of evolution that we can divide into five distinct periods: First, simple urethral gonorrhœa; second, renal infection, with gonorrhœal nephritis; third, general infection; fourth, localization as a pleurisy; fifth, empyema.

Notwithstanding prompt and apparently efficient surgical intervention, the pleura was not freely liberated of its purulent contents. The full drainage was ineffectual on account of an encysted empyema forming pockets of pus due to adhesions, which may possibly have been favoured by adopting Von Bier's method. The principal impediment seems to have been in the upper portion of the pleuritic cavity. Each time that the full flow of pus was arrested the patient got much worse, and immediately a purulent flush occurred the general state improved. This complication, which invalided our patient for so long a period, maintaining high fever and a most disquieting state, is quickly cured by a more complete drainage, aided in its execution by endoscopic methods. Neither antigonococcic serum, electro-argol, nor a good deal of purified santal oil (arheol), which were given fair trials, seemed to show any influence on the malady. Whether or not the serum given him raised his resisting power to destroy the infection later, as soon as the focus from which large doses of toxins were being daily absorbed was removed, is food for hypothetical speculation and for future investigation.

The important point in the observation of this case is the fact of its being a "unilateral purulent pleurisy," resisting treatments a priori suitable to such a case, which was cured entirely by surgical methods.

As a fact, generalized gonococci infections are not rare when we know how to track them. In the large majority of cases a general infection starts with an endocarditis. Next may occur a sero-fibrinous pleurisy, single or, more often, bilateral, in the exudate of which pure cultures of gonococcus may be found. Or this organism may be associated with other microbes, more especially with a staphylococcus. This sero-fibrinous variety is often curable by general treatment or simple puncture and aspiration, though this is not so with the purulent form, as evidenced in the above case. This form is, however, very rare;

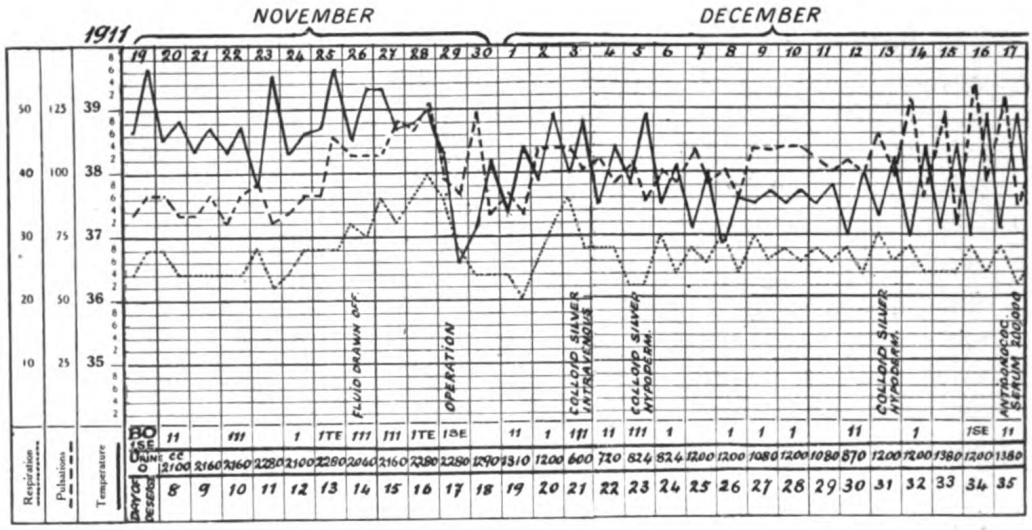
when it does occur, it is met with in gonococcic septicæmia of pyæmic type, and coincides with abscesses of other organs, or with subcutaneous ones. Usually they are only recognized post mortem. In our researches and in the documents kindly placed at our disposal by our colleagues Dr. Froin and Dr. Faure-Beaulieu, we have not found a single case of purulent pleurisy following as a single localization a gonococcic infection, and a fortiori cured by surgical intervention. Our case is, so far as our knowledge goes, the first one recorded.

When the case came under treatment there was absolute anuria (*see* chart). From the following day onwards the amount was within normal limits. The daily amount was recorded for the next forty days and only varied within the usual limits dependent upon the amount of drink, perspiration, &c. Neither during this period were any marked pathological constituents detected in the urine, except, of course, what was derived from the urethritis, which was very ill defined and eventually disappeared.

What is most striking is the lesson taught by the graphic curves of respiration, pulse and temperature (*see* chart). Throughout the whole illness there is marked concordance and parallelism between respiration and the pulse-rate. These, compared with the temperature, however, show that, with very few exceptions, as the temperature rose the pulse and respiration went down. Take, for instance, such a period as that from November 30 to January 5—thirty-six days—during which there was no exception to this rule. As each wave of high temperature occurred this reversal of the usual order of things was repeated, but as the temperature tended to approach normal, then the pulse tended to rise and fall symmetrically with the fever. As soon as the pocket of pus was evacuated and true convalescence began, then a parallel curve began also. We have the records up to May 4, but thought it of no interest to give them; at this date his weight had increased from 60·220 to 65·800 kg., or an increase of 5·580 kg. (over 12 lb.). All we need say is that these remaining records are characterized by the usual symmetry of curves.

What the mechanism may be by which such a perversion of the usual physiological order of things was brought about we are unable to explain, and shall be very grateful for any suggestions on the subject.

We have thought it justifiable to bring the case before the Royal Society of Medicine and to append the somewhat extensive chart. The effects of different treatments adopted are therein fully portrayed, and may serve as a guide or comparison to any colleague who meets with such a case.



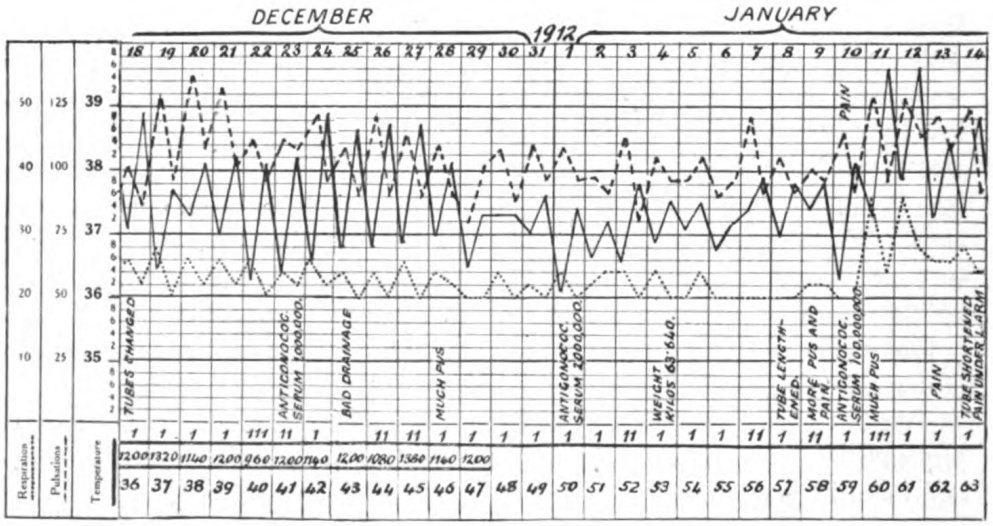
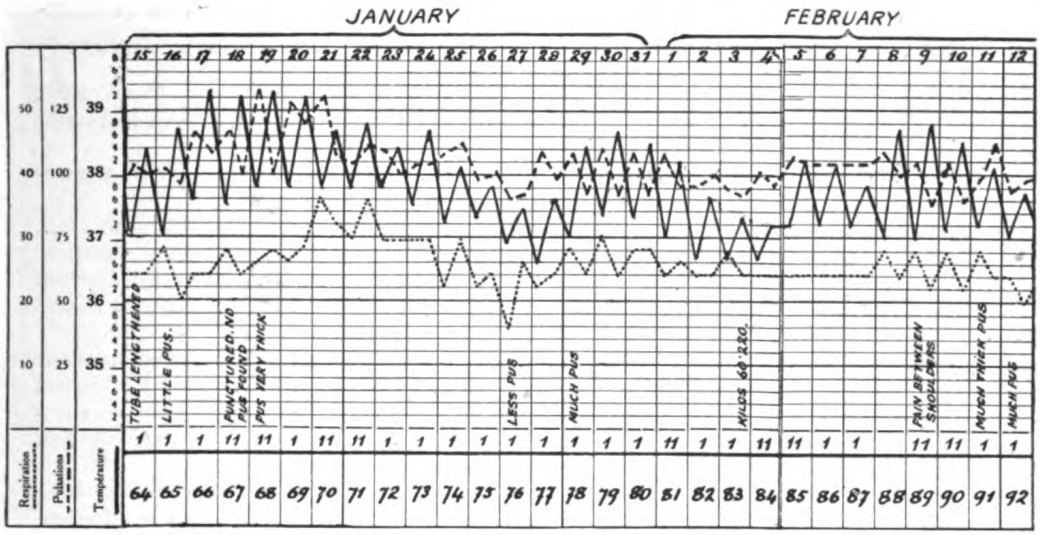
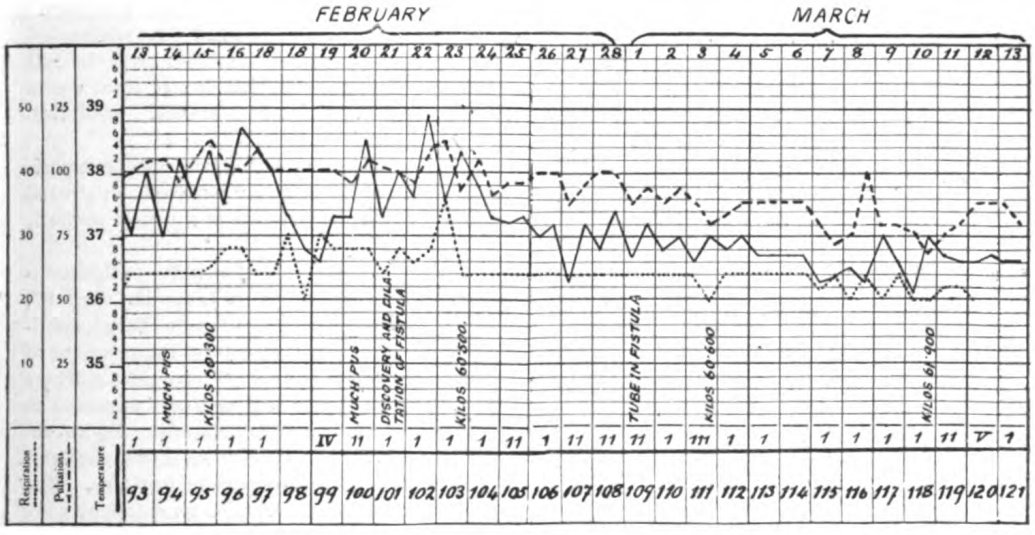


Chart of case of gonococcic empyema, showing





the respiration, pulse, and temperature.

The following are the principal bibliographical references that we have been able to collect. They are chiefly abstracted from the thesis of Dr. Faure-Beaulieu ("La Septicémie gonococcique prouvée par la Constatation du Gonocoque dans le Sang circulant," *Thèse*, Paris, 1906) :—

[1] BERTRAND. "Essaie sur la pleurésie dans la Blennorrhagie," *Thèse*, Paris, 1896. Pleurisy (right). In the exudate was found a diplococcus reacting to the usual stains as a gonococcus and not growing on the ordinary media.

[2] BORDONI-UFFREDUZZI, *Deutsch. med. Wochenschr.*, 1894, xx, p. 484, observed a gonococcal pleurisy as a complication of gonorrhœa with arthritis.

[3] CARDILE. "Sopra un caso di pleurite con gonococco di Neisser," *Clin. Med. Ital.*, Milano, 1899, xxxviii, p. 549. Female, aged 23. Gonorrhœa of six weeks' duration, rigors and fever, pain in right side with cough ; right pleurisy. Aspirated fourteen days later: fluid found to contain gonococci by culture and stain. Recovery in two and a half months.

[4] CHIAISO ed ISNARDI. "Sopra un caso di rheumatismo blennorragico con complicazione vicerali," *Giorn. R. Accad. Med. di Torino*, February, 1894. Girl, aged 10, infected by rape. One month after, fever, pain in left shoulder and right pleurisy, endocarditis. Recovery in a month from the pleurisy. Two pleural punctures—the first negative, the second containing diplococci.

[5] CROSBY. "Gonorrhœal Urethritis with Unusual Complications," *Amer. Journ. Med. Sci.*, Philad. and New York, 1905, n.s., cxxix, p. 880. End of three weeks infection, gonorrhœal stomatitis and rhinitis ; death in forty-eight hours. Post mortem found double pleurisy (1,000 c.c. or 40 oz.), purulent but thin fluid. Alveolar collection in lung, trachea and bronchi full of pus, pus in lower pelvis. No endocarditis. The gonococci were determined on morphological and staining characters, but not by colour.

[6] KRAUSE. "Zwei Fälle von Gonokokkensepsis mit Nachweis der Gonokokken im Blute bei Lebzeiten der Patienten," *Berl. klin. Wochenschr.*, 1904, xli, pp. 492-94. Publishes a case identical with Thayer's.

[7] MAZZA. "Un caso di poliseriosite de gonococco (Cultura dell'esudato pleurico)." *Giorn. R. Accad. di Med. di Torino*, Marzo, 1894. Girl, aged 11, contracted gonorrhœa, result of rape. Fourteen days after, general malaise, pain in left shoulder and articulations. Eight days later, bilateral pleuritic fluid; eight weeks later aspirated, in which gonococci were detected. Endo-pericarditis. Evolution and result not mentioned.

[8] PALDROCK. "Der Gonokokken Neisseri. Eine literarische und bakteriologische experimentelle Studie," Dorpat, 1907. It is recorded on p. 91 that "Sanarelli cultivated on Wertheim's medium gonococcus derived from pleural exudate of a young patient, aged 11."

[9] PROCHASKA. "Bacteriologische Untersuchungen bei gonorrhoischen Allgemeininfektionen," *Deutsch. Arch. klin. Med.*, Leipz., 1905, lxxxiii, p. 184. H., aged 37, dyer, contracted gonorrhœa at the end of 1904 ; disappearance of the discharge, epididymitis with constant fever (40° C.), which persisted notwithstanding improvement of the former and absence of other local lesions. Blood examination gave pure cultures of gonococcus. A month later, in the left infra-scapular area dullness appeared at level of the sixth rib, together with diminution of respiratory sounds. Aspiration of the pleura gave issue to a clear serous fluid which, by culture methods, gave rise to gonococcal colonies, whereas inoculation tests on guinea-pigs were negative. The patient was treated for a long time with clysters of collargol. The temperature only fell slowly, and the patient only recovered very gradually. During convalescence a sudden rise of temperature occurred (40° C.). Two months later the patient left the hospital, having lost his symptoms of pleurisy.

[10] THAYER and LAZEAR. "A Second Case of Gonorrhœal Septicæmia and Ulcerative Endocarditis," *Journ. of Exper. Med.*, New York, 1899, vi, pp. 81-116. The patient had double pleurisy, the exudate of which showed gonococci by direct observation.

PROCEEDINGS

OF THE

ROYAL SOCIETY OF MEDICINE

VOLUME THE FIFTH

COMPRISING THE REPORT OF THE PROCEEDINGS FOR THE SESSION 1911-12

DERMATOLOGICAL SECTION



LONDON
LONGMANS, GREEN & CO., PATERNOSTER ROW
1912

Dermatological Section.

OFFICERS FOR THE SESSION 1911-12.

President—

Sir MALCOLM MORRIS, K.C.V.O., F.R.C.S.Ed.

Vice-Presidents—

H. A. G. BROOKE, M.B.
T. COLCOTT FOX, M.B.
JAMES GALLOWAY, M.D.
J. HERBERT STOWERS, M.D.

Other Members of Council—

H. G. ADAMSON, M.D.
R. A. BOLAM, M.D.
J. L. BUNCH, M.D.
S. E. DORE, M.D.
W. T. FREEMAN, M.D.
E. GRAHAM LITTLE, M.D.
J. E. R. MCDONAGH, F.R.C.S.
J. M. H. MACLEOD, M.D.
GEORGE PERNET, M.D.
E. STAINER, M.B.
A. WINKELRIED WILLIAMS, M.B..
KENNETH WILLS, M.B.

Hon. Secretaries—

J. H. SEQUEIRA, M.D.
WILFRID FOX, M.D.

Representative on Library Committee—

T. COLCOTT FOX, M.B.

Representative on Editorial Committee—

J. H. SEQUEIRA, M.D.

DERMATOLOGICAL SECTION.

CONTENTS.

October 19, 1911.

PAGE

Onychia Sicca Syphilitica associated with Total Alopecia (Alopecia Areata Type). By H. G. ADAMSON, M.D. 1

Granuloma Annulare in a Boy, aged 2½. By J. L. BUNCH, M.D. 3

By WILFRID FOX, M.D.

(1) Two Independent Primary New Growths on the Face, one Pigmented and the other not, in a Woman, aged 72 5

(2) Darier's Disease in a Married Woman, aged 30 7

Keratodermia Blenorrhagica in a Young Man. By E. G. GRAHAM LITTLE, M.D., and S. R. DOUGLAS 8

By E. G. GRAHAM LITTLE, M.D.

(1) Hæmochromatosis with Diabetes 10

(2) Dermatitis Artefacta 10

(3) Case for Diagnosis 11

Hæmorrhage into the Nail-matrices and Nail-beds of the Finger-nails. By J. M. H. MACLEOD, M.D. 13

Elephantiasis of the Lip. By G. NORMAN MEACHEN, M.D. 14

Tuberculides in a Girl, aged 5. By J. H. SEQUEIRA, M.D. 15

Note on the Ætiology of Leprosy. By WILLIAM TURNER, M.D. (Gibraltar) ... 17

November 16, 1911.

By J. L. BUNCH, M.D.

(1) Atrophic Tuberculide 21

(2) Multiple Angiomata 23

(3) Urticaria Pigmentosa in a Baby, aged 10 months 24

Lupus Erythematosus localized on the Scalp. By T. COLCOTT FOX, M.B. ... 24

Case of Vitiligo. By E. G. GRAHAM LITTLE, M.D. 25

By J. M. H. MACLEOD, M.D.

(1) Granuloma Annulare in a Little Girl, aged 4 27

(2) Case for Diagnosis 29

PAGE

Paget's Disease of the Umbilicus cured by the Application of Radium. By W. A. Milligan, M.D 30

Extensive Ringworm of the Trunk and Extremities, with Granulomatous Formations. By J. H. Sequeira, M.D. 33

A Discussion on Eczematoid Ringworm of the Extremities and Groins. Opened by:—(I) Arthur Whitfield, M.D. (II) Dr. R. Sabouraud 36

Dr. J. J. Pringle (p. 52)—Dr. G. Pernet (p. 54)—Dr Colcott Fox (p. 55)—Dr. H. G. Adamson (p. 56)—Dr. J. L. Bunch (p. 57)—Dr. S. E. Dore (p. 58)—Dr. Sequeira (p. 58)—Dr. Graham Little (p. 59)—Dr. Bolam (p. 60)—Dr. A. M. Gray (p. 60)—Sir Malcolm Morris, K.C.V.O. (p. 61).

December 14, 1911.

Pityriasis Lichenoides Chronica or Lichen Variegatus. By Wilfrid Fox, M D. 63

By E. G. Graham Little, M.D.

(1) Recurrent Granuloma Annulare 65

(2) Case for Diagnosis 65

Culture of Fungus from Hairs affected with "Piedra." By J. M. H. MacLeod, M.D. 66

Syphilitic Elephantiasis of the Scrotum (Lymphangitis). By J. E. R. McDonagh, F.R.C.S. 67

Case of Nævus. By Sir Malcolm Morris, K.C.V.O., F.R.C.S.Ed., and S. E. Dore, M.D. 70

By J. H. Sequeira, M.D.

(1) Lupus Vulgaris in a Syphilitic Subject 71

(2) Microscopical Section from a Case of Extensive Ringworm with Granulomatous Formations 73

(3) Culture of Achorion Quinckeanum (Mouse-favus) obtained from a Girl, aged 5 years 74

By Arthur Shillitoe, F.R.C.S.

(1) Case of Syphilis 74

(2) General Small Lichenoid Syphilide 75

By A. Winkelried Williams, M.B.

(1) Acne Agminata 75

(2) A peculiar Nævus 76

January 18, 1912.

By E. G. Graham Little, M.D.

(1) Striæ Cutis Distensæ 77

(2) Case for Diagnosis 79

Case for Diagnosis. By J. M. H. MacLeod, M.D. 80

Case for Diagnosis. By G. Norman Meachen, M.D. 81

PAGE

By J. H. Sequeira, M.D.

(1) Case for Diagnosis 83

(2) Cultures of Trichophyton Plicatile from a Case of Extensive Ringworm of the Trunk and Extremities, with Granulomatous Formations 84

(3) Extensive Nævus affecting chiefly the Left Side of the Body and partially the Right Side of the Neck in a Girl, aged 14 85

Sequel to Case of Chronic Artificial Skin Eruption. By F. Parkes Weber, M.D. 87

February 15, 1912.

Extraordinary Pigmentation of the Face. By Haldin Davis, F.R.C.S. ... 89

Small Linear Nævus on the Palm of the Hand of a Girl, aged 5. By S. E. Dore, M.D. 91

By E. G. Graham Little, M.D.

(1) Two cases of Alopecia Universalis 91

(2) Case for Diagnosis 92

Leucodermia Syphilitica with Central Atrophy. By J. E. R. McDonagh, F.R.C.S. 94

An Unusual Case of Cicatrizing Folliculitis of the Scalp in a Woman, aged 55. By Sir Malcolm Morris, K.C.V.O., F.R.C.S.Ed., and S. E. Dore, M.D. ... 94

Alopecia Universalis with Dystrophy of the Nails. By J. H. Sequeira, M.D. ... 96

By A. Winkelried Williams, M.B.

(1) Case for Diagnosis 96

(2) Morphœa Guttata 97

The Practical Use of Eucerin as an Ointment Basis. By Dr. Eugen Unna (Hamburg) 97

March 21, 1912.

Raynaud's Disease associated with Calcareous Degeneration. By Haldin Davis, F.R.C.S. 99

Congenital Xanthoma. By J. L. Bunch, M.D. 101

Case showing an Abnormal Condition of the Nails of the Hands associated with Secondary Carcinomatosis. By E. Greaves Fearnsides, M.B. 102

Hebra's Prurigo in a Baby Girl, aged 15 months. By T. Colcott Fox, M.B. ... 104

Dermatitis Herpetiformis. By Sir Malcolm Morris, K.C.V.O., F.R.C.S.Ed. ... 105

Xantho-erythrodermia Perstans (Crocker-Pernet). By George Pernet, M.D 106

By J. H. Sequeira, M.D.

(1) Case for Diagnosis 107

(2) Lichen Planus Annularis 109

Double Cervical Ribs associated with Vascular Phenomena suggesting Raynaud's Disease. By E. G. Fearnsides, M.B., and J. H. Sequeira, M.D. ... 110

Sections of the Skin of a Kitten affected with Microsporon Ringworm, and a Culture from the Hair of a Child infected by the Kitten. By A. Whitfield, M.D. 113

April 18, 1912.

PAGE

Multiple Benign Cystic Epithelioma. By S. E. Dore, M.D. 115

Case for Diagnosis. By Wilfrid Fox, M.D. 117

Extensive Pigmented Hairy Mole on the Trunk and Small Pigmented Moles on the Face and Limbs in a Female Child, aged 2 years. By S. E. Dore, M.D. 118

Cases with Characteristics of Lupus Erythematosus. By J. M. H. MacLeod, M.D. 120

Bromide Eruption. By G. Norman Meachen, M.D.... 122

Fibromata in a Girl, aged 17. By Sir Malcolm Morris, K.C.V.O., F.R.C.S.Ed. 123

Sclerodactylia with Subcutaneous Calcification. By R. E. Scholefield, M.D., and F. Parkes Weber, M.D. 124

By J. H. Sequeira, M.D.

(1) Case of (Infective) Angioma 128

(2) Case for Diagnosis 129

Tropho-neurotic Separation of Nails, followed by Alopecia Areata. By F. Parkes Weber, M.D. 129

Case for Diagnosis. By A. Whitfield, M.D. 130

May 16, 1912.

By H. G. Adamson, M.D.

(1) Persistent Circinate Papulo-necrotic Eruption (? Tuberculide) ... 131

(2) (?) Tuberculide of the Type called "Sarcoid" of Darier-Roussy ... 132

By J. L. Bunch, M.D.

(1) Lupus Erythematosus 133

(2) Generalized Ichthyosis in a Girl, aged 14 134

By S. E. Dore, M.D.

(1) Case for Diagnosis 135

(2) Tuberculide in a Boy, aged 10 135

(3) Hydroa Vacciniforme vel Æstivale in a Boy, aged 8 136

Case of Pseudo-pelade. By Wilfrid Fox, M.D. 136

Eruption due to Ferrous Iodide. By E. W. Goodall, M.D. 138

Case for Diagnosis (Ferrous Iodide Rash). By Sir Malcolm Morris, K.C.V.O., F.R.C.S.Ed. 140

Case of Syringoma. By J. E. R. McDonagh, F.R.C.S. 140

Maculo-anæsthetic Leprosy in a Woman, aged 25. By J. M. H. McLeod, M.D. 142

Lupus Vulgaris and Scrofulodermia treated by the Pfannenstill Method. By J. H. Sequeira, M.D. 144

Erythema Nodosum, associated with Mammary Tuberculosis. By F. Parkes Weber, M.D. 145

Case of Ichthyosis. By A. Winkelried Williams, M.B. 146

June 20, 1912.

By S. E. Dore, M.D. PAGE

(1) Circumscribed Lichenification (Névrodermite) 147

(2) Acquired Syphilis in a Girl, aged 10 147

Multiple Scars on the Body and Limbs, and Partial Destruction of the Ears. By Willmott Evans, F.R.C.S. 148

Case of ? Potassium Iodide Eruption. By A. M. H. Gray, M.D. 149

Case of ? Erythema Multiforme. By E. G. Graham Little, M.D. 150

Xanthoma Tuberosum Multiplex associated with Peculiar Osteo-arthritic Changes in the Joints. By J. M. H. MacLeod, M.D. 151

Alopecia Areata et Totalis Cured by Pregnancy, and Relapsing with the Re-establishment of the Menses. By G. Norman Meachen, M.D., and F. L. Provis, F.R.C.S.Ed. 152

Ichthyosis with Late Development. By Sir Malcolm Morris, K.C.V.O., F.R.C.S.Ed., and S. E. Dore, M.D. 154

Case of ? Urticaria Perstans Annulata et Gyrata. By J. J. Pringle, M.B. ... 156

By J. H. Sequeira, M.D.

(1) Two Cases of Pityriasis Rubra Pilaris 158

(2) Solar Epitheliomatosis (late Xerodermia Pigmentosa) in a Man, aged 33 160

By A. Whitfield, M.D.

(1) Generalized Sarcomatosis in a Man, aged 62 161

(2) Developing Systematized Nævus on the Back and Front and Arms in a Boy, aged 8 months 163

A Discussion on Prurigo, Pruriginous Eczema, and Lichenification. Introductory address by Sir Malcolm Morris, K.C.V.O., F.R.C.S.Ed. ... 164

Dr. Colcott Fox (183)—Dr. James Galloway (185)—Dr. Leslie Roberts (187)—Dr. Whitfield (188)—Dr. Stainer (190)—Dr. Graham Little (191)—Dr. J. M. H. MacLeod (192)—Dr. Bunch (193)—Dr. Dyson (193)—Dr. Douglas Heath (194)—Dr. G. Pernet (196)—Dr. Adamson (198)—Dr. Sequeira (198)—Sir Malcolm Morris (reply) (199).

July 18, 1912.

Ichthyosis. By Sir Malcolm Morris, K.C.V.O., F.R.C.S.Ed. 201

Nævus Unius Lateris of Skin and Mucous Membrane. By J. H. Sequeira, M.D. 201

Case for Diagnosis. By A. Whitfield, M.D. 203

Acne Scrofulosorum (Papulo-necrotic Tuberculide) with Episcleral Tubercle. By H. G. Adamson, M.D. 204

Case for Diagnosis. By A. M. H. Gray, M.D. 205

Keloids of the Ears following Piercing for Ear-rings. By J. M. H. MacLeod, M.D. 206

Two Cases of Summer Eruption. By Haldin Davis, F.R.C.S. 206

Drawing and Photograph of a Case of Mycosis Fungoides d'emblée. By G. Pernet, M.D. 207

The Council think it right to state that the Society does not hold itself in any way responsible for the statements made or the views put forward in the various papers.

LONDON:
JOHN BALE, SONS AND DANIELSSON, LTD.,
OXFORD HOUSE,
GREAT TITCHFIELD STREET, OXFORD STREET, W.

Dermatological Section.

October 19, 1911.

Sir MALCOLM MORRIS, K.C.V.O., President of the Section, in the Chair.

Onychia Sicca Syphilitica associated with Total Alopecia (Alopecia Areata Type).

By H. G. ADAMSON, M.D.

The patient was a married woman, aged 47. She had begun to get bald in patches (*alopecia areata*) fifteen years ago, and had been totally bald for thirteen years. The nails had been affected only for six months. Six months ago they had commenced to be roughened at their proximal ends. Now each nail was completely involved, being thickened, opaque, roughened, furrowed and ribbed, and somewhat raised from its base by a dense thickening beneath. Every nail was affected to an equal extent, finger-nails and toe-nails alike. The patient had lost all her teeth at the age of 20. There was a slight but widespread chronic superficial glossitis.

The exhibitor regarded the nails as an example of *onychia sicca syphilitica*, and this diagnosis was supported by the fact that the blood-serum had given a positive reaction to the Wassermann test, and also by the patient's history. She had been married sixteen years. Two years after marriage she had had a miscarriage. Then one year later a boy, now living and healthy (aged 13). Six years after marriage a second miscarriage, and then a girl, now living (aged 10), a hydrocephalic idiot in Dartford Asylum. A Wassermann test of the boy's blood had given a negative reaction, but there had been no opportunity of examining the girl's blood. A point of interest in the case was the



FIG. 1.

Total alopecia.



FIG. 2.

Dystrophy of the nails in the same patient.

occurrence of alopecia areata, followed by total alopecia in a syphilitic patient, especially in view of Sabouraud's recent finding of 10 per cent. positive Wassermann reactions in cases of alopecia areata. In the case exhibited, in spite of the nail trouble and the positive Wassermann, one could not definitely associate the alopecia with syphilis, for the onset of the alopecia dated so far back that it was quite possibly earlier than the syphilitic infection. The exhibitor thought that it merely pointed to a susceptibility of the skin appendages—teeth, hair, and nails—to noxious influences. In the case of the nails, syphilis had been the offending agent; in that of the hair and teeth, possibly some other cause. It was interesting to note that the patient's elder sister had been totally bald since she was 20 years of age, and this fact possibly pointed to a family sensitiveness of the skin appendages.

Ringworm of the nails had been excluded by careful examination of filings.

Granuloma Annulare in a Boy, aged 2½.

By J. L. BUNCH, M.D.

WHEN the patient first came under his care, some two months ago, there was a well-defined, raised ringed eruption on the dorsum of the right foot, which had already been present some two or three months. This eruption consisted of a single continuous ring, about 1 in. in diameter, composed of solid, definitely elevated and deeply seated tissue, which felt firm to the touch, and in places was appreciably nodular. Nodules were, in fact, distinctly visible to the eye at some parts of the ring, and, like the rest of the ring, had a smooth, pale surface, perhaps a trifle glistening. In addition to this complete ring, and situated 1½ in. away from it, was an irregular nodular mass about the size of a pea, which when first seen two months ago was said to have been present only a fortnight. During the interval this lesion had increased in size, spreading slightly at the ends until it tended to form a crescent. To the touch there was no appreciable difference in structure from that of the larger ring. As far as could be made out from the history, the ring had originated as one or more nodules, which had coalesced with later nodules to form a more or less circular patch such as was now present. If there had at any time been any nodules within the circumference of the ring—and there appeared to be traces of such nodules remaining

—these had undergone considerable involution, leaving more or less normal skin in the centre of the ring.

The child seemed to suffer no inconvenience from the presence of the lesions, nor were any subjective symptoms apparently present.

Nothing of any importance could be elicited in the family history to throw light upon the eruption.

Attention was drawn to the youth of the patient, which was unusual in this disease, even if it was not the youngest case on record.

While not contending that the lesions were structurally anything more than a deep cellular inflammation with infiltration of the type seen in chronic inflammatory processes of the skin such as is seen in the lichen group, or that they showed anything of the charac-



Granuloma annulare.

teristic changes of a granuloma, the exhibitor still preferred to call the disease granuloma annulare. The name celluloma annulare, which had been suggested, might be applied to many skin diseases, and was quite as unsatisfactory a name as lichen annularis, sarcoid tumour, or Audry's name of érythémato-sclérose circinée du dos des mains.

DISCUSSION.

Dr. PERNET said he had suggested the term "Celluloma annulare." He had shown long ago the condition was not a granuloma.

The PRESIDENT (Sir Malcolm Morris) remarked that this was a term which the Nomenclature Committee would probably have before it.

Dr. COLCOTT FOX said he had seen the condition in babies, and he demonstrated it at the old Dermatological Society of London. He saw a woman who was under the care of Dr. Radcliffe-Crocker many years ago, and the condition was called lupus erythematosus. That woman had still got it, though he saw her first twenty years ago.

Dr. BUNCH, in reply, said he had hoped for suggestions as to the ætiology. He thought the Americans had definitely adopted the name granuloma annulare.

Two Independent Primary New Growths on the Face, one Pigmented and the other not, in a Woman, aged 72.

By WILFRID FOX, M.D.

THE history of the case was as follows: On the left cheek, just underneath the eye, was a white mole, which had been present, as far as the patient could remember, all her life. Fifteen years ago this mole began to bleed readily and ulcerated. This slowly increased, until the ulcer was, at the present time, about the size of a shilling, with raised, rolled edges, and fairly typically epitheliomatous in aspect. There were no glands to be felt, and this growth had remained unpigmented throughout. Twelve or thirteen years ago she noticed on the other cheek, situated a little lower than that on the left cheek, a dark, pigmented, freckle-like patch. This slowly spread, the pigmentation becoming rather deeper, until the pigmented area was, at the present moment, about 2 in. in diameter and roughly circular in shape. Three years ago a small raised nodule appeared in the centre of this pigmented area about the size of a cherry-stone. This began to ulcerate about three months ago, and at the present moment there was a little exudation coming from it and a small adherent scab. On this side also there were no enlarged glands to be felt. A biopsy had been made from both the growths, and sections from these, which were kindly stained by Dr. Chipman, showed epithelioma in both cases. The pigmented one was, in the exhibitor's opinion, clearly a carcinoma, and not a melanotic sarcoma, as these conditions used to be called in the older text-books. The epidermal downgrowths were quite evident. It had been noted in the section under the microscope that the carcinomatous condition extended not only under the nodule, but also,

as far as the section went, under this flat pigmented area at the side. The patient had recently had exposure from radium, so that one of the lesions was at the present moment undergoing reaction from that cause.

DISCUSSION.

The PRESIDENT remarked that he had seen cancer change from the seamen's type to the ordinary type. The subject would prove a very interesting one for a set discussion. Not long ago he saw a very bad case in the person of a medical officer in the Indian Service, aged about 60. He had three epithelial growths, which became rapidly malignant, and he died in a few months.

Dr. MACLEOD said that the case was of great interest as it belonged to the important group of the "nævo-carcinomata." He considered that the fact that one of the lesions was pigmented while the other was not definitely so could be explained by the co-existence of pigmented and non-pigmented moles, which was not uncommon.

Dr. COLCOTT FOX considered that it was the affection to which Sir Jonathan Hutchinson drew attention years ago, the cancer coming on in elderly people in the face and giving rise to such an appearance. In seamen's cancer there was also a keratotic condition preceding the cancer.

Dr. PERNET said that not long ago he had a private case in which a cancerous growth started from a pigmented mole on the big toe. It became irritated in some way, and when seen by him it was involving a large part of the end phalanx of the big toe, and was evidently malignant. The rapidity of the development and evolution of these cases depended, he thought, on age. The older the patients were the more likely was the condition to develop slowly. He considered Dr. Fox's case differed from the condition known as sailor's skin, though he had seen a growth very like the one on the left side of the nose of the present patient, only much larger, develop on the dorsum of the hand of an old sailor—a large epithelioma. Sailor's skin was more like what was observed in X-ray cases.

Dr. WILFRID FOX, in reply, thought this condition was quite separate from seamen's cancer. The remarkable points which struck him in the case were that, first of all, two primary independent growths in one subject should occur within a short space of one another; secondly, that the pigmented one did not, as far as could be made out, arise from a mole, as was usually the case, whereas the one which did arise from a mole was unpigmented; and, thirdly, the important part which the pigment seemed to take in the melanotic growth as a forerunner of the carcinoma.

Darier's Disease in a Married Woman, aged 30.

By WILFRID FOX, M.D.

The patient had had the condition since the age of 14. It started on the forehead and then spread to the front of the chest and gradually from there out to the two axillæ, down the front of the abdomen on to the legs. When the case first came up it looked more like a very extensive seborrhœic eruption with small seborrhœic warts. There were no subjective symptoms, but the condition had got distinctly worse since the woman married three years ago. The exhibitor thought the case was one very similar in aspect to that figured in the Jacobi-Pringle atlas. A biopsy had been made, and sections kindly stained by Dr. Chipman were shown under a microscope. These showed the characteristic horny plug and the pseudo-sporosperms which have been described by Darier in this condition. The patient had had sulphur and salicylic acid ointments in the way of treatment, and there was some improvement as regards the extremely greasy condition. Recently she had been applying vasogen iodine, 1 dr. to the ounce of liquid paraffin, and the exhibitor thought that the improvement had been more rapid under this. He would be glad of any further suggestions as to treatment. Her general health was normal.

DISCUSSION.

Dr. MACLEOD said that there was great difficulty in accounting for the peculiar changes in the epidermis which characterize Darier's disease. They seemed to be caused by some interference with the process of cornification, and somewhat resembled those which occur in molluscum contagiosum. He wondered if the affection could be explained as an infective process on a seborrhœic soil.

The PRESIDENT agreed with Dr. MacLeod that it looked as if there was an infective process in the case.

Keratodermia Blenorrhagica in a Young Man.

By E. G. Graham Little, M.D., and S. R. Douglas.

The patient, a policeman by occupation, aged about 30, had been admitted to the Inoculation Department at St. Mary's Hospital under Captain Douglas about six weeks previously, suffering from severe arthritis of both knee-joints which was diagnosed as gonorrhœal. The patient had urethritis, in which, however, it had not been found possible to find the gonococcus, at the time of admission; he had had gonorrhœa for some weeks, and had apparently had another attack of the same disease two years before. His temperature was very irregular, rising to 102° F. and higher, and he was constitutionally ill. He showed in addition the following remarkable condition of the skin: On the soles of both feet there were very numerous conical profusions of translucent, brown, horny aspect, raised from ¼ in. to ½ in. from the skin, and surrounded at their base by a faint inflammatory areola. These brown masses would be dislodged after a time, leaving a red base, the projections forming rows which justified the French description which compared them to the mountain ranges on a relief map. On the dorsum of the feet there were several rupia-like scabs, and on the backs of the elbows and on the hands there were similar scabs. The nails of the hands were affected, being partly dislodged from the matrix, which was inflamed and showed red under the nail. The man was anæmic and feeble and unable to stand.

He had been treated with injections of a stock gonococcal vaccine, autogenous vaccine being unprocurable, and had improved very greatly, his temperature falling rapidly to normal, the arthritis disappearing, and the skin lesions being greatly diminished.

The Wassermann reaction had been shown to be negative.

The patient described an eruption of warty scabs as having appeared on his elbows after the gonorrhœal attack of two years ago, but it had not then shown itself on the feet.

This case recalled very faithfully the appearance of the case shown by Dr. Sequeira, which was the first shown in England.[1] The condition was certainly infinitely rare.

[1] *Proceedings*, 1910, iii, p. 77.

DISCUSSION.

Captain DOUGLAS remarked that the patient had two or three patches on the left knee when he came in, but they were now quite cleared up. Also, on arrival at the hospital his temperature was 100° F. to 101° F., but it was now normal, and the knees were rapidly clearing. No gonococci had been found in the lesions, though the patient was ill.

The PRESIDENT remarked that he believed this case was only the third of its kind which had been shown before the Section. As Dr. Sequeira had shown a similar case, members would be glad to hear his comments.

Dr. SEQUEIRA said[1] the condition which had been described was slightly different from that in his own case, for in the latter the masses on the feet were darker, of a purplish-brown in the old or central parts, and the margin had the horny character described by Dr. Little. His own case was first under the care of Dr. Fred Smith. The patient had a very bad gonorrhœal arthritis, and was very ill with it. He was septic, and for some weeks had a temperature. Three months after the onset the keratomatous masses peeled off. When he looked into the literature of the subject he found that this was the usual history. It was interesting to note that the patient in one of the cases recorded had had six attacks of gonorrhœa. In the first two attacks he had urethritis only; in the second two attacks he had both urethritis and arthritis, and in the last two he had urethritis, arthritis, and keratodermia. That seemed to show that the condition was gonorrhœal. In his own case the treatment followed was by means of a stock gonococcal vaccine. But there had been no gonococci found when searched for in the urethral discharge. That seemed to be the case here also. In the history of the cases the discharge appeared to have been suppressed very early; the urethritis dried up very rapidly, and was quickly followed by general pyæmic symptoms, with joint affection.

Dr. ADAMSON asked whether the condition of the nails here seen had been described. They were evidently part of the same conditions and due to the formation of lesions on the nail-beds.

Dr. WINKELRIED WILLIAMS remarked that in his case[1] the lesions were yellow. The joints were affected. There was pain in one ankle, but no effusion.

Dr. GRAHAM LITTLE, in reply, said that some of the lesions had the kind of purplish areola which Dr. Sequeira described in his own case. One suggestion which had been made was that the condition might have been syphilitic, but the Wassermann reaction had been done, and it proved negative.

[1] *Proceedings*, 1911, iv, p. 12.

Case of Hæmochromatosis with Diabetes.

By E. G. Graham Little, M.D.

The patient was under the care of the exhibitor's colleague, Dr. R. H. Miller, to whom the Section owed its thanks for permission to show the case. He was a man, aged about 35, who gave the history of having noted the pigmentary changes in his skin some eighteen years previously; his skin had become a deep slate colour, especially dark in tint upon the face, the hands, the neck, the axillæ, umbilicus, groin and perineum, and popliteal spaces. There were also deep guttate pigmentations of his hard palate. His health had not, however, appeared to suffer greatly until some three months ago, when he began to lose weight rapidly; he came under observation some weeks after this, and was found to be suffering from marked glycosuria, probably diabetic in causation, from 2 to 3 per cent. of sugar being constantly present in his urine. His emaciation was now extreme. He passed some five or six pints of urine daily.

His liver was enlarged to nearly the level of the umbilicus, but not tender upon pressure. There was no other visceral symptom. The patient thinks the skin is less dark in colour since his wasting began. The shade now present is that of slate. The French expression "diabète bronzé" might be considered very justifiably applied to this case. Instances of this disease were rare; in Winkelried Williams's invaluable index to the *British Journal of Dermatology* there is no record of a case shown at the Dermatological Societies in the period covered by that work (1888-1909).

Case of Dermatitis Artefacta.

By E. G. Graham Little, M.D.

The patient was a girl, aged 12. She commenced to menstruate about nine months ago, and has had five periods. One of these lasted for four months. She began to show skin lesions about three months ago. They were always of the same character—very superficial abrasions with the long axis in the axis of the arm, situated on the

forearms and hands. These lesions always occurred during the night, and the patient ran to show them to her mother. She had three new lesions that day, all on the right forearm; but there had been others previously on the left forearm and on the left hand.

The soft palate and pharynx were completely anæsthetic. The means of producing the lesions had not been ascertained, but they all bore the appearance of friction with a wet finger.

DISCUSSION.

Dr. COLCOTT FOX remarked that there had recently been in Westminster Hospital, in Mr. Stonham's wards, a very similar case, that of a young girl with complete hemianæsthesia, including the palate. The story was that there was probably some genuine inflammation about a finger-nail. When this got well she had a curious-looking inflamed pustular patch on the back of the same hand. She became hemianæsthetic, and the day after coming into the hospital she developed one of those oblong excoriations on the arm. She said she was becoming paralysed on one side, but when she was told she could not have her dinner unless she used that arm, she ate her dinner without difficulty. The lesion was produced by friction.

Dr. PERNET said he had demonstrated pharyngeal anæsthesia in such cases a good many years ago. He had been under the impression that it was a matter of common knowledge. He had suggested the possibility of an alteration of personality in some of these cases.[1]

Case for Diagnosis.

By E. G. GRAHAM LITTLE, M.D.

E. W., AGED 60, has had an affection of the skin dating from two years ago, when she fell and bruised the right buttock. The first swelling appeared on the thigh below the injured buttock, the skin of which had not been abraded. The swelling came two to three months after the injury. There was no pain, but a swelling which disappeared without becoming inflamed, to be followed by others. Some of these painless and colourless swellings became reddened and broke down in parts. Now she has several cribriform ulcerations on an indurated base which is reddened and painful. These open

[1] Pernet, "The Psychological Aspects of Dermatitis factitia," *Trans. Amer. Derm. Assoc.* (Philadelphia, 1909), p. 20.

ulcerated areas are on the left upper arm, the left thigh, and the right thigh. There are also several colourless or faintly blue indurated subcutaneous swellings on the thigh, the buttocks, and the upper arms. The broken areas have developed only within the last three months.

She has had seven children, all alive and well. She had one miscarriage between the first and second. The Wassermann reaction has been shown to be negative. Pus, examined with naked eye and microscopically, unstained, showed no actinomycosis or sporothrix. The urine was of specific gravity 1015, and contained neither sugar nor albumin.

DISCUSSION.

Dr. ADAMSON remarked that the lesions on the buttock, which consisted of grouped, deep-seated nodules, were suggestive of the hypodermic sarcoid of Darier-Roussy. The punched-out ulcers of the lesions on the shoulder (recalling those of Bazin's disease) were also a feature of the Darier-Roussy sarcoid. So was the sex and age of the patient. Dr. Adamson suggested that the patient should be tested for a tuberculin cuti-reaction. The alternative diagnosis seemed to be sporotrichosis, but in that case there should be no difficulty in finding the sporothrix culture; but he was inclined to regard the case as a tuberculide of the "sarcoid" type.

The PRESIDENT said it was necessary to await the further results of investigation. His own idea was rather against the diagnosis of actinomycosis; unless there was general infection he did not see how such deep abscesses could be produced. He had seen tuberculides in elderly people, in whom there had been painful swellings, which had subsequently suppurated.

Dr. PERNET said in a case of the late Dr. Radcliffe-Crocker's, with a brawny infiltrated area with soft fluctuating points about it over the left hip, he had found a streptothrix (actinomycosis). Dr. Pernet had shown the preparations at a meeting of the old Dermatological Society of London.[1] The smears were stained by Gram eosin and showed the characteristic appearances of streptothrix, viz., mycelial elements very abundant, aggregated here and there into felted masses, but there were no rosettes of clubs such as occur chiefly in actinomycosis of cattle (actinomyces bovis).

Dr. GRAHAM LITTLE, in reply, said he examined the lesions carefully with the microscope, as well as with the lens. In one case which he had there was post-mortem evidence of an internal infection.

[1] *Brit. Journ. Derm.*, xvii, 1905, p. 265.

Case of Hæmorrhage into the Nail-matrices and Nail-beds of the Finger-nails.

By J. M. H. MacLeod, M.D.

The patient was a delicate-looking man, aged 43, by occupation hotel porter. At the age of 15 he had rheumatic fever, which caused organic disease of the heart. With this exception he had had no severe illnesses, and though there was a history of tuberculosis in his family he had no definite signs of tuberculosis himself, nor had he had syphilis. On examination it was found that the apex-beat of his heart was displaced outwards and downwards, and that a marked systolic murmur was present. He had also suffered for many years from a weak peripheral circulation. His hands and feet always felt cold, the tips of his fingers were slightly swollen and cyanosed, and he was a martyr to chilblains. He had no definite Raynaud's phenomena.

The hæmorrhages occurred about fourteen days ago. For three months previously he had been out of work, and had become run down from insufficient and improper food. He had then got temporary employment as a hotel porter, and was set to wash dishes in very hot water with soda in it, which he did for three hours. The same night his fingers became painful near the tips. The pain increased, and two days later he noticed that red patches were present "like blood beneath the nails." The hæmorrhage appeared first beneath the lunule, and spread forwards for varying distances, in two instances extending to the anterior border of the nail-bed. The fingers were all affected and practically simultaneously. On exhibition the hæmorrhages were still evident as purplish patches beneath the nails involving more or less of the nail area.

The effect of the immersion in the hot water had been to cause a sudden dilatation of the blood-vessels of the nail-matrix and nail-bed, which, owing to the impaired circulatory conditions at the finger-tips, had put too great a strain on the capillaries, and led to the hæmorrhage.

DISCUSSION.

Dr. Pringle said that not long ago he was a witness in a County Court in connexion with a case of precisely similar nature. The man was a kitchen porter in the employ of a large hotel, and came to the Middlesex Hospital with

hæmorrhages below and round the nails. He not only gave a history of circulatory disturbances such as Dr. MacLeod narrated, but they were of great degree, and he had marked syncope alternating with asphyxia of the extremities; in other words, he was a subject of Raynaud's disease. He came to the hospital twice, and seven months afterwards he (Dr. Pringle) was subpœnaed to appear as a witness in the case, the man having brought an action against the hotel for damages, saying that the dirty water with which he was forced to work was the cause of the condition. The position he (Dr. Pringle) took was that the man's work was not the primary cause, but merely a contributory condition. Although other medical men gave a contrary opinion, the Court took his view.

The PRESIDENT pointed out that the heat of the water might have had something to do with the condition.

Dr. MACLEOD, in reply, said the patient had a bad circulation since 15 or 16 years of age. He was now 43, but had never had this condition before. He had been a porter at the hotel many years, his duties including the same kind of work, but he did not get the state of things he now showed until he returned to work after three months' lack of employment, when, no doubt, he was suffering from deficient nourishment.

Case of Elephantiasis of the Lip.

By G. NORMAN MEACHEN, M.D.

THE patient, who had kindly consented to appear before the Section, was a young married lady, aged 26. She consulted the exhibitor upon the day of the meeting, having been sent to him by Mr. G. H. Morris, of Brook Street, under whose dental care she had been for the past few weeks. She came over to England from the Cape in December, 1910, and in March of the present year she first noticed a swelling of the upper lip which has been gradually getting worse until the present time. A small swelling had also appeared behind the last right upper molar tooth, which was painful. The lip had been painted, upon the advice of a medical man, with iodine. Upon inspection, the left half of the upper lip was thickened with a slight eczematous condition of the over-lying integument, but there was no marked eversion. The left corner of the mouth drooped somewhat, giving one the impression of a facial paresis, but the motor power of the facial muscles upon that side was unimpaired. The

mucous surface of the swollen portion was superficially ulcerated, and there was a small fissure in the vicinity. The dentition was good, and there was no pyorrhœa. The swelling was said to be worse upon waking in the morning. Her general health was good, and there was no direct history of tuberculosis in her family.

DISCUSSION.

The PRESIDENT said some members would remember the case of a young woman from America whom he showed, with a thickened lower lip, the thickening having been persistent for a long time. He treated it with applications of radium, and the whole of the swelling went down. Within the last fortnight he had heard from her that her lip was perfectly well, and that there had been no recurrence of the trouble. That was a case which had been going on for several years, and there had been constantly recurring inflammatory attacks, and very considerable pain and sensitiveness. It was very difficult in these cases to prove whether they were streptococcic or not, but there was strong presumptive evidence that they were. He believed that the cases of solid œdema below the eyes were streptococcic infections following the so-called recurrent attacks of erysipelas. In answer to a question by Dr. Pringle, he said the radium was applied to the surface of the lip once a fortnight for several sittings, and there was no inflammatory reaction. It would be reasonable to try radium in this case, but he thought anti-streptococcic serum was likely to be of service.

Dr. SEQUEIRA said his own case was supposed not to have been due to struma or scrofula, but to repeated attacks of streptococcal infection. There were chronic fissures at the angle of the mouth, and the thickening was supposed to be due to repeated attacks of erysipelas. The streptococcus was not isolated.

Tuberculides in a Girl, aged 5.

By J. H. SEQUEIRA, M.D.

THE history given by the child's mother was that she was taken ill about Christmas, 1910, with sickness and diarrhœa, together with pains in the abdomen. The child was in bed for three months. There were no other particulars obtainable. While the patient was in bed with this illness a number of spots appeared upon the extremities. The mother positively stated that they all came out in one crop.

There was no history of tuberculosis in the family, and the patient has no palpable enlarged glands or the scars of glandular abscesses. The skin eruption had the following characters and distribution: On the right elbow there were two flat nodules which were scaly upon the surface. The nodules were of a purplish colour; one was about the size of a threepenny-piece, and the other rather larger than a pea. The small plaques were raised above the level of the surrounding skin for about 1 mm. to 1½ mm. The nodules were freely movable over the subjacent structures, and felt hard on palpation. Other nodules of exactly similar character were present upon the left elbow, on the knuckles of the first and second fingers of the left hand, over the interphalangeal joints of the second and third fingers of the left hand, on the knuckles of the first and second fingers of the right hand, and over the interphalangeal joints of the second and third fingers of that side.

Nodules were also present in the creases of skin over the tendo Achillis on each side, and on the outer aspect of the left foot there were some small nodules.

On the buttocks there were some reddened scaly patches, and a similar condition was present upon the knees.

The helix of each ear showed a linear nodule about ½ in. long and ⅛ in. wide. This nodule had the same character as the nodules elsewhere; it was red and covered with slight scaling.

None of the lesions gave pain.

The temperature was normal, and there were no visceral complications. A definite reaction was given to the von Pirquet and Moro tuberculin tests.

The exhibitor thought that the extraordinary symmetry of the lesions put the case into the group of tuberculides rather than into that of lupus vulgaris. He, however, recognized that the peculiar warty character of the individual lesions suggested tuberculosis verrucosa.

DISCUSSION.

Dr. ADAMSON thought that the possibility of the case being one of multiple lupus after measles should be considered. An objection to this view and a feature in favour of tuberculides was the symmetrical distribution on the extremities and ears. The appearance of the lesions, however, strongly suggested lupus of the verrucose type, and lupus verrucosus was a not uncommon type of lesion in cases of multiple lupus. The first case of multiple

lupus Dr. Adamson had seen, which was shown by him at a meeting of the Dermatological Society in 1895,[1] was of this form. Dr. Crocker ("Diseases of the Skin") had reported two cases of multiple lupus verrucosus.

Dr. SEQUEIRA, in reply, said there was an illness, consisting of diarrhœa and sickness, but nothing more. It was difficult to see why she should get tuberculosis verrucosa, which was due to inoculation. The whole eruption came out at once.

Note on the Ætiology of Leprosy.[2]

By WILLIAM TURNER, M.D. (Gibraltar).

NOTWITHSTANDING that repeated attempts have been made at the instance of the Colonial Office, the Royal College of Physicians, and other interested bodies, to solve the question of the ætiology of leprosy, by sending to various parts of the world Commissions equipped with this object in view, and in spite of the fact that these expert Commissioners have almost unanimously reported their conviction that this disease is not propagated by contagion except in very rare instances, the subject of the causation of leprosy remains still shrouded in a veil of mystery. Large sums are still being devoted, in certain of our Colonies, to the maintenance of leper establishments—an expenditure which ought to be largely curtailed were the findings of these investigators believed in and acted upon. A most important advance in the inquiry was undoubtedly made by the discovery of the essential microbe—the *Bacillus lepræ*—by Hansen, of Bergen, in 1874, but the complete life-history of this bacillus and its channels of entrance into the human system have yet to be determined. There does not, at present, exist any general consensus of medical opinion on this subject.

The case to be related is in some respects a unique one, and may help to limit the inquiry which aims at the elucidation of the ætiology of this dread disease.

The patient was an imbecile, who was admitted at the age of 28 to the Gibraltar Lunatic Asylum in the month of June, 1883.

Family history: His father, an Italian by birth, was a carpenter in the Gibraltar Port Department, and lived for many years in a stationary ship in the harbour. He was a very healthy man, and died of old age

[1] *Brit. Journ. Derm.*, 1895, vii, p. 111.

[2] Read at the meeting of the Section held on July 20, 1911.

at 94. His mother, a native of Gibraltar, died at the age of 87 from a like cause. Two sisters are alive, both of highly neurotic temperament, but quite healthy. Of two brothers, one is now in the lunatic asylum suffering from dementia with epileptic tendency; the other, a confirmed drunkard, is a carpenter by trade. A cousin is an inmate of the lunatic asylum, a chronic imbecile. There have been other evidences of insanity in the family, all traceable to the mother's side. None of these relatives have presented any signs of leprosy.

Patient's history: He is said to have been imbecile from his birth. He never had any education, and never followed any employment nor earned his own living, but passed his time in wine-houses as a buffoon for the diversion of customers, who supplied him with drink. At the age of 28 he was admitted to the lunatic asylum at the request of his family. From the time of his admission till the year 1902 (i.e., for nineteen years) nothing remarkable in the patient's condition took place. He showed no signs of physical defect or disease of any kind. He employed himself mainly in gardening and scrubbing floors, cleaning utensils, &c. In 1902 his hands and feet became affected with dusky red thickenings on their dorsal aspect, and pinkish swellings appeared also on the face, especially on the brow and eyebrows. It was not until May, 1903, that this affection of the skin suggested the idea of leprosy. (He had, indeed, been treated for some time with mercury on the suspicion of its being syphilitic.) From that time the leonine configuration of the face began to assert itself. The case was then isolated, and measures taken for keeping separate his clothing, feeding utensils, washing arrangements, &c. As years passed the patient remained in good general health. The hands gradually became more deformed and the face more disfigured. Sometimes the affected parts would appear more deeply congested and swollen than at other times, but the hard, callous folds on the forehead and eyebrows never diminished in size, and dark pigmentation of the skin supervened. In March, 1906, one of the toes became erysipelatous and eventually dropped off. The extensor aspect of the forearms, the shoulders, and the chest became studded with brown, tuberculated swellings, and ulcers formed on the elbows. Opacity of the corneæ set in, and the voice became aphonic from swellings in the mouth and pharynx. Finally, all forms of nourishment were rejected, and the patient died exhausted on September 21, 1906.

Thus the case from beginning to end was typical of the tuberculated form of leprosy. The presence of the *Bacillus lepræ* was abundantly demonstrated in specimens taken from the hypertrophied tissues of the forehead.

Now the question which presents itself is : How came this man to be affected with leprosy ?

No case of leprosy has occurred amongst the inhabitants of Gibraltar for a great many years, certainly not within the past thirty years to my own knowledge. There is no record that the disease has ever been endemic here. The patient in question, born and reared in Gibraltar, had been an inmate of the lunatic asylum for nineteen years before he became affected with leprosy. He had had no communication with the outer world during that time, except through the visits of his sisters for half an hour once a fortnight, and the sisters showed no signs of the disease. It should be stated also that Gibraltar may be regarded, to all intents and purposes, as an island. The town is entered from the mainland by only one narrow road, and the lunatic asylum occupies an isolated site half a mile beyond the town. Its gardens are fenced in by high retaining walls, outside of which the patients do not pass. The idea, therefore, of a *contagious* origin in this case seems absolutely untenable.

The question likewise of an *hereditary* origin appears to be answerable only in the negative, seeing that neither parents nor any known relative ever manifested any evidence of the disease.

Insanitary conditions of life, *uncleanliness* of person or of abode, or of clothing, may similarly be eliminated as impossible causes in this case, for the accommodation in the asylum is ample, well ventilated, almost luxurious, and the washing and bathing arrangements receive the utmost attention.

Emanations from the soil have been suggested by some authorities as possible carriers of the virus of leprosy, but in this case there is no history that the disease has ever been endemic here so as to contaminate the soil, at all events not for centuries past, and one can hardly conceive of the bacillus remaining effective in the soil after so long a period, even if it had existed centuries ago.

The problem of causation, therefore, so far as affecting the present case, appears to resolve itself into a question of *food supply*, all other considerations having failed to help us in our inquiry. The dietary provided in the asylum is a liberal one and includes a large proportion of fresh vegetables and also fresh meat. Up to the year 1900 a ration of salt pork was issued once a week, but this was then struck off as it was suspected of having been the cause of an outbreak of dysentery. Up to the year 1903 two rations of fish were issued weekly, one of fresh fish, the other of dried salt codfish. The latter was then omitted from the dietary on the appearance of the case of leprosy.

Whether or not the salt pork or the dried salt fish can be held in any way answerable for the case in question, or are to be regarded in the light of simple concomitants, it was hard to say, but the coincidence is interesting in view of the theory held by Sir J. Hutchinson that dried and decomposing fish is to be regarded as the true cause of leprosy.

Announcement.

The PRESIDENT announced that committees had been formed for the purpose of considering:—

(1) The nomenclature of dermatology.
(2) The alleged dangerous results following the administration of arsenic.

Communications upon these subjects should be addressed to the Hon. Secretaries.

Dermatological Section.

November 16, 1911.

Sir MALCOLM MORRIS, K.C.V.O., President of the Section, in the Chair.

Case of Atrophic Tuberculide.

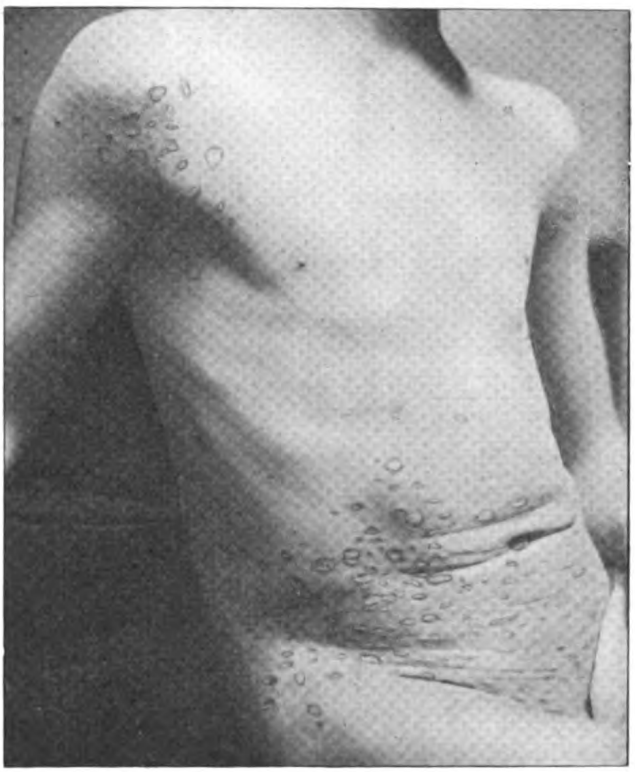
By J. L. BUNCH, M.D.

THE patient was a boy, aged 12, who had been under his care for the past five years at the Queen's Hospital for Children, and before that under Dr. Adamson's care at the same hospital, who had shown him at the Dermatological Society of London on May 9, 1906. The eruption had begun when the child was aged 4 as a single red patch at the navel, on which small red nodules had developed later. The nodules were slightly raised, somewhat papular in character, and distinctly infiltrated. They had a tendency to necrose, and always left a superficial, shallow scar about $\frac{1}{8}$ in. to $\frac{1}{3}$ in. in diameter.

In 1906 there were about thirty of such scars round the umbilicus, and scattered among these were about a dozen millet-seed- to split-pea-sized, raised, red papules. During the succeeding years similar necrotic papules had made their appearance in the inguinal region, on the thighs, on the upper part of the buttocks, in front of and behind both axillæ, and on the shoulders and back. Such papules were succeeded by superficial, depressed scars, similar to those previously described, so that there are at the present time a very large number of such scars on the areas already mentioned—so many that they have not been counted. The disease is evidently still progressive, for when shown at the meeting there was a well-marked, red, raised nodule on the abdomen which had made its appearance during the past ten days such as always went on to the formation of a depressed, atrophic scar.

Attention was called to the fact that the nodules and scars were always preceded by a circumscribed, irregular, dry, scaly, red dermatitis,

such as had been described in 1906 on the inner side of the thigh and arm, where there were now the characteristic scars. Similar appearances had preceded the atrophic tuberculide elsewhere, and there was now a very well marked patch of such dermatitis on the right shoulder, which probably denoted the appearance of the nodular eruption within the next year or two.



Papulo-necrotic tuberculide.

The boy attended school regularly, and was not inferior in physique to other boys of his own age; there were no signs or symptoms of lung trouble, but there were some enlarged glands in the neck and groin.

He had not reacted to injection of 1 mgrm. old tuberculin, nor on two occasions to von Pirquet's reaction. On one occasion a member of the hospital staff was inoculated at the same time with the same tuberculin solution and gave a positive reaction, while the patient did not. Sections showed some giant cells, and a well-marked infiltration of small round cells, but no tubercle bacilli.

DISCUSSION.

Dr. SABOURAUD expressed the opinion that it was a papulo-necrotic tuberculide, and suggested that a guinea-pig should be inoculated as a test.

The PRESIDENT (Sir Malcolm Morris, K.C.V.O.) said he did not think it was acne scrofulosorum; it was not that type of lesion, nor had it the distribution.

Dr. GRAHAM LITTLE considered the case came well within the group of tuberculides of the type of acne scrofulosorum; the distribution, though unusual, was not rare; he could recall two of his cases reported in the *British Journal of Dermatology* (vol. xiv, p. 92; p. 265), in which similar lesions were noted, with a distribution on the chest, abdomen and buttocks as well as the lower part of the body, and a third case in which the buttocks were particularly involved as well as the legs (*British Journal of Dermatology*, vol. xiii, p. 185). As regards the nomenclature, though "acne" might not be and was not justified by the pathology of these cases, the term "acne scrofulosa" was consecrated by long usage and acceptance (vide remarks by Adamson, *British Journal of Dermatology*, vol. xviii, p. 358), and deserved to be retained.

Dr. PERNET said he did not think it could be anything but a tuberculide, despite the fact that the von Pirquet reaction was negative. The patient had enlarged glands and the general aspect of a tuberculous individual.

Case of Multiple Angiomata.

By J. L. BUNCH, M.D.

THE child was aged 2 months, and had come under his care when three weeks old, the numerous skin lesions having been present since birth. These consisted of more than a hundred small, purple, raised tumours, from a pea to a small nut in size, on the trunk, limbs, face and scalp. They became pale when pressure was made on them with a diascope, and were in most cases soft to the touch, but one or two on the legs seemed slightly firmer in consistence. None of them showed a yellowish tint such as is seen in xanthomata. Some of the lesions on the face had already been treated by the exhibitor with solid carbon dioxide and been cured, and it was proposed to treat in the same way those which caused disfigurement.

DISCUSSION.

The PRESIDENT considered that some of the lesions felt like deep-seated fibromata.

Mr. MACDONAGH said he thought it might belong to the group of multiple benign endotheliomata of the congenital xanthoma type, because many if not all the lesions of congenital xanthoma started like this case—clinically angiomata; then the angiomatous condition disappeared, giving place to a yellowish lesion resembling xanthoma, which on section was found to be endothelioma. He had seen lesions as vascular as in this case pass into xanthoma. He had shown a case at the Society, which at first had lumps like this case, but larger, all over the body. Then they got smaller, became yellow just as in the case he showed last year, and disappeared spontaneously.

Urticaria Pigmentosa in a Baby, aged 10 Months.

By J. L. BUNCH, M.D.

PIGMENTED spots had begun to show themselves on the skin a month or two after birth following on small wheal-like lesions, which gradually flattened down and became brownish in tint. There were now a large number of brownish or blackish irregular lesions from a millet-seed to a bean in size on the chest, abdomen, back, thighs, neck, face and scalp. They seemed to be unusually numerous and well-marked on the scalp, as the baby's hair was thin and did not hide many of them. The spots were, when recent, somewhat irritable, and there were, when shown, some small but definite urticarial wheals, and any of the skin lesions could be made prominent by rubbing.

The child was a male, as are the majority of these cases.

A section was shown, having the usual structure and characteristics.

Case of Lupus Erythematosus localized on the Scalp.

By T. COLCOTT FOX, M.B.

THE patient was a young woman who had the condition in the front of the scalp for an indefinite time; she only noticed it accidentally a few years ago. There were seven atrophic patches, not very rounded, with irregular denuded little bags. She had nothing on the face or

elsewhere. On looking carefully at the patch over the left temple it would be found to have a distinct red infiltration on one side. Dr. Fox had not been able to trace any perifolliculitis. The tendency of lupus erythematosus in contrast to lupus vulgaris to affect the scalp was well known. Cases had been seen by most in which it was confined to the scalp. Nearly all the atrophic wasted patches were in the front of the head, and one could not trace except in one what they had arisen from. The case illustrated the difficulty which was sometimes encountered in diagnosing some of these atrophic affections of the scalp. There was no family or personal history of tuberculosis obtainable, and the patient first discovered the patch quite by accident, by the touch of her finger. There was a patch on the parting, which was a linear extension of an atrophic patch.

Case of Vitiligo.

By E. G. GRAHAM LITTLE, M.D.

THE patient was a married lady, aged 45, who had begun to have disorders of pigmentation eight or nine years ago, apparently about fourteen days after a severe attack of diarrhœa accompanied by illness and fever. She had been under the observation of Dr. Davidson, of Uxbridge, to whom the exhibitor owed his best thanks for a complete record of the case. These attacks of abdominal pain, with fever and diarrhœa, have been frequently repeated during Dr. Davidson's observations in the last seven years, and have been regarded as of the nature of colitis. She has, further, tingling sensation down the arms, swelling and discoloration of the hands, which become of a bluish-red colour when they are warm. The pigmentation disorder consists both of lessening of pigment and hyper-pigmentation, and chiefly affects the hands, forearm and forehead, and in a slighter degree the dorsum of the feet.

The patient has had two children, of whom one died early in childhood, and one is still living, aged 14½, and in good health. She has had no miscarriages. Latterly, apparently coincident with a fresh extension of vitiligo, she has had an exacerbation of colitis, and the tingling sensations in her arms and hands, and has become very high-strung and nervous. She suffered for several years from uterine hæmorrhages ascribed to fibroids, and had the uterus removed without its appendages at the beginning of this year.

The exhibitor thought this history of the complex of symptoms—colitis, acroparæsthesiæ and disorders of pigmentation—too definite to be disregarded, and was unfamiliar with this association, for which reason he invited the expression of the experience of others.

With regard to nomenclature, although leucodermia seemed to be loosely used as synonymous with vitiligo, and was actually given the preference in Radcliffe-Crocker's "Text-book," and in the article in the recent edition of Allbutt and Rolleston's "Medicine," it seemed undesirable to regard these terms as synonymous, especially in view of Besnier's express differentiation, and retention of the term "leucodermia" for a simple achromia (albinism), vitiligo for the mixed hyper- and hypo-pigmentation now under discussion.[1] The disease was not a common one in Europe and the United States, at least Crocker's hospital statistics give 1·5 per 1,000, which exactly correspond with the American Dermatological Association's statistics of 123,746 cases, in which vitiligo formed 1·55 per 1,000.

DISCUSSION.

Dr. WHITFIELD remarked that Mr. Cheatle had pointed out that these conditions originated in Head's "maximal areas." No definite nerve lesion had been found beyond this. The numbness and tingling present in this were described by Cassirer. He called it "acroparæsthesia,"; it was a well-known symptom-complex, and Cassirer said it was always due to intestinal toxæmia, which view he (Dr. Whitfield) shared. It was enormously increased if there was an exciting cause, such as was the case in laundry women, who needed to keep their hands in water very much. If one inquired carefully among female out-patient's of middle age who suffered from marked dyspepsia, it would be found that they frequently had a mild degree of acroparæsthesia, of course without colour changes. Apparently it had nothing to do with Raynaud's disease. The only cases he had seen bearing upon the present one had not leucodermia. He had records of three cases of it in which what he might broadly term indigestion was present, with extraordinarily thin nails. The nails had not always been thin, but their thinness was acquired. It was known that leucodermia occurred also in association with atrophy of nails, and this seemed to be another link in the chain. He suggested investigating this patient with a view to using *coli* vaccine. Very likely she did not drink enough water. Many women took far too little fluid in the day.

Dr. PRINGLE said he thought it must be the experience of all dermatologists that leucodermia was frequently associated with acroparæsthesic symptoms,

[1] Stelwagon's "Essentials of Diseases of the Skin" (appendix). Note to Kaposi's "Diseases of the Skin,' ii, p. 155.

and like them, often occurred in persons of this patient's sex and age. The association of colitis did not seem to him to be a very frequent one, and he questioned the causal nature of the relationship. He had at present at the hospital under observation the most quickly spreading leucodermia he had ever seen: the man—a robust navvy—was rapidly becoming white, but nothing wrong with him in any other way could be found although the blood and every function had been scrupulously examined. The "toxic" theory of the origin of all pigmentary disorders of the skin demanded further careful investigation.

The PRESIDENT said that leucodermia was relatively so common without being associated with any other symptoms that it was difficult to associate it with other nervous phenomena, except as an accident. He saw a large number of such cases, and found them in perfect general health. One of the strongest and most robust members of the medical profession had leucodermia for many years, but enjoyed quite good health with it. He asked whether any member could make suggestions as to the treatment of leucodermia, apart from the nervous symptoms.

Dr. F. PARKES WEBER remarked that acroparæsthesia was so extremely common that if leucodermia (that is to say, vitiligo) had been frequently associated with it, such an association would be generally recognized as an ordinary one. But the fact was that in nearly all the cases of acroparæsthesia leucodermia was not present, although the latter was not a very rare affection by itself. Moreover, colitis had been very much discussed in the last years, from both the medical and surgical sides, and in scarcely any of the cases was vitiligo described as associated with it. He therefore thought one might almost say that the only connexion in Dr. Little's case between the vitiligo on the one hand and the acroparæsthesia on the other was that vitiligo and acroparæsthesia both had something to do with the nervous system, and that all kinds of intestinal toxæmia (including that from colitis) tended to increase all kinds of disorders more or less connected with the nervous system (including both acroparæsthesia and vitiligo).

Granuloma Annulare in a Little Girl, aged 4.

By J. M. H. MACLEOD, M.D.

THE lesions were situated on the back of both thighs and the calves of the legs and were nine in number. They were similar in type to those of a number of other cases which have been brought before the Society, and were practically identical with one of the original cases shown by Dr. Colcott Fox at the Dermatological Society of London in 1896 under the heading of a "ringed nodular eruption" in an infant, aged 2. The lesions were perfectly typical, and consisted

of small whitish nodules like boiled sago grains and rings with a pearly border varying in size from a sixpence to a shilling. The affection was first noticed by the mother, in the hot weather towards the end of August, when two or three nodules appeared on the buttocks. From the appearance of the lesions and the observation of the mother, the exhibitor had come to the conclusion that in this case the rings were formed by essential spreading and not by the aggregation of a number of nodules in a circinate manner. New lesions had kept on appearing since that time, but up to the present none had disappeared. The exhibitor regarded the condition as an entity and quite independent of the annular type of lichen planus with which it had sometimes been confused.

DISCUSSION.

Dr. COLCOTT FOX said the case was very similar to a case in a child which he exhibited before the old Society [1] many years ago, with the indefinite name "ringed eruption of the buttock." He never could decide what name to give to the condition. It was not an ordinary erythema, and it was indolent.

Dr. HALDIN DAVIS said he had shown a similar case in which the eruption followed the same distribution. The lesions were of exactly the same shape, only in his case they were harder and tougher. He showed the case as one of granular annuloma, and members had agreed with the diagnosis. The lesions in that case disappeared in six months; he did not know whether they did so spontaneously, or as the result of treatment. He gave salicin internally; no external application was used.

Dr. BUNCH, referring to the case of the kind which he showed at the last meeting, said that as Dr. MacLeod had just mentioned it he would like to add that since the last meeting another case of the kind had come under his care which illustrated the transition between these conditions. In this last case the hands, feet and legs were involved, and there were in all eight or ten lesions.

Dr. GRAHAM LITTLE deprecated the continuance of the term "lichen annularis," which was inevitably confused with lichen planus annularis He regarded this case as definitely one of granuloma annulare; the lesions were unusually numerous, but this feature was paralleled by a case reported by Dr. Grover Wende,[2] of Buffalo, a photograph and section of the skin of which had been kindly submitted by Dr. Wende to him. In the present case the circles were made up of separate nodules, as could be plainly seen in the more salient lesions on the lower part of the leg. The position was one of the commonest for the development of granuloma annulare.

[1] Dermatological Society of London, December 1895, *Brit. Journ. Derm.*, 1896, viii, p. 15.
[2] *Journ. Cutan. Dis.*, New York, 1909, xxvii, pp. 388-393.

Case for Diagnosis.

By J. M. H. MacLeod, M.D.

The exhibitor considered this was probably an example of "Parapsoriasis *en plaques*" of Brocq. The patient was a parlour-maid, aged 28. She enjoyed good general health and had had no serious illnesses, and there was no history of tuberculosis, psoriasis or other cutaneous disease in the family. The eruption for which she was exhibited to the Society appeared two months ago without any definite warning, and no cause for it could be assigned by the patient; previous to this she had never suffered from any form of skin disease. The eruption was situated chiefly on the outer aspects of the thighs and buttocks, the left thigh being specially affected, a few lesions also being present below the knees, on the left shoulder and upper arm. The eruption consisted of macules and patches varying in size from a pin's head to 1½ in. in diameter. The initial lesion consisted of a small macule or slightly raised papule about the size of a pin's head; this was roundish in outline, and appeared to the exhibitor to be independent of the follicles as a rule, though a few of them were follicular. The maculo-papule was pinkish in tint at first, but tended to become pigmented later and to assume a brownish-yellow colour like that which is present in association with the cayenne pepper-like patches which occur in the legs in connexion with venous stasis. The pigmentation was not diffuse but in the form of specks. These lesions appeared in small clusters which coalesced to form patches about the size of the little finger, which were irregular in outline, brownish in colour from the pigmented specks, and presented a slightly scaly surface.

By the development of new papules at the periphery these lesions gradually increased in size up to 1½ in. in diameter. In the larger lesions a partial involution had taken place in the centre, so that a circinate patch resulted with a deeper brown, slightly raised border, in which, here and there, the individual papules could be detected. The lesions had a somewhat delicate appearance, but were extraordinarily resistant to treatment. An ointment containing 2 per cent. of sulphur precipitate and salicylic acid had been rubbed into them regularly for six weeks without any appreciable difference except for

the reduction of the scales. The superficial venules on the patient's thighs were dilated and the follicles abnormally prominent.

The case was brought before the Section with the object of discussing the diagnosis. The appearance of the lesions, their situation, the absence of greasy scales and their singular resistance to treatment with sulphur put any form of seborrhœic dermatitis out of court. The group under which it seemed to come most naturally was that of the resistant maculo-papular scaly erythrodermias to which Brocq has given the name of parapsoriasis, and more especially to that type which he has called "Erythrodermie pityriasique en plaques disseminees." In certain of its features it resembled the condition described by the late Dr. Radcliffe-Crocker as "Xantho-erythrodermia perstans," which was probably allied to the affection described by Brocq.

DISCUSSION.

Dr. PRINGLE said that at first sight he could not help noting the superficial resemblance of the condition to guttate psoriasis. Therefore he had just asked Dr. Sabouraud if it would be classified in Paris as a parapsoriasis *en gouttes*—a condition he had never been able clearly to identify—but the reply was in the negative.

Dr WHITFIELD said there was a well-defined line, beyond which some of the follicles had little horns in them. Occasionally they took the form of a pustule. He believed the patches were part of a general horny dystrophy, and that the folliculitis was related to the patches.

Paget's Disease of the Umbilicus cured by the Application of Radium.

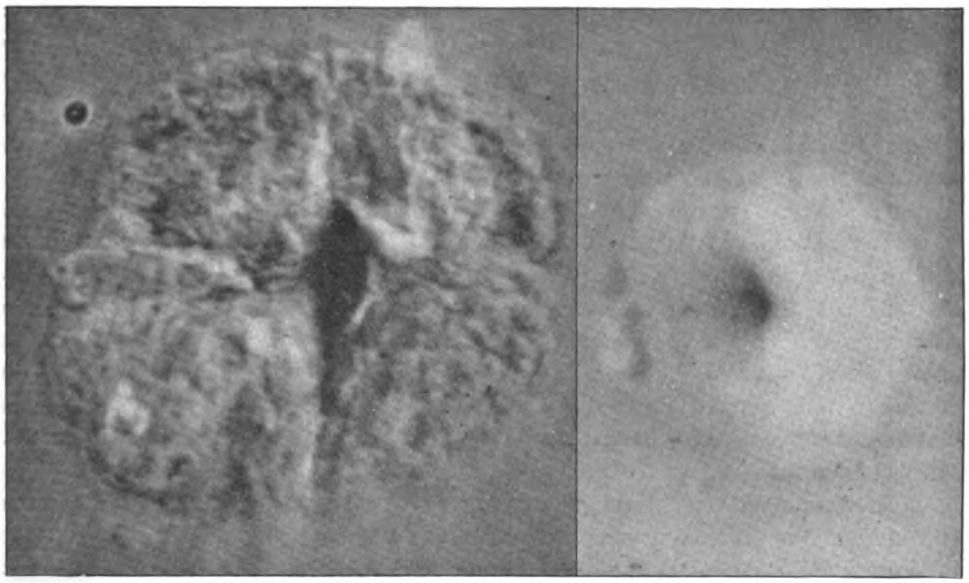
By W. A. MILLIGAN, M.D.

MISS W., aged 31, came complaining of a smelly discharge from the navel, accompanied by an eruption round the navel. The trouble began four years previously, with a smarting pain round the waist and a redness towards the right side of the umbilicus.

Ordinary remedies were tried, but with no success, the condition steadily getting worse. The patient was then subjected to X-ray treatment—four applications of ten minutes each. This apparently cured it, but very shortly afterwards it broke out again. For twelve months or so ordinary remedies were resorted to, but with no result.

Again X-ray treatment was tried—six applications—but this time it got worse instead of better.

Sir Malcolm Morris saw the patient in consultation about the middle of June last, and he advised either total excision or radium treatment. Accordingly small doses of radium were applied round the edge of the eruption, which now had a radius of about 2 in. from the umbilicus. The radium was applied in successive places round the edge and each place had an exposure of four hours. This certainly had a good effect, although it did not cure it. Finally, on August 21, 1911, at the



a *b*

Paget's disease of the umbilicus; *a*, photograph taken August, 1911; *b*, November, 1911, after treatment by radium.

Radium Institute, the patient had a treatment of 70 mg. of pure radium for one and a half hours direct on the skin, there being no intervening screen. For ten days nothing was felt by the patient, and then she had a burning sensation round the waist and the discharge got worse. This lasted for two weeks and then the skin healed, leaving only a small sore spot on the right side.

The condition prior to the last application of radium is well shown in the photograph and consisted of a raised indurated edge all round with a raw weeping surface extending into the umbilicus.

The condition is now apparently cured, although there is still some discharge, and the question arises as to whether there may or may not be a patent urachus. This has not been conclusively proved, although at times the discharge has an ammoniacal smell. It is interesting to note the large dose of radium used by Mr. Pinch at the Radium Institute—a dose corresponding to two million activities.

DISCUSSION.

The PRESIDENT remarked that when he saw the patient in June the lesion was so marked in its circumference and it was so defined that at first he felt very doubtful of its exact nature, whether it was true Paget's disease or whether it was a very superficial epithelioma or a rodent. He concluded it was Paget's disease. Another interesting fact was that there had been a discharge, and it looked as if she had a patent urachus. When Mr. Pinch applied the radium he squeezed the area, and expressed the opinion that the discharge had a distinct odour of urine. The discharge was still present. At first it seemed as if the action of the radium had caused a blockage, and had healed up the condition for the time. But it was questionable whether it was healing, as it was still discharging. The radium given was in a large dose, and there was only one screen of india-rubber. She had a violent reaction, which was at its height at the end of a fortnight. At the end of three weeks the condition had quieted down. A case of Paget's disease of the umbilicus was shown years ago before the old Society by Mr. Marmaduke Sheild. He (the President) had treated two cases of Paget's disease by radium, but in neither of them could he get a biopsy. The cases were typical as far as the clinical appearance was concerned, and had lasted some years. One was of the nipple, with a well-defined border, lasting many years, in a lady who absolutely refused to have the breast amputated, and therefore one had to be content to do one's best short of that. She was treated with radium in a similar manner to this case, and the result had been satisfactory up to the present time. It was done two years ago, and there had been no return. The other case was also well marked, but was not connected with the nipple; its situation was on the side of the breast, not far from its free border, and that case did extraordinarily well with radium. There was one application of radium for two hours, the amount being 25 mgrm. At the end of the reaction every trace had apparently gone. But at the end of six months there was a small recurrence. Radium was again applied, and up to the present time there has been no further trace of the disease. There seemed to be no risk of deep-seated changes there.

Dr. PRINGLE was impressed by the complete absence of scarring, and asked if the diagnosis had been confirmed by biopsy or microscopical examination.

Dr. COLCOTT FOX said he had described in the *British Journal of Dermatology*[1] with Dr. J. M. H. MacLeod, a man who had the disease all round the umbilical region. It had existed for several years, and microscopically it was proved to be genuine Paget's disease. The area was not very large, and it was removed by operation.

Dr. SEQUEIRA said he had treated three cases of Paget's disease by means of X-rays, and in each case healing resulted. Two were instances of Paget's disease of the nipple and the third was on the glans penis, a case which he showed at the Section. In each of the breast cases a duct carcinoma developed later. In the case in which the glans penis was affected there was a nodule of carcinoma in the bulbous urethra. In the present case there was no glandular stricture lying immediately beneath the lesion, so perhaps the outlook would be better.

Dr. WHITFIELD said it was not necessary to make a biopsy in order to diagnose Paget's disease. One could use Darier's method of taking a scale with some discharge, and examining by staining or in potash. He had only seen a limited number of cases, but in his hands the method had invariably shown the characteristic pseudo-psorosperm bodies.

Dr. MACLEOD said that he had had the opportunity of studying both clinically and histologically the case of Paget's disease of the umbilicus referred to by Dr. Colcott Fox. Paget's disease affecting regions other than the breast was exceedingly rare. Cases had been reported on the penis and scrotum, at the anus, over the pubes, and on the vulva, but so far as he knew, the case described by Dr. Fox and himself was the first recorded on the umbilicus.

Extensive Ringworm of the Trunk and Extremities, with Granulomatous Formations.

By J. H. SEQUEIRA, M.D.

THE patient, a young man, had been brought before the Dermatological Society of London in 1906, and the case was so remarkable that it was published in full in the journal (*British Journal of Dermatology*, August, 1906) with photographs of the patient and of the cultures which had been made by Dr. Colcott Fox. After nearly two years' treatment the patient was apparently cured, and he was again shown to the Society. There had recently been a fresh outbreak, arising, in the exhibitor's opinion, from the fact that the disease had persisted in the

[1] C Fox and J. M. H. MacLeod, *Brit. Journ. Derm.*, 1904, xvi, pp. 41-62.

nails, and he now took the opportunity of bringing the patient before the Society in order that he might have the opinion of Dr. Sabouraud on the condition. As Dr. Sabouraud has kindly undertaken to make cultures and to report upon their nature, Dr. Sequeira proposed to publish a fuller account of the case later. It will therefore suffice to say that the patient had suffered from tinea since 1898, and that when he was first shown there was, in addition to a widespread scaly eruption, a large ulcer at the umbilicus and numerous inflammatory swellings in different parts. These were treated very vigorously by chrysarobin, iodine, and finally by injection with carbolic acid and by prolonged antiseptic baths. The nails had all been removed, and in spite of this the infection still persisted, and in the present attack, now of several months' duration, the scaly ringworm involved large tracts, particularly in the flexures and on the upper and lower limbs, and in the right axilla there was now a granulomatous infiltration tending to break down into an ulcer with overhanging edges exactly similar to that present in 1906. In the left groin there were also numerous small, button-like, purplish-red swellings. The boy's sister had also suffered from an extensive scaly ringworm. There was no evidence of infection from a tropical source, but this could not be excluded, as the family had lived at Limehouse near the water-side, and the sister probably might have been infected as she was at one time engaged in rag-picking.

DISCUSSION.

Dr. PRINGLE suggested that iodine cataphoresis might be tried for the nails. He had used it in two such cases, and remarkable improvement ensued, one of the cases having been certainly cured.

Dr. PERNET said that in 1905 [1] he saw a patient from the East, who gave a history of two years' duration. The trouble had started about the gluteal cleft, and had spread rather widely over the right buttock, and more recently on to the left. The lesions were well defined, infiltrated, especially in the neighbourhood of the cleft, where there was ulceration with a granulomatous look about it, and something like a broken-down tertiary syphilitic condition. Dr. Pernet took some scrapings and found a trichophyton—long, slender branching mycelia. He inoculated a tube, but unfortunately the result was not noted in his case-book, but was no doubt included in the notes on a number of cases he had handed to the late Dr. Radcliffe-Crocker. Dr. Pernet ordered a chrysarobin ointment

[1] Case Book E, fol. 182.

(gr. x. ad. ʒi). Ten days later the parts looked practically all right, but treatment was not dropped. On telling the patient he was suffering from ringworm, he then stated he had had Dhobie itch of the crutch. For the buttock trouble he had consulted several men in the East, who apparently had not diagnosed the condition as ringworm. Their remedies, he said, had done him no good at all. As to the nails, he had about a year ago evulsed all the nails of a patient (also from the East), but apparently without curing the ringworm condition. In that case there was no ringworm of the body.

Dr. WHITFIELD said he regarded the condition of the nails as such that their treatment must be regarded as a necessary forerunner of the treatment of the general condition. But he had one patient under treatment whose nails he had evulsed four times, and it would have to be done again. The case had been treated with chrysarobin and everything else he could think of. It had been kept for three months under Dr. Sabouraud's preparation under a rubber stall, and had now been treated by Norman Walker's recent method with copper sulphate. But none of these methods had cured the condition. In some cases it caused enormous hyperkeratosis of the nail-bed, so that it seemed almost impossible to get it away. One seemed to leave something, especially at the lateral folds, deep down.

Dr. GRAHAM LITTLE had had a case of ringworm of the nails of all the digits,[1] both fingers and toes, in a man in whom the disease was persistent probably for fourteen years, yet no extension of the disease to the body had occurred in that time. The patient was a man, aged 45 when seen, and fungus was demonstrated from scrapings from all the nails. He had tried ionization among other treatments, but had had difficulty in getting electrical contact on the surface of the nail and had had no success with this method.

[1] *Brit. Journ. Derm.*, 1907, xix., p. 204.

Eczematoid Ringworm of the Extremities and Groins.

Addresses Introductory to a Discussion on the Subject.

(I) By ARTHUR WHITFIELD, M.D.

IT is well said that no one man makes a discovery, and although for some time I was under the impression that I was the first in the particular field of investigation with which we are dealing to-night, I have found out that this is not the case, but that I was preceded by some sixteen years.

Attention was of course first called to eczematoid ringworms generally by Hebra in the year 1860, but according to Sir Malcolm Morris in his able article on ringworm in the recent edition of Clifford Allbutt and Rolleston's " System of Medicine " the nature of the disease was already known to Devergie.

At all events, the disease which was called by Hebra eczema marginatum was very well known to all the workers in skin diseases for many years without any particular attention having been paid to its occurrence in other parts than the groin.

However, in 1892 Djélaleddin Moukhtar,[1] while working in Paris in Fournier's clinic, drew attention on three separate occasions in the Société de Dermatologie to the presence of a fungus in disease of the feet. The clinical description given by him seems to me to have been extremely accurate, but he appears to have scarcely realized the relationship of the fungus to the disease in all the cases, as in his second communication he drew attention to an eczema of the part and presumed that the fungus had invaded a case of chronic eczema, whereas in the light of my own experience I should say that it was almost certain that the fungus was the actual cause of the eczematoid eruption.

For some years it has been my habit to examine microscopically scales from desquamative diseases and the fluid contents from vesicles. This method of investigation appears to me useful, as it gives one a good idea of the amount of infection present in those diseases in which one finds only the ordinary pyogenic cocci, and has in more than one case led to somewhat more interesting results.

Thus in July, 1908, I found in an intertriginous dermatitis of the

[1] *Ann. de derm. et syph.*, Paris, 1892, 3me sér., iii, pp. 152, 651, 855.

groin masses of oval bodies which on cultivation proved to be a yeast. As the organism was found in enormous abundance and grew in pure culture from every scale which was planted it was probably of causal significance. In February, 1909, I discovered in a marginate dermatitis of the glans penis in an elderly man a very large and abundant mould, which, however, I failed to obtain in culture. Both these cases were reported and their specimens shown at the meetings of this Section. These cases, however, are perhaps hardly within the scope of the discussion to-night, and I only mention them to add emphasis to my opinion that systematic examination of material from surface eruptions may furnish us with valuable information.

I now come to my first share in the elucidation of the eczematoid ringworm of the hands and feet. In July, 1908, I published in the *Lancet* a short series of cases of ringworm on the hands and feet in which the clinical appearances did not correspond with the well-known forms of tinea circinata. In none of these cases was I successful in obtaining a pure culture, and perhaps I may confess that I was more interested in establishing the presence of the fungus of ringworm and curing the patient than I was in determining the variety of the parasite, as at that time it seemed to me to be of rather academic interest. Here, as later events showed, I was wrong.

In 1910 Sabouraud published an exhaustive article on the subject in the *Annales de Dermatologie et de Syphiligraphie* in which he had collected seven cases. With his usual thoroughness he had obtained the organism in pure culture and had established the fact that in his cases the organism present was that which was the common cause of eczema marginatum and so called "Dhobie itch," and which he had named the *Epidermophyton inguinale*. In this article he referred to the work of Djélaleddin Moukhtar, but remarked that having seen his cases he was quite sure that they were entirely different from those that he was then describing. I notice also in the *Pratique Dermatologique* he refers to the same investigator, and in fairly close connexion states that the trichophytic invasions of the thick horny layer are generally due to the *Endothrix acuminatum*. In none of Sabouraud's cases do I find any record of the invasion of the nail-substance, but in one of my early cases I noted it, and in the nail from a case of Sir Malcolm Morris's which I investigated I found the fungus. This last case is important as in it I was able to complete the investigation by obtaining the fungus in pure culture. This culture I first believed to be an endothrix, but I have convinced myself on further investigation that it was an instance of the

Epidermophyton inguinale, a conclusion at which Dr. Adamson arrived when he first saw the cultures.

Before leaving the history of this affection I may draw attention to the paper of Aldo Castellani in the *British Journal of Dermatology*, May, 1910, in which he separates the Epidermophyta at present known into three varieties: (1) *Epidermophyton cruris* (Castellani, 1905); synonyms—*Epidermophyton inguinale* (Sabouraud, 1907), *Trichophyton cruris* (Castellani, 1905), *Trichophyton Castellani* (Brook, 1908). (2) *Epidermophyton Perneti* (Castellani, 1907). (3) *Epidermophyton rubrum* (Castellani, 1910). It is somewhat curious to note that as late as 1910 Castellani, though living in the home, as we may say, of epidermophyton infection, should not have identified the eczematoid invasion of the feet and hands. I know that this form of the disease must be plentiful in India and Ceylon, as many of my patients developed the disease there.

I now turn to the clinical portion of my paper. First as to incidence. It is remarkable how seldom one comes across inguinal ringworm in the out-patient classes, and I can scarcely call to mind an instance. It is therefore not surprising that I have not a single case of the severe chronic and relapsing type of eczematoid ringworm of the hands and feet derived from my hospital clinics. I have, it is true, come across stray infections of one hand in the out-patient department, but these are certainly not of the same nature. If I may hazard a guess I should say it is largely because in the out-patient class the washing is done at home, and I may say that I believe the expression "dhobie"—i.e., washerman's itch—is often ætiologically correct.

Secondly as to clinical types. I divide the disease into three main types, namely, the acute vesiculo-bullous, the chronic intertriginous of the toes, and the chronic hyperkeratotic of the palms and soles. Of the acute vesiculo-bullous type it may be said that it almost defies the attempt at clinical diagnosis without the aid of the microscope. The disease bursts out suddenly in twenty-four to forty-eight hours, and has all the characteristics of acute vesicular eczema or dysidrosis. Pus formation is generally absent, but the exudation into the vesicles and bullæ is so violent that it is commonly pinkish from the presence of a little blood mixed with the serum. It is *not* usual to find any suggestive grouping of the vesicles into disks or rings, nor is it the rule to find a particularly defined spreading edge such as might lead one to suspect the centrifugal growth of a parasite. In my early cases the finding of the parasite was merely the result of a routine examination of scales and fluid.

Some of these cases are the result of the infection of the epidermis by the epidermophyton, some are apparently infections by an ectothrix, and some may be infections by an organism not ringworm at all. In this connexion I may instance a case of acute bullous dermatitis material which was kindly sent me by Sir Malcolm Morris. On a rapid examination I reported that the scales were heavily infected with some mould fungus, but that I was not at all certain that it was a trichophyton. Culture gave an intensely rapidly growing white mould with which I was unfamiliar, but which was certainly not one of the common contaminating moulds which so often foil one's attempts at culture (in my experience these are always Penicillium). I therefore sent the culture to Dr. Massee at Kew, explaining how I had come across it, asking whether he thought it would be more likely to be an unusual contamination, or whether he thought it probable that it was of truly pathogenic nature. I received the reply from the Royal Botanic Gardens at Kew that it was a secondary conidial form of a species of Helminthosporium, one of the black moulds, and that some species of Helminthosporium are parasitic on plants. I still remain uncertain, therefore, of the relationship of the mould to the disease, though, as the original examination of the scales revealed large amounts of the fungus, I am inclined to believe that it was pathogenic in this case.

The second type of dermatosis produced by fungi is the intertriginous type. This is, in my experience, always secondary to a more or less acute attack, and is extremely chronic and difficult to cure. The eruption takes the form of a white and sodden mass of epithelium between the toes, with a more or less definite margin and slight vesiculation at the dorsal edge of the interphalangeal skin. On the plantar surface the eruption spreads downwards beyond the roots of the toes to terminate in a well-defined but somewhat irregular line about opposite the heads of the metatarsals. At this free edge the horny layer is in a state of constant desquamation, and the free edge of the scale is, as is always found in ringworm, very definitely turned towards the centre of the eruption. The hands may be similarly affected, but in this case the eruption spreads much further up on to the palms. The salient feature is intense itching and occasional soreness from the patient rubbing himself. In two cases now I have found the toe-nails affected.

The third or hyperkeratotic type is alluded to by Moukhtar, and was at the meeting of the Society at which he mentioned it recognized by Fournier, who said that the heaped-up horny layer formed " Une

véritable carapace." The disease in these cases attacks the whole of the soles, and occasionally also the palms. The salient features of the eruption are the enormous and irregular massing of overgrown horny layer. There is absolutely no marginate configuration, but here and there there are small indolent pustules, some of which show only staphylococci, and are in all probability due to secondary infection, while others bear the fungus in the roof.

The duration of this disease is extremely variable, but in most of the intertriginous and hyperkeratotic cases which I have seen the disease has been present for a considerable number of years, the longest period being over twenty years.

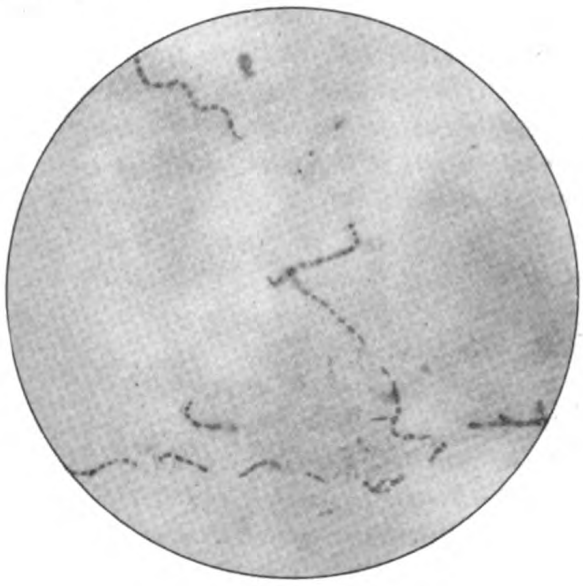


FIG. 1.

Ringworm from eczematoid eruption of feet; nine years' duration; stained methylene blue. Culture: *Epidermophyton inguinale.*

The complications of the disease appear to be due to secondary pyogenic infection with lymphangitis. Thus one of my cases was treated by a distinguished nerve specialist for neuritis of the lower leg, and another had a very swollen foot and ankle, which was believed to be due to true gout. This patient, by the way, was interesting from the facts, first, that it was entirely localized to the right foot, and secondly, that I found on looking up my notes that I had treated her husband six years before for inguinal ringworm.

Next as regards the frequency of the eruption. It is evidently no mere pathological curiosity. I have now seen fifteen cases of the

disease of one sort and another, and I find on looking up my notes that they form an absolute numerical majority of the cases of eczematoid eruptions limited to the palms and soles that I have seen during the last two years. I believe the reason for my having collected a reasonable number of cases only so lately is that it is extremely rare in the out-patient class, as I have already said. I have seen several cases of a typical ringworm of the hands in out-patients, but none of these were chronic cases, and of course I have been on the look-out for them.

Diagnosis.—The diagnosis lies, of course, in the demonstration of the fungus microscopically, but this is not always easy. Two methods

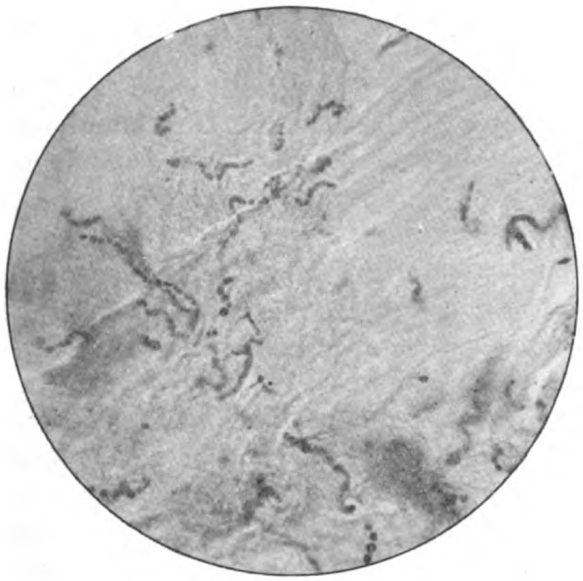


FIG. 2.

Ringworm from eczematoid eruption of feet; nine years' duration; horizontal section of scales stained with methylene blue. Culture: *Epidermophyton inguinale.*

have been chiefly used by me, namely, examination in potash, and staining with methylene blue. If one uses potash it is necessary to take certain precautions. The first is to peel off the scale from the most suggestive undermined part of the eruption, and to lay the scale with its deep surface upwards on the slide. By attention to this small detail it is commonly easy to see the fungus immediately, whereas if the scale is laid with the deep side downwards one may have to wait an hour or more before one can see sufficiently clearly to identify the fungus.

For the manufacturer of permanent specimens it is comparatively easy to imbed the thick scales as a tissue, and to cut horizontal sections so as to obtain nice specimens. The photographs were produced in this way.

The cultures, of course, vary according to the nature of the fungus causing the disease. I show two specimens of the epidermophyton, both practically identical, though from patients who had never met. The growth takes place in the form of a small heap, which later develops a rudimentary crater with much wrinkling, and is of a dirty buff colour. Pleomorphism, as pointed out by Sabouraud, occurs early. Also, I have brought the culture of the Helminthosporium and a culture from an out-patient, which turned out to be what I believe is a form of ectothrix. Culture is enormously difficult in these cases, the chief reason being that *Penicillium glaucum* is so common a contamination. With cocci and yeasts one may deal, as their rate of growth at a low temperature is comparatively slow, and the ringworm will outgrow them and may be subcultured, but the moment the blue discoloration of the Penicillium is seen on the tube or flask all is lost, as this mould overgrows everything so rapidly.

Lastly as regards treatment. There is no doubt that even in early cases the disease is quite unusually resistant for a surface tinea. In my researches I managed accidently to infect my own forearm by the jumping of a piece of scale when I was subdividing it. I discovered it at home on a Sunday, and having nothing much else I scrubbed it vigorously with tincture of iodine until it bled. Next day I painted an area about twice the size of the ring with pure carbolic acid, and of course produced a considerable superficial eschar, yet three weeks later when the scab fell off the ringworm commenced to grow again, and I found the new horny layer riddled with fungus. I then rubbed in an ointment of 5 per cent. benzoic and 3 per cent. salicylic acids in soft paraffin and coco-nut oil for three days and it disappeared.

I venture, therefore, to recommend the use of this ointment for general use in superficial tinea. It is not very irritating, although I have seen a certain amount of erythematous dermatitis set up by it in tender skins when used too vigorously; it is absolutely cleanly and practically odourless. For the eczematoid ringworm of the feet and hands it may be used stronger, and I have used it up to a strength of 1 dr. of benzoic acid and ½ dr. of salicylic acid to the ounce. I believe the base used, namely 2 dr. of soft paraffin and coco-nut oil to the

ounce, is of special value. I usually have it dispensed in a collapsible tube as it is of a rather curious consistency, being very stiff in cool weather, but running down to a thin oil immediately on being brought into contact with the warm skin.

This ointment does, however, sometimes fail, and then I think one should try chrysarobin in some form or other. A fairly convenient preparation is 1 dr. of chrysarobin dissolved in equal parts of chloroform, alcohol, and acetone to the ounce. This application may be employed in the morning on rising, and when it has dried, which it does in a very short time, the socks are pulled on over it. At bedtime it is washed off with soap and water, and nothing at all is worn at night. By this means the staining and destructive effects are limited to a couple of pairs of socks or stockings, whereas if it is worn at night it is almost certain to ruin the sheets, &c.

On the whole my experience leads me to believe that chrysarobin is a more powerful agent then the benzoic and salicylic acids mentioned above, but I have on occasions cured cases with the latter that have resisted the former. I am quite convinced that the application loses its effect after a time if the ringworm is not pretty quickly cured, so it is well to have several different formulæ.

(II) By Dr. R. SABOURAUD.

(Translated by Dr. SEQUEIRA.)

THE subject naturally falls into five parts:—

(I) The first, devoted to the *Eczema marginatum* of Hebra, caused by the *Epidermophyton inguinale* (Sabouraud).

(II) The second comprises the analogous "epidermophytie" produced by the *Trichophyton rubrum* of Castellani (*Epidermophyton purpureum* of Bang).

(III) The third includes the ringworms of the thickened horny epidermis, clinically studied by Djélaleddin Moukhtar in 1892.

(IV) The fourth comprises the ringworms of the back of the hands and feet usually caused by pyogenic trichophytons.

(V) In this section are differentiated from the preceding types the ringworm-like eczema in nummular plaques when it occurs on the extremities—a common situation.

(I) Eczema Marginatum of Hebra (Epidermophyton Inguinale).

Hebra's *Eczema marginatum* presents such distinct clinical characters that it would seem that the consensus of opinion of dermatologists would be unable to improve upon the masterly description made by Hebra. A whole medical generation has seen in eczema marginatum either an erythrasma or a common intertrigo, or a ringworm with a fortuitous localization in the groin. So much is this the case that our most recent medical works no longer describe it, and do not even mention it.

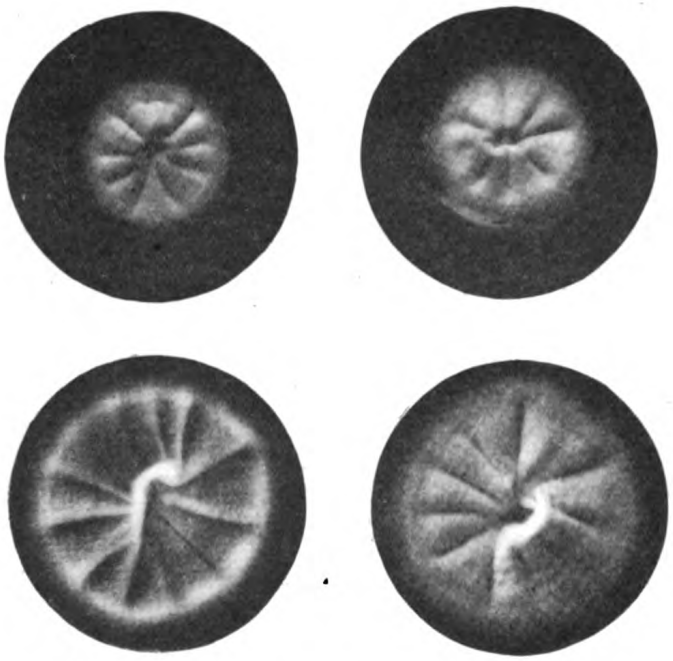


Fig. 1.

Four cultures of *Epidermophyton inguinale* on proof medium, fifteen and twenty-five days.

(1) *Erythrasma*, however, presents lesions very different in appearance from those of eczema marginatum. Its parasite cannot be cultivated and is much smaller than the *Epidermophyton inguinale*, the cultivation of which is easy.

(2) *Intertrigo* always occurs in the flexures, but not specially in the groin flexure. Its lesion is not marginate. It is almost always moist, while that of eczema marginatum is almost always dry.

(3) *Eczema marginatum* is not trichophytic. Its parasite does not attack the hair, it requires the bottom of a flexure for its inoculation. The aspect of the lesions produced, their outline, the different secondary localizations of the affection, its peculiar epidemicity, its whole symptomatology, make it a distinct disease. To the eye, the culture of its parasite differs from all the cultures of the trichophytons by its external characters, and the unique rapidity of its "pleomorphic" degeneration. Botanically, this parasite differs from all the ringworm fungi so much that the differentiation may be made by a glance at a preparation through the microscope. Eczema marginatum is therefore a distinct disease to which Hebra was right in giving a special name, though the name he gave it is not a happy one.

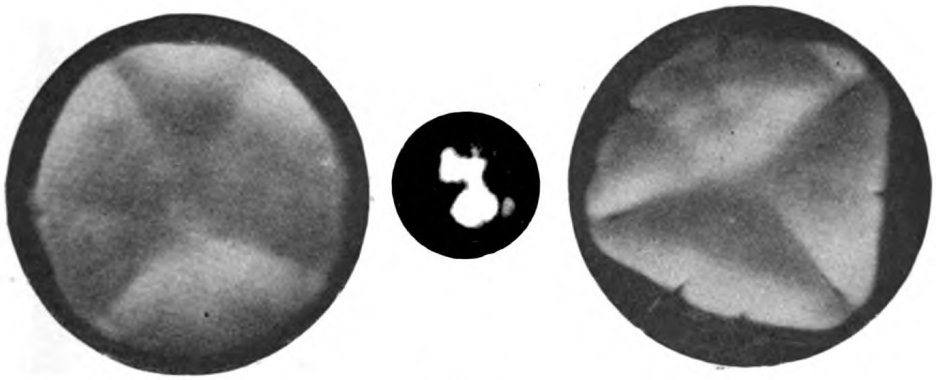


FIG. 2.

Cultures of *Epidermophyton inguinale* showing pleomorphism.

Clinical Description.—The description Hebra gave of it remains true in all particulars. He described it in its ordinary and in its rare situations, with the polycyclic form of its plaques, and their festooned outline, their red margin and buff-coloured, scaly centre, &c. When the lesions multiply they develop in the axillary flexures, in the umbilical fold, and in women in the submammary fold. Outside the flexures they may develop, but tend to die away spontaneously.

Eczema Marginatum of the Flexures of the Toes.—What is newest in the history of the disease is the fact of its frequent occurrence in the flexures of the toes, a localization which I described last year. I can say to-day that eight out of ten cases of so-called "intertrigo" of the toes are caused by the *Epidermophyton inguinale.* On the feet, as elsewhere, the localization of the parasite is at first at the bottom of the flexures, in the interdigital clefts, but ultimately it reaches the folds on

the flexor surface. The eruption overruns these folds but it keeps them as a centre. The lesion resembles that of intertrigo, with which it has always been confused; oozing at the bottom of the flexures, dry outside them, its red margin is seldom well defined and only becomes perceptible when it reaches the dorsum of the foot. One of the symptoms which facilitates the diagnosis is the existence around the lesions of peripheral isolated vesicles raising the epidermis and filled with a clear or turbid fluid. Another very important feature is the co-existence of typical inguinal lesions in the same subject.

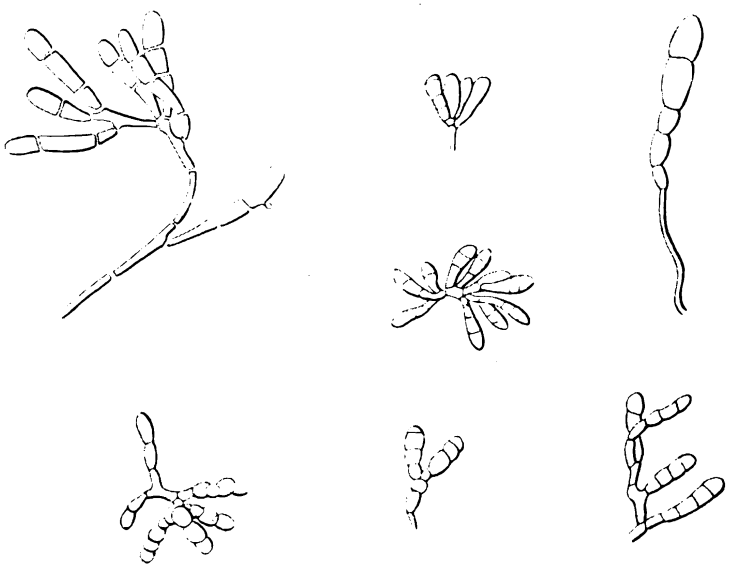


FIG. 3.

Fructifications of *Epidermophyton inguinale* in culture.

The demonstration of the parasite in the affection of the foot may present some difficulty when the almost dry lesions are bounded by a callous margin. It is under this thickening that one must seek the fragments of scales for examination, but one may have to make four or five examinations before meeting the parasite. On the other hand, if one finds it, it is abundant. It forms mycelial networks exactly like a trichophyton.

Diagnosis and Treatment.—This localization of eczema marginatum to the flexures of the foot constantly gives rise to errors of diagnosis and treatment. I have seen such lesions treated for two years without success as an eczema, by various measures, including spa treatment, cured in three weeks by iodine and chrysophanic applications; for the

cure is easy and certain if a correct diagnosis has been made. The conclusion of this is that the physician confronted by an intertrigo or an eczema of the feet must always think first of the eczema marginatum of Hebra. *On the hands* eczema marginatum is rare. I have only seen one case in a man who had the disease in its typical situations in the groins and on the feet. The diagnosis remained uncertain for two months, and was only made when there appeared along the index finger a quarter of a circle, the scales of which showed the parasite on

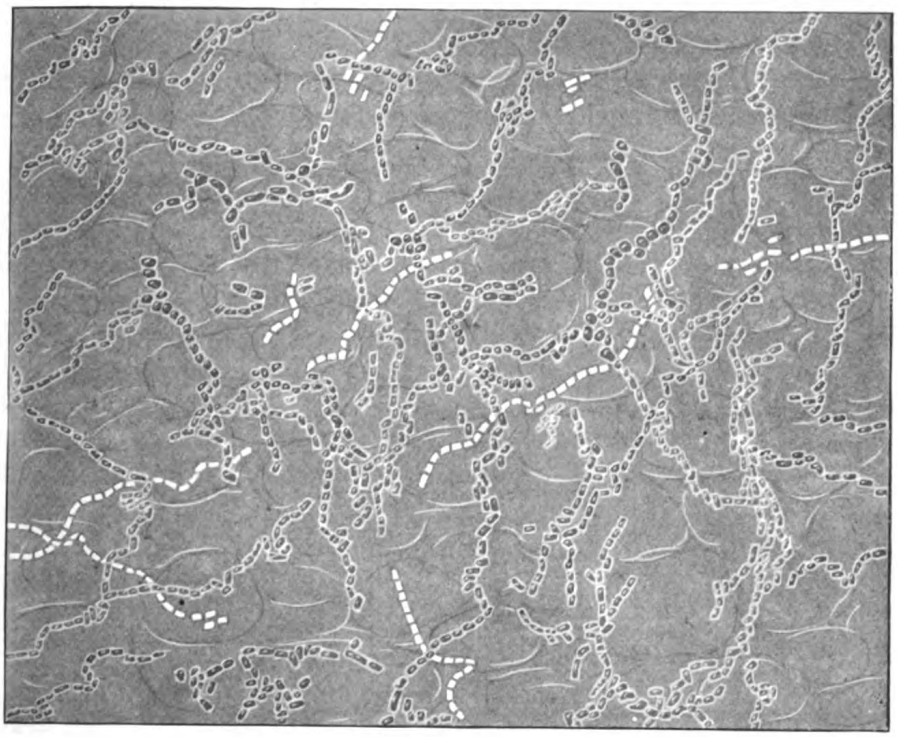


FIG. 4.

Eczema marginatum of inguinal fold. Microscopical specimen of scales, unstained.

examination. Finally, I obtained the parasite in isolated vesicles, very similar to those of eczema and dysidrosis, along the sides of the fingers. Here the error is at first inevitable. A probable diagnosis can only be made if one finds on the feet and in the groins the characteristic lesions. Certainty only follows a microscopical examination, and one may make many negative examinations before finding what one is looking for. In France, at any rate, this eruption appears to be rare on the hands,

although it is frequent on the feet. On the feet it is really very common. To sum up, this is a very definite affection, the knowledge of which is indispensable to the physician. Left to itself, it is contagious and of indefinite duration. Well treated, it may be cured in a few weeks. The cure is effected by the application of 1 per cent. solutions of iodine and chrysophanic acid. The applications must be well made, that is to say by hard friction, and after the preliminary decortication of the callous borders of the lesions when such exist in the regions where the horny epidermis is thick.

(II) Trichophytic Eruption caused by the Trichophyton Rubrum of Castellani (Epidermophyton Purpureum, Bang).

Next to the eczema marginatum of Hebra must be placed a trichophytic erythema which may involve large areas of the body, which was first studied in my laboratory by Dr. Henrik Bang, of Copenhagen. In three out of four cases there was over the whole body an eruption of disseminated ringworm, made up of large and small patches, forming regular maps, of a buff colour, limited by red points excoriated by scratching, these points marking out the polycyclic contour of the lesions. The lesions may occur anywhere, particularly on the trunk, the buttocks and the thighs. When they occupy the inner aspect of the thighs they may lead one to think of eczema marginatum. But this eruption is distinguished by its generalization, which eczema marginatum never attains. Eczema marginatum is an eruption of the flexures, the trichophytic eruption I am now considering is an eruption of flat surfaces. This morbid type is not described in my book, "Sur les Teignes." It was discovered after its publication. H. Bang has described it in the *Annales de Dermatologie*, 1910, p. 229. This is the culture of the parasite which Bang has called *Epidermophyton purpureum*. It forms a carpet of white down, but under the white down there is a layer of purplish pigment, which is easy to see where it joins the side of the glass. In Europe this parasite appears to be always imported. It has been seen in cases coming from Mexico and Indo-China.

A little before Bang studied and named his parasite, Castellani, of Ceylon, had discovered it and called it *Trichophyton rubrum*. Professor Castellani was kind enough to send me from Colombo the cultures and papers which show without doubt the priority of his discovery and his observations.

I have just seen a case of this affection in a patient who has recently returned from the East, in whom the *Trichophyton rubrum, vel purpureum,* had caused a parasitic intertrigo of both feet, identical with that which is caused by the *Epidermophyton inguinale.* It would, therefore, appear that in other countries—for instance, India or Siam—parasitic intertrigo of the toes may be caused either by the *Trichophyton purpureum* or the *Epidermophyton inguinale.* What is certain is that

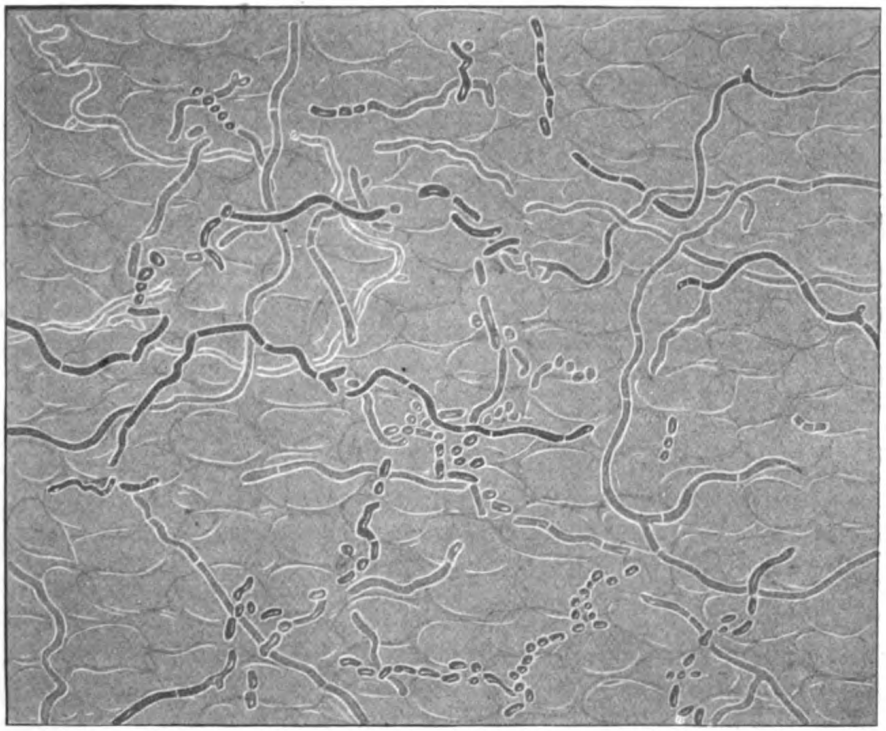


FIG. 5.

Eczema marginatum of foot. Microscopical specimen of scales, unstained.

the latter parasite exists even in Ceylon, where Castellani has isolated it from inguinal lesions.

Even to the naked eye the diagnosis between the two parasitic types ought to be generally possible, for the localization of the *Epidermophyton inguinale* in the cutaneous flexures is almost constant, and exceptional in the case of the *Trichophyton purpureum.* In the case in which I saw the latter produce an intertrigo of the foot, there was a parasitic patch of a very eczematoid character at the tip of the internal malleolus.

(III) Palmar and Plantar Ringworms.

Besides the two types we have just studied, it is necessary to speak of the palmar and plantar ringworms mistaken for local eczemas, described in the work of Djélaleddin Moukhtar in 1892. This author first showed their trichophytic nature. He described their characteristic signs, their long duration in situ, their polycyclic form, with a thickened and raised peripheral epidermal border, their slow extension, &c. The very good models of Baretta, corresponding with this work, of which I have published photographic reproductions, show well the important signs of these lesions, the diagnosis of which as a matter of fact is easy enough. The work of Djélaleddin Moukhtar was purely clinical and microscopical.

The study of the mycology has shown me the different fungus parasites which usually cause these eruptions. The most common is the *Trichophyton violaceum*. Then comes the *Trichophyton acuminatum*. Once in a lesion on the thenar eminence I have met the true *Microsporon Audouinii*, which one so rarely finds in chronic cutaneous lesions.

(IV) Ringworm of the Back of the Hands.

Ringworms of the back of the hands and fingers are of a quite different clinical and mycotic type. They are commonly suppurating ringworms, approaching the type of Kerion Celsi, and they are almost always due to the *Trichophyton microïdes*, of the types *gypseum* or *niveum*. Sometimes, after having infected a finger or an interdigital cleft, the lesion may invade by its edges the palmar surface of the hand. As a rule it does not spread there to any great extent. The evolution of these ringworms is, moreover, acute or subacute, forming phlyctenules full of turbid fluid, an evolution which contrasts with that of the primary palmar ringworms, which are chronic and dry.

We have, therefore, to distinguish between the dorsal ringworms of the hand and the palmar ringworms. They rarely have analogous characters and are rarely caused by the same species of fungus.

(V) Nummular Trichophytoid Eczemas of the Back of the Hands.

Besides the types of ringworm which we have just reviewed, the physician ought to know that all the ringworm-like lesions of the

extremities are not ringworm. There exists, especially upon the back of the hands, the thumbs and the forearms, an eczema in rounded, nummular patches, in which I have often and for a long time searched for trichophytons, but have never yet discovered them. My confrères have often sent me patients affected with this form of eczema, asking me if there were no fungus present, and many have been very astonished at my disagreement with their diagnosis. They are roughly orbicular, round or oval lesions, which may reach 4 cm. in diameter, rose coloured on their whole surface, excoriated, weeping or dry. They are chronic lesions often produced by contact with irritants used in the work of the patient (professional eczema). Most often these lesions, small or large, disseminated even to the tip of the thumb, are five to seven in number, too numerous to justify the hypothesis of ringworm. And there are some small spots among the large, and the smallest are more clearly eczematous and less like ringworm than the large. Their aspect, like the corolla of a flower, with radiating petaloid divisions, leaves no doubt as to the diagnosis. But they are eczemas resistant to the usual treatment and they bear very well nitrate of silver and even chrysarobin.

Summary.—Conclusions.

If one wishes to summarize what can be said about the eczematoid ringworms of the groin and extremities, one is able to arrive at the following conclusions:—

(1) The principal eczematoid ringworm is the *Eczema marginatum* of Hebra, caused by the *Epidermophyton inguinale*. Its primary and principal site is the groin flexure, but it often determines a so-called intertrigo of the feet and, rarely, eczematoid lesions on the hands.

(2) An analogous ringworm, but usually causing a trichophytic eruption disseminated all over the body, which may also be situated on the feet like the preceding. These lesions are due to the *Trichophyton rubrum* of Castellani (*Epidermophyton purpureum* of Bang). It appears that this is an exotic parasite.

(3) Besides these parasites, several may give rise to the chronic dry ringworms of the thick horny epidermis described by Djelaleddin Moukhtar on the sole of the foot and the palm of the hand. They are usually caused by the *Trichophyton violaceum* and *Trichophyton acuminatum*.

(4) Finally, on the back of the hands ringworms are common. They are usually suppurating ringworms caused by the pyogenic

trichophyta of the *Trichophyton microïdes* group, of the types *Trichophyton gypseum* and *Trichophyton niveum.*

But there occurs on the back of the hand, thumb, and forearm a persistent nummular trichophytoid eczema, with little weeping, which is not a ringworm, and in which the aspect of the early lesions permits one to distinguish them objectively.

DISCUSSION.

Dr. J. J. PRINGLE said his contribution to the discussion would be a very humble one and from the purely clinical point of view. In his hospital practice during the previous eight and a half months, out of 1,034 patients ten cases of eczematous and vesiculo-bullous lesions of the extremities which appeared likely to present the epidermophyton had been examined microscopically. All were, however, affections of the hands—including "dysidrosis" and "interdigital eczema"—and not a single case of "crutch eczema" or of interdigital eczema of the toes had presented itself in his out-patient department during that period. In only two of these hospital cases had fungus been found, and in neither of them did the clinical appearances suggest ringworm in the first instance, as both showed only isolated large pemphigoid blebs on the thumbs and index fingers. One of the cases occurred in a school-girl, aged 13, and the other in a washerwoman, aged 27. In both a broad, double-outlined, jointed mycelium was easily demonstrated in potash preparations, but cultures were not obtained from either of them. They recalled two cases described by M. Sabouraud as herpès circiné ("Les Teignes," p. 300), which occurred on the hands and were due to a *Trichophyton violaceum*, to which the speaker's attention had been drawn by Dr. Adamson.

On the other hand, in his private practice, out of 1,127 cases he had observed twenty-two instances of "crutch eczema," in eight of which the epidermophyton had been demonstrated, but in none of them had co-existing interdigital eczema of the feet been noted. Several cases of eczematous conditions of the feet not associated with crutch eczema had been investigated during the same period with negative results, but that might, perhaps, be due to some deficiency in his technique, the occasional difficulty of which had been emphasized by M. Sabouraud. Of these eight cases of "eczematoid ringworm" of the crutch or axillary regions four occurred in women. (Probably many of the remaining fourteen were instances of inflamed erythrasma.)

In four of the eight cases there was no evidence as to the manner of contagion, but two of the other cases occurred in a husband and wife, and two in a gentleman and his paramour. The fungus in all four cases presented the usual microscopical characters of the epidermophyton, and cultures by Mr. A. G. R. Foulerton, which grew freely on agar, accorded with M. Sabouraud's description, but their primary characters were soon obscured by the predominance of pleomorphic forms.

On throwing his mind as far back as 1876, when he was a house physician in the Edinburgh Royal Infirmary, he very clearly remembered his first case of "eczema marginatum" of the groins. It occurred in the person of a cowboy, and the direct infection from a cow to the patient was admitted to be quite possible. He had had a similar case in London about twenty years ago, also in a cowman, who gave an identical history, with the additional fact that ringworm was known to exist among his cattle.

He would like to say a few words about the four *epidemics of eczematoid ringworm* which he had had the opportunity of studying fairly closely. The first of these occurred in 1903 in a sanatorium for tuberculosis. Seven *men* out of about forty inmates in the institution (half of whom were females) were affected with eczematoid ringworm of the groins, and in most of them the axillæ were also involved. In all, the organism now called the "epidermophyton" was easily demonstrated. A very peculiar point about the epidemic was that all seven men belonged to a party who played "bridge" in one particular shelter. No other person in the sanatorium save those seven bridge-players was attacked. The second epidemic of which he had actual cognizance was also in a large tuberculosis sanatorium; he was informed that about twenty *men* were affected, but *no women*. He saw four of them, and all presented a large jointed mycelium which he did not doubt was Sabouraud's epidermophyton. The third epidemic which he had observed closely occurred in a residential institution for thirty young *men* in London, twelve of whom were infected. The first case which occurred was possibly of sexual origin, but the rapid spread of the disease to other residents could not possibly be accounted for in a similar manner. The fourth outbreak was in a preparatory school for little *boys* in the neighbourhood of London, and fifteen of them out of a total roll of forty had eczematoid ringworm with typical fungus. A study of these various epidemics had convinced the speaker that *infection from water-closets* was an extremely likely explanation of their occurrence and rapid extension. Two of his private cases occurred in married

ladies and confirmed this impression in a rather curious manner. In neither case was the husband affected, so a source of infection had to be looked for elsewhere; one of the ladies was an inveterate "racing woman" who used water-closets indiscriminately at various race-meetings, and the other, who lived in a suburb of London, came up almost daily to town and invariably used the water-closet at the railway terminus. The careful disinfection of water-closets seemed therefore to be an essential part of the treatment of such epidemics, as well as of sporadic cases. One of the eight private cases attended to was in a gentleman from the Taquah district of the Gold Coast, where it had been recognized as a case of "kraw-kraw." The lesions were identical with eczematoid ringworm as seen in this country, as was the fungus which Mr. A. G. R. Foulerton found to grow freely on agar.

In thanking and congratulating the openers of the discussion he asked whether they or any other Fellow present had any evidence to impart confirmatory of his observation as to the possibility of the communication of eczematoid ringworm from animals to the human subject by direct contact.

Dr. G. PERNET said that since the year 1892 he had examined quite a number of specimens obtained from various parts of the body —from between the toes, the hands, the axilla, the inguinal region, and in one case from the gluteal cleft. In many of these he was able to find the fungus, and from some he was able to obtain cultures. He had been able to cultivate the *Epidermophyton Perneti*, which was a rosy-pink fungus. One of these cases came from China and another from South Africa. This was not surprising when one remembered that there was a good deal of communication between India and South Africa. When Dr. Castellani cultivated a pink fungus he at first thought it was a newly discovered one, but on finding that Dr. Pernet had described it some years previously, he very kindly gave it Dr. Pernet's name. Most of the cases which Dr. Pernet had examined came from abroad—India and the East generally, but in some cases the patients had not been in the East. Dr. Radcliffe Crocker and he had collaborated, and intended to publish an account of these conditions incorporating the microscopical work and cultivations carried out by Dr. Pernet. He quite agreed with what Dr. Sabouraud said as to the upper classes being the chief sufferers from the condition about the groin. Dr. Crocker and he had noticed that these conditions about the hands and feet, especially about the feet, always seemed to die down in

the winter. When patients came from the Tropics and hot countries to England the condition improved to some extent, but when they returned to tropical or subtropical climates the condition became just as bad as before. With regard to treatment, he had found that chrysarobin answered best, but other remedies were also useful. He quite agreed with Dr. Sabouraud that it was desirable, and even necessary, to remove the upper layers first of all, so as to enable the drug to have a more direct influence on the fungus. The condition was a difficult one to tackle, and was obstinate in many cases. This was especially the case when the toe-nails were affected, because apparently the nails could be a source of reinfection of the other parts. In another series of cases he found the epidermophyton in patients who attended hospital, and were engaged in unpacking Japanese goods. He thought such packings from Japan and other Eastern countries were a possible source of infection. He had examined some of the packing, but had not been able to find the fungus.

Dr. COLCOTT FOX said that since the publication of the discoveries in eczematoid troubles of the hands and feet, he had examined suspicious cases which came under his care, and had found the fungus in one or two, but had been most unfortunate in his cultivations. In only one case was he quite successful; this was in a boy with an excoriated rounded patch occupying the palmar surface of the right thumb, which suggested to him the possibility of ringworm. On the backs of two or three fingers there were large well-formed bullæ, which appeared to be indistinguishable from those of pemphigus or streptococcic infection. He found chains of fungus which were successfully cultivated. Still, he did not pronounce definitely on the species of fungus, but he believed it to be epidermophyton. He had expected more to be said as to the extraordinarily increased prevalence of these ringworms in the last few years in this country. He had for many years seen an occasional sporadic case, and also examples from hot countries of tinea inguinale. But in England itself it had not been a common condition. Some years ago, indeed, he wrote a short paper on a few family epidemic outbreaks, because such an occurrence was exceptional. But during the last few years there had been a wide prevalence of the condition all over England. It was now well known that in the majority of the great public schools there had been large numbers of cases. It had also been prevalent amongst Army officers; and from those sources it had become disseminated amongst the general population, but chiefly in the upper classes. He had not seen it in the poorer classes.

Dr. H. G. ADAMSON said that it had for long been known that ringworm might attack the fingers and toes, the palms and the soles; but until Dr. Whitfield and Dr. Sabouraud called special attention to these cases we had not realized how common the disease was in these parts, and that it might be mistaken for eczema, nor did we recognize its association with ringworm of the groin. He would briefly relate his own experiences of ringworm of the extremities. The first case of this kind that he remembered was in a patient under the care of Dr. Pringle in 1898, with a scaly palmar eruption which had been diagnosed as syphilis, but which Dr. Pringle thought to be ringworm. From this case he had obtained a peach-coloured culture (which he now showed), which Dr. Sabouraud had since called *Trichophyton persicolor*. Of the plantar ringworms, which Dr. Sabouraud said were first described by Djélaleddin Moukhtar in 1892, he had seen four examples, from which he had obtained fungus but not cultures. In these cases the disease affected the spaces between two or more toes and spread for some distance on to the sole of the foot. The effect was as though the thick epidermis had peeled off, leaving a red glazed surface with a sharp margin formed by the overhanging edge of the normal thick epidermis. In a second class of cases there were herpetiform groups of tiny vesicles upon the fingers, hands, or feet. Three such cases were as follows: (1) A man who had a violet endothrix ringworm of the beard, a papulo-vesicular ring on the back of one hand, and "eczematoid" lesions on the side of each forefinger. *Trichophyton violaceum* was obtained from the lesions on the fingers. (2) A man who had a large area of scattered vesicles on the inner side of one foot simulating cheiropompholyx—*Trichophyton gypseum*. (3) A nurse-maid who was looking after a child with an enthothrix ringworm of the scalp, and who developed an "eczematoid" patch on the thumb and extending on to the palm. From this case he obtained a culture of *Trichophyton crateriforme*. Finally, there were those cases of ringworm between the toes which were associated with tinea cruris. Since his attention had been called to these cases by Dr. Sabouraud's article in the *Annales* in 1910 he had seen six examples during the past eight or nine months. These cases had all been associated with tinea cruris, either present at the time or already cured. They had all been in males, and he had not seen the fingers attacked. The lesions were mere excoriations or dry red areas between two or more toes, with minute vesicles at their margins where the skin was thin, and an overhanging fringe of epidermis where the skin was thick towards the sole. In one case the patient was

wearing fingered socks; it had been regarded as eczema for several years. Cultures (exhibited) were obtained without difficulty from each of these six cases. They corresponded with Sabouraud's *Epidermophyton inguinale* in the following particulars: (1) They grew slowly; (2) they had a powdery surface; (3) they quickly developed white pleomorphic growths which sometimes enveloped the original growth; (4) they showed the characteristic fruit organs, namely, club shapes and abortive spindles. Two features were absent, or at any rate less constant. First, the colour of the cultures was only occasionally greenish-yellow, more often yellow or dirty grey. Secondly, the cultures did not conform to one pattern; sometimes they presented a central knob with a skirt, or the knob was a small crater, and the surrounding skirt pleated; or the culture was wholly crateriform. These colours and forms were taken by the same growth in different tubes, for the colour and shape of the growth seemed to be very easily modified by slight variations in the medium or the conditions under which it grew. The fruit organs, however, distinguished the growth from the ordinary *Trichophyton crateriforme*, the fruit organs of which were grape-like spore masses. It was to be noted that all the cases of ringworm of the toes due to *Epidermophyton inguinale* had been associated with groin ringworm, and the exhibitor believed that they were always secondary to that form of ringworm. As regards the method of infection of the groin ringworms he was of opinion that this took place through chairs which were in common use in institutions, schools, &c. He believed that the scales worked their way into the trousers and thus on to the chairs, and vice-versa to another person. This would explain the occurrence of the eruption only in men—the thicker clothing worn by women preventing infection in this way.

Dr. J. L. BUNCH brought a culture in which he had been interested—namely, a purplish one, which he obtained from the horny layers of the foot. He had not seen anything of the kind before. But he now saw that Dr. Sabouraud had several more luxuriant cultures of exactly the same condition. He had been interested to hear how seldom eczema marginatum had been found in women, and recalled the first case of the kind which had come under his care. The patient, a young medical man, from whom he obtained typical cultures, had failed to recognize the lesions in his groin, and had taken no precautions to avoid infecting his wife, but she had, nevertheless, shown no skin symptoms of any kind.

Dr. S. E. DORE said he would like to refer to one case, which was under the care of the President. It was that of a woman who had ringworm of the axilla, and Sir Malcolm, wondering how she got the ringworm in that position, asked to look at her toes. The ringworm fungus was found there. As to the technique for the examination of the scales, he found it more satisfactory to examine the scales in liquor potassæ than by staining, provided the specimen had been soaked in the former sufficiently long. It was scarcely necessary to mention the fallacies which liquor potassæ might lead to, but he had been struck by the curious appearances caused by crystallization of the liquor potassæ and the way in which fat sometimes insinuated itself between the epithelial cells, so as to simulate spores or mycelium. He had heard it stated that it was not always possible to see the fungus in potash, and that without staining one could not make the diagnosis. But this was not his experience, although there was, of course, no difficulty when the fungus was stained successfully.

Dr. SEQUEIRA said that there was a special feature which had not, in his opinion, received the attention it deserved, except in a few words which had fallen from Dr. Adamson—namely, the prevalence of the disease in the male as compared with the female. He thought that everyone would agree that an enormous proportion of the cases seen occurred in the male. He had investigated an epidemic which occurred in a sanatorium in which there were fifty male and fifty female patients. He found eczema marginatum in fourteen out of the fifty males but amongst the females there was not a single case. This disposed of the possibility of the disease being propagated through the linen sent to the laundry, for the whole of the washing was done in one place. The disease was in this instance almost certainly due to water-closet infection. With regard to the question of clothing, one must recognize, that at any rate in this country, there was an enormous number of lads and men in the upper and middle classes who did not wear drawers. He believed that this was a very important factor in the chronicity of the affection. He thought it certain that the infection resided much longer in the fabric of trousers, and the disease occurred just where the trousers would tend to rub the inner side of the thigh. The fact that the condition was met with more commonly in certain public schools than in others was probably due to this custom of not wearing washable under-garments. In one case of groin ringworm under his observation the infection was undoubtedly caused by the wearing of borrowed

flannels used in a well-known gymnasium. Dr. Sequeira had seen several cases of eczematoid ringworm of the toes, and had been able to recognize their nature, since attention had been drawn to the condition by the openers of the discussion. In some instances there was no groin ringworm at all. In one such case he traced the infection to a Turkish bath, the patient being infected either from the slippers lent to bathers or from the floor of the Turkish bath. The subject of the eczematoid ringworm was of the first importance to dermatologists and to the public, and he believed that a discussion of this kind was of the greatest value in leading to careful examination for trichophytic infection in hitherto unsuspected parts of the body.

Dr. GRAHAM LITTLE said he would like to confirm from his own observation of one case the accuracy of the description given by Dr. Whitfield in the second group which he had described that evening, and the clinical history of the case would, he thought, point to some methods of spread of the disease. The case occurred in his private practice three years ago, before the subject had been elucidated. He mistook the case for one of ordinary ringworm of the groin. It occurred in a doctor, who came to him with the affection very well marked in the groin, but he had also what appeared to be a weeping eczema of the toes. He took specimens from the groin and toes, and was surprised to find the fungus in the toe-products as well as those from the groin. The patient had been living in a room which had been vacated by an attaché to the Turkish Embassy; he was of very cleanly personal habits, and was much distressed by this affection. This gentleman, on investigation, found that a previous tenant of the rooms had had ringworm, and so it was concluded that the rooms were the source of infection. Yet, before taking possession he had washed out the drawers and lined them with paper. In this case also the course of the disease was very protracted, and the irritation, which Dr. Whitfield laid stress on, was one of the marked features; the patient was driven almost crazy by it. Dr. Little's experience had been the same as others as to the cases of eczema marginatum hailing chiefly from Eastern parts. He did not think that the treatment of the condition was very satisfactory; in the case of which he was speaking it was six months before the symptoms could be said to have disappeared. He found tincture of iodine as useful as anything. It was some satisfaction to him to hear that other members who had charge of hospital clinics had also had a difficulty in identifying the fungus in out-patients, as he had put his own failure

down to a fault in his technique. Sir Malcolm Morris, at a meeting some time ago, hazarded the suggestion that most of the cases of dysidrosis might be of the nature under discussion. He (Dr. Little) had examined cases of dysidrosis with that in mind, but had not succeeded in finding the fungus in any one of them.

Dr. BOLAM (Newcastle) desired to ask if the condition which had been described as occurring in the toes might not be produced by other kinds of ringworm than the epidermophyton. He had seen three cases since Dr. Sabouraud's paper was published, and in two of them he had succeeded in cultivating the crateriform fungus. In the third case he obtained the ordinary epidermophyton. In the two cases no history of previous eczema marginatum was given by the patients who were medical men, and therefore quite qualified to judge. There was no doubt that they had not any contamination of the groin previously to the condition in the feet, which was identical with the classical description given by the distinguished visitor of the evening. The treatment in these cases was a little difficult, especially in the case of the elder man, who would not submit to anything drastic. But in the case of the younger man, following Sabouraud's line of treatment, chrysophanic acid was successful. In cases of the ordinary eczema marginatum he had had success by adopting a method of treatment which, though somewhat drastic at the time, yielded rapid results—namely, painting with iodine in the first instance, and making use of the violent reaction produced by the subsequent application of sulphur ointment. By grading the strengths of the two applications the action could be kept under control. This caused the eczema marginatum to clear up quickly. He thought the spread of the contamination in public schools was explained by the way in which boys snatched up and wore the first football knickers which came to hand, as well as the habit of omitting to wear washable linings to the trousers.

Dr. A. M. GRAY desired to mention two or three cases in a small epidemic of eczema marginatum which had come under his notice in the last three days. They occurred in a large institution where many young men resorted, and the patients affected all made use of the gymnasium and baths attached. One of the cases had been persisting for some years, and was possibly the source of the infection. He thought it very likely that the use of the towels was largely responsible for the spread of the disease. The baths had been used by the affected people for

some years past, but it was only recently that some carelessness had occurred about the use of the towels. Previously each man had to provide himself with a fresh towel and take it back again, but recently the towels had been left about, and other people had used them. An interesting fact was that one or two other people using the same baths had had slight eczematous lesions about the groin, which had now disappeared. Possibly the prevalence of the condition in public schools might be due to the promiscuous use of towels when bathing and changing after games.

The PRESIDENT (Sir Malcolm Morris, K.C.V.O.) said he felt sure the discussion would be fraught with the greatest possible benefit to the community. It would lead to the careful examination of every case of groin eczema which one encountered, in order to ascertain whether it was a parasite case or not. Another important result of the subject having been brought forward would be that dermatologists would be led to examine carefully the condition of the finger-nails and toe-nails. In this way the specialty could do very much towards stamping out the disease, which at present was very rife, and there was evidence that it was spreading. As Dr. Colcott Fox said, the disease was much more frequently met with now than formerly. His own feeling was that the increase in the number of cases was largely due to the South African War; many cases came from that country. The condition not being understood and it not being suspected that the toes might be attacked by the disease, in many cases the condition of the groin improved but no routine examination of the toes was made. Ever since his attention had been directed to the matter he had made it a matter of routine to examine the toes. This was admittedly a worry and added to the trials of practice, but it was a duty which the profession owed to the community. There was one particular type of case in which he had had the greatest possible difficulty in proving whether the fungus was answerable or not—namely, that very common form in which the tissue between the toes had a white, sodden appearance. On a routine examination of the toes being made it was surprising in how many cases had this condition occurred. His own opinion was that they were all parasitic, but he found great difficulty in proving it; in some cases however, the fungus had been found. The difficulty was due not so much to faulty technique as in taking the right material for examination. Still, with the extension of knowledge on the subject, and the greater opportunities of examining the cases, it was hoped that a

method of more easily identifying the condition would be discovered. In other cases of acute eczematous conditions there was not the same amount of difficulty, nor in the class of desquamative dermatitis of the palms and soles, in which large flakes of skin were given off. With regard to the method of infection—which, from the public standpoint, was by far the most important matter—he had seen many cases in public schools. In one particular public school, which many present might be able to identify from his remarks, it was considered to be bad form to wear drawers. He had written to the doctors connected with that school protesting against such omission. He had seen one boy three times with an affection of the groin; he was cured, and then was re-infected. This was no doubt due to the boy wearing the same trousers as before; it was, as was well known, the habit of boys to go on wearing trousers for an indefinite period. The dirty habit of wearing no drawers next to the skin was answerable for much of the trouble. If the scales could go through the trousers and contaminate seats, as Dr. Adamson suggested—an idea he could not quite subscribe to—it was easy to conceive of the infection being conveyed by bed-pans and water-closets. He knew of an epidemic in which there was no doubt contamination occurred through the bed-pans in a ward; the bed-pans were not covered with flannel. It seemed that the condition went from the groins to the nails by means of scratching, for this form of eczema marginatum was very irritating. In the first instance a large number of the cases seemed to have come from South Africa, India and Ceylon, as well as other tropical places. It was of peculiar interest that this particular form of trouble was almost limited to the upper classes.

Dermatological Section.

December 14, 1911.

Sir MALCOLM MORRIS, K.C.V.O., President of the Section, in the Chair.

Case of Pityriasis Lichenoides Chronica or Lichen Variegatus.

By WILFRID FOX, M.D.

THE patient was a woman, aged 49. She first came to the out-patient department five years ago complaining of a red and burning face, which the exhibitor diagnosed as eczema. The condition at that time was confined to the face and upper part of the neck. There was then very little scaliness, no exudation, and no attempt at formation of bullæ. All manners of local treatment were tried for about a year or eighteen months without producing any effect at all, although the skin, especially round the neck, became a little thickened and lichenified, but not to the extent seen at the present time. She was then under the care of Dr. Dawson, and was shown by him before the Section three years ago. About two years ago she returned to St. George's Hospital for treatment, and the exhibitor then found that the condition had progressed considerably. The red scaly area came down on to the chest, ending off very sharply about the top edge of the mammæ and on the back about the centre of the shoulder-blades. There were also patches on the buttocks and thighs. They were all of them very much as seen at the present time, that is to say, red pigmented areas considerably infiltrated and lichenified, with adherent scales over the greater part. The pigmentation has increased perhaps in some of them owing to treatment. The patient was perfectly well, only complaining of the intense itching, which made sleep difficult. Various

anti-pruritic treatments were tried, but the only thing which appeared to control the irritation was the leucodescent lamp, although after a time this, too, began to lose its power. X-rays had no effect in relieving the irritation, and, if anything, rather increased it. From time to time there have been spots which were peculiarly irritable, and which appeared to be the centres from which the irritation started. Some of these had been treated with the actual cautery with considerable benefit, otherwise the only application which appeared to have any beneficial effect was an ointment of menthol and carbolic acid, although of course this was only palliative. During the last six months there have been a few isolated bullæ appearing, particularly on the neck and shoulders. The face, apparently of its own accord, has improved a good deal, and is now not so red or scaly, and is also less irritable.

DISCUSSION.

Dr. PRINGLE said this patient had been under his care, and his diagnosis was lichen variegatus. He founded that opinion to a large extent on the ribbon-like arrangement of the lesions. His conception of the disease described by Juliusberg and other German dermatologists as pityriasis lichenoides was entirely different from the condition presented by the patient. He thought he had identified two typical examples of it in this country within the last year.

Mr. DAWSON said that when he saw the case he showed it as one of parakeratosis variegata.[1] The eruption then occupied much the same position as now. The lesions which Dr. Pringle referred to were better marked down the arm, flat and shiny looking. There was an urticarial margin. At that time Dr. Crocker agreed with the diagnosis.

Dr. PERNET said that in these cases of extreme irritation of the skin, which did not yield to medical treatment or to X-rays, a lumbar puncture often gave a good result, at least for a time. Six drachms of the cerebro-spinal fluid should be removed.

Dr. GRAHAM LITTLE said he much regretted he had been unable to bring the case of parakeratosis variegata which he had announced on the agenda list, and which would have formed an excellent comparison case. The patient, a young lady, aged 30, had shown eighteen months previously the beginning of the retiform type of the disease, which had slowly spread, until at the present time all the body, with the exception of the hands, feet and face, were involved.

[1] *Brit. Journ. Derm.*, 1908, xx, p. 260.

Case of Recurrent Granuloma Annulare.

By E. G. GRAHAM LITTLE, M.D.

THE patient had been seen about five years ago, and the description of his eruption, both from clinical and histological standpoints, was given in the exhibitor's paper on that subject,[1] under the initials W. S., Case 47. He had been entirely free from the eruption for some two years after that record was made. This had commenced again in the form of small reddish discrete nodules on the wrist and elbow, but especially in the former position. There was at present no ring-formation, but the nodules were exactly like the early nodules seen before in this patient and in the same positions, and the exhibitor had not a doubt that it was the same disease now appearing without ring-formation, which in the earlier attack had been very pronounced. The exhibitor had not previously seen a case of granuloma annulare showing recurrences after a long interval of freedom, and brought this case to show that recurrence was possible.

Case for Diagnosis.

By E. G. GRAHAM LITTLE, M.D.

THE patient was a child, aged about 2, somewhat thin and delicate-looking, who eight weeks previously had begun to develop a large number of small hypodermic nodules scattered extensively over the body and numbering, perhaps, fifty. The earliest nodules were the size of a small pea, deep-seated, subcutaneous, and the skin over them was not in any way altered. Later, the nodules came nearer the surface, and the skin was reddened over them almost as if suppuration were about to take place: one such swelling was incised, but no pus was evacuated. The tumour bled freely. The mother gave a history of the disappearance of some nodules from time to time, and, contrary to expectation, these observations were verified by the exhibitor, who found that some nodules had undoubtedly completely

[1] *Proceedings*, 1908, i, p. 133.

disappeared, notably the one incised, which had been one of the largest, and a fortnight after incision had completely vanished. The nodules were most numerous on the flanks of the body, on the back, the shoulders, the upper areas and forearms, the thighs, legs and feet. The case had been now admitted to the Children's Hospital, Shadwell, and further investigations would be made.

DISCUSSION.

Mr. MCDONAGH offered the suggestion that the tumours were subcutaneous rheumatic nodules, the fibrous nodules often met with in children. The mother said they disappeared spontaneously, while others appeared equally quickly.

Mr. DAWSON said one of the lesions looked like erythema iris, and he would not be surprised to learn that it was erythema multiforme. He understood the mother to say that some went away and others came up in a day.

Dr. PRINGLE said some of the Fellows, with whom he agreed, thought it was a tuberculous condition, and that it corresponded to what had been called by Boeck the "sarcoid" group.

Dr. COLCOTT FOX said he was familiar with the so-called tuberculous gummata in children, and Drs. MacLeod and Ormsby had made a section of one of his cases and found the tubercle bacillus in the walls of the tubercular structure. He was doubtful if Dr. Little's case belonged to this category. As to the name "sarcoid," he did not think it helped them much.

The PRESIDENT (Sir Malcolm Morris, K.C.V.O.) expressed his disagreement with the view of Mr. McDonagh that they were rheumatic nodules; the appearance of the latter was different, and sometimes the skin was movable over them.

Culture of Fungus from Hairs affected with "Piedra."

By J. M. H. MACLEOD, M.D.

THE fungus was obtained from the specimens from British Guiana, demonstrated at the meeting of the Dermatological Section of the Royal Society of Medicine in July, 1911.[1]

Pieces of hair in which a nodule was present were soaked in absolute alcohol for five minutes, and planted on proof agar and

[1] *Proceedings*, 1911, iv, p. 149, and *Brit. Journ. Derm.*, 1911, xxiii, p. 255.

incubated at room temperature about ten days afterwards. The culture appeared as a small white knob about the size of a pin's head, with no marked duvet but with a slightly woolly surface. A week later it had reached the size of a split-pea, and had become irregular in outline. In the centre of the culture the colour had changed to a greenish-grey tint. As the culture grew the greenish tinge spread to the edge of it, and the outline became somewhat crenated. Finally the culture tended to assume a brownish tinge, a little lighter than cooking chocolate.

In broth a flocculent white culture was obtained. This, when subcultivated on proof agar, gave a growth similar to that described above.

A detailed account and photograph of the culture will be published later in the *British Journal of Dermatology.*

Dr. PERNET said that Dr. MacLeod's culture somewhat resembled the culture he (Dr. Pernet) had shown in 1900 to the old Dermatological Society in London.[1] But in Dr. Pernet's case the culture was a plate one (proof agar). As to the photograph of Dr. MacLeod's case, it agreed with Dr. Pernet's microscopical description.[2]

Case of Syphilitic Elephantiasis of the Scrotum (Lymphangitis).

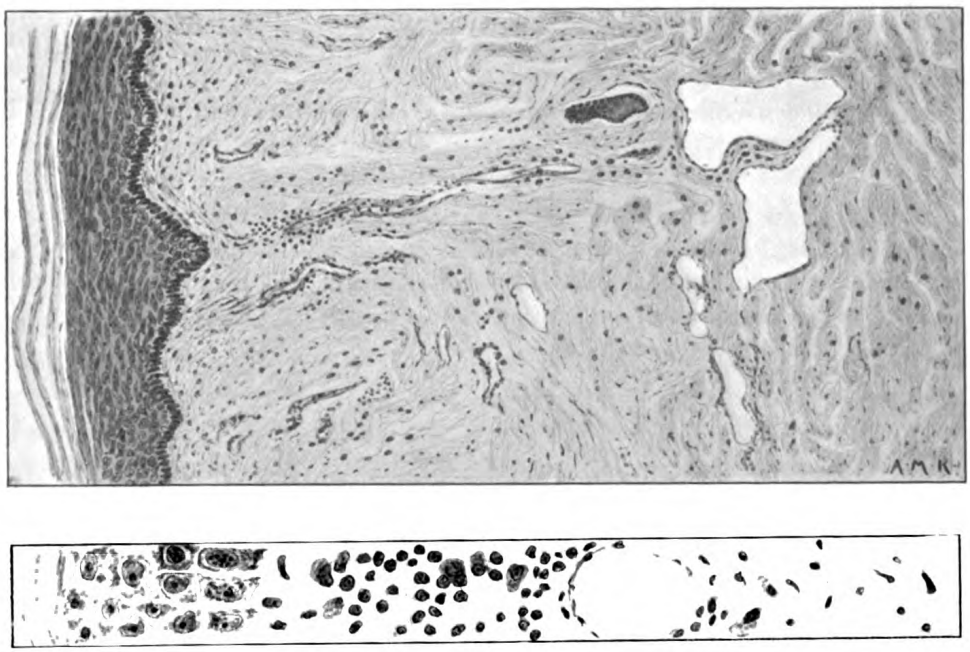
By J. E. R. McDONAGH, F.R.C.S.

C. H., AGED 46, contracted syphilis in 1882, for which he was treated irregularly with mercury internally. The first recurrence was a papular syphilide on the right palm in 1890, and two small papulo-pustular lesions on the same wrist which had left scars. The next recurrence was a serpiginous syphilide on the left half of the scrotum and another on the gluteal region on the same side in 1900. The patient stated that the scrotal lesion resembled the one on the palm in every way.

A year later the scrotum, first on the affected side, and in time the whole, began to enlarge, but the left side always remained bigger and

[1] Pernet, *Brit. Journ. Derm.*, 1901, xiii, p. 11.
[2] Pernet, *ibid.*, 1900, xii, p. 141.

harder. On examination, in 1911, the syphilides had disappeared, the scrotum was 28½ in. round, eczematous on the surface. The swelling appeared to be in the skin, which was hard, barely œdematous, and retained its rugose appearance. The testicles could not be felt; and although the skin of the penis was thickened, the organ itself was hidden by the swelling of the scrotum. The folds and inner sides of the thighs were eczematous. The patient had also chronic superficial glossitis and gave a positive Wassermann reaction. As a result of



Drawings (upper × 108, lower × 405) of microscopical section of another case of syphilitic elephantiasis—one of the leg, removed by amputation.

thirteen intramuscular injections of grey oil the circumference of the scrotum measured 13½ in., the skin became softer, the penis did not swell, and the testicles, which appeared normal, could be felt underneath.

Syphilis as a direct cause of elephantiasis has very seldom been described, although we are perfectly familiar with the condition following gummata and deep-seated mischief as periostitis, in the former being probably due to secondary infection and in the latter to mechanical obstruction.

Syphiloma hypertrophicum diffusum, described by Mračeк and Lang, a primary specific elephantiasis, which they considered was caused by the fusion of multiple subcutaneous gummata, does undoubtedly occur, but although the swelling may be diffuse the subcutaneous nodules are quite distinct. In the case exhibited the swelling was diffuse; no subcutaneous gummata were palpable and no gummata had appeared elsewhere. The swelling started after the appearance of the serpiginous syphilide, which showed that there were specific organisms present in the skin. The syphilide did not ulcerate, therefore a coccal infection could play no part, and the eczema first occurred when the swelling was at its maximum. The exhibitor considered that the swelling was due to a diffuse syphilitic infiltration of the cutis and subcutis arranged chiefly around the lymphatics—a syphilitic lymphangitis. The manner in which it has improved under treatment supports this view, as the other described conditions proved very refractory.

Histological examination: The accompanying drawings explain the condition. The epidermis was practically unchanged except for hyperkeratosis. In the corium were numerous newly-formed connective tissue cells, and in the deeper layers there were wide lymphatic spaces lined with one layer of endothelium; also a great increase of connective fibrous tissue. Around the lymphatics are small round cells—lymphocytes and a few plasma cells. The arteries and the veins remained practically unchanged.

DISCUSSION.

Dr. PERNET pointed out that the results in this case showed the great value of grey oil injections, which should not, therefore, be lightly discarded.

Dr. COLCOTT FOX had found that after a certain effect had been produced by the mercury the improvement stopped, which was only natural, as one could not expect fibroid induration to yield to that treatment.

Dr. ADAMSON regarded the case as of interest in that it showed that elephantiasis might occur in syphilis apart from streptococcal infection. It had been suggested that in many cases of syphilitic elephantiasis, especially those of the lower limb associated with ulceration, secondary streptococcal infection might be the cause of the elephantiasis rather than syphilitic lymphangitis, but in this case streptococcal elephantiasis seemed to be excluded.

Dr. PRINGLE said his experience of three cases of syphilitic lymphangitis of the lower limbs confirmed what Dr. Colcott Fox had just said regarding the

limitations of treatment. He thought Mr. McDonagh was much to be congratulated on the result so far obtained from the use of grey oil, even although the case was one of short standing only. He asked if any member could bring forward microscopical evidence of the implication of the lymphatics in these cases. Some years ago he looked up the literature, and although people talked glibly enough of "syphilitic lymphangitis," no one, as far as he had ascertained, had established the microscopical characters of the intimate pathological changes.

Mr. McDONAGH, in reply, said he had looked up the literature in connexion with the case, and found a record of only one which at all resembled it. Other cases of syphilitic lymphangitis had been described, but they were all secondary to gummata which had ulcerated, and were due to streptococcal or staphylococcal infection, and they improved but little on administration of mercury, owing to the amount of fibrous tissue formed. Mraček's cases were probably diffuse subcutaneous gummata, and they, again, did not disappear under mercury.

Case of Nævus.

By Sir MALCOLM MORRIS, K.C.V.O., F.R.C.S.Ed.,
and S. E. DORE, M.D.

N. L., AGED 8. Family history: Father and mother in good health. Mother had right breast removed two years ago for cancer. Six healthy children. Personal history: General health good. No illness with exception of measles.

History of present condition: Five weeks after birth a small, slightly raised mark, "like perforated cardboard," about the size of a threepenny-piece, was noticed above and a little to the inner side of the right nipple. The mother's attention was first called to the condition after having rubbed the child's chest with camphorated oil, but the oil was only applied once. Iodine and some caustic lotion had been painted on without effect, and the patch had gradually increased in size until the present time.

Present condition: The lesion consisted of a number of small, irregular, smooth, slightly shiny patches of the same colour or a little darker than the normal skin, grouped in a linear or herpetiform manner over the second, third and fourth ribs and intercostal spaces from the middle line of the sternum to the anterior axillary fold. The patches were formed by the aggregation of small, hemispherical or somewhat

flattened skin-coloured follicular papules, each papule having a minute dotted depression in the centre. In some parts, as at the margin of the areola of the nipple, a few single papules could be seen. There were no subjective symptoms.

DISCUSSION.

The PRESIDENT added that the question was whether it was a new growth or an anomalous form of congenital nævoid growth which had spread in that peculiar way. He invited suggestions with the view to checking its spread. The classical case of nævus published by Sir Jonathan Hutchinson was more warty and streaky than this. He would endeavour to get a biopsy. By daylight the older parts were seen to have a yellowish tinge. He had thought of the possibility that it might be congenital xanthoma.

Dr. ADAMSON regarded it as a unilateral linear nævus made up probably of sebaceous glands. He had recently exhibited cases of pigmentary linear nævus and a case of vascular and warty linear nævus, and had now under his care at the hospital a case of hairy linear nævus made up of pilo-sebaceous follicles. A linear nævus was generally a warty growth, but it might be derived from any of the structures or appendages of the epidermis.

Dr. MACLEOD said that he considered the affection to be a form of unilateral nævus. It appeared to develop around the follicles and was possibly of sebaceous gland origin, but it might be composed of cells similar to ordinary soft nævi, and he was unable to give any definite opinion with regard to its minute structure.

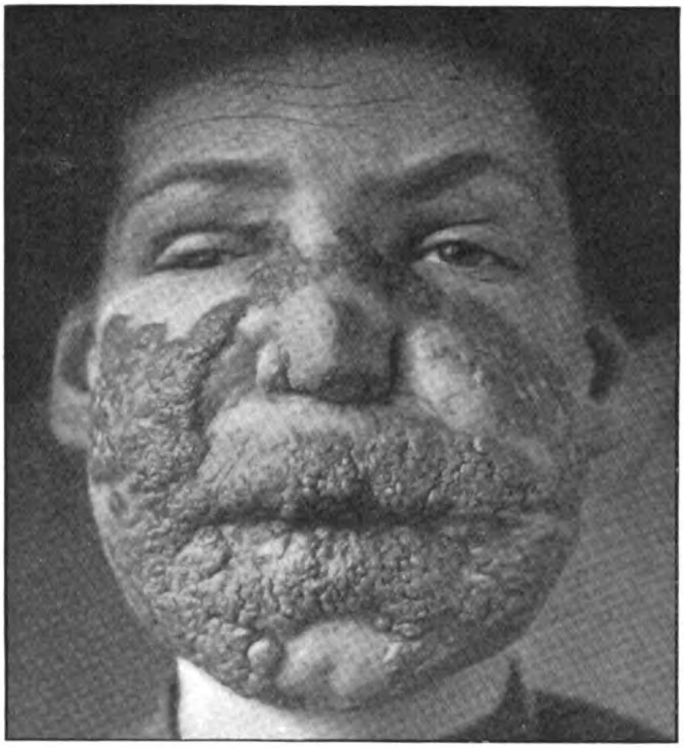
Mr. MCDONAGH regarded it as a nævus, probably of the sebaceous gland type.

Lupus Vulgaris in a Syphilitic Subject.

By J. H. SEQUEIRA, M.D.

THE patient, a married woman, aged 36, has been under Dr. Sequeira's care for nearly three years. She has a brother who has been in the Victoria Park Hospital suffering from phthisis. She has had two miscarriages and has one child alive and in good health. Seven years ago she noticed that her voice became husky, and that she had some pain in the throat on swallowing. There was also some discharge from the nasal cavity and this was scraped. Four

years ago a "pimple" appeared on the left side of the nose a little below the inner canthus, and a large number of nodules developed with great rapidity over the cheeks, nose, and the upper and lower lips. The present condition is well shown in the accompanying photograph. The nodular masses were soft and easily broke down, discharging a thin yellowish pus. The mucous membranes of the lips were involved, but there was no extension on to the gums. There was a small ulcer with



Lupus vulgaris in a syphilitic subject.

granular surface in the middle line of the palate, and there was well-marked lupoid infiltration of the larynx. It is difficult to see how much of the larynx was involved, as the pain on opening the mouth prevented a thorough examination.

The patient was in the London Hospital in 1908, and the rapidity of the spread of the disease with the history of miscarriages suggested a syphilitic origin. She was treated with mercurial inunction and improved considerably; the ulcerated surfaces healed, but the nodular

lesions persisted. Atoxyl was then tried and some further amelioration was observed. She then ceased to attend for some months, and on her return to the hospital the areas were more extensive and the ulceration had again appeared. Treatment by mercury and iodides proved of little service, and the patient steadily got worse. In February, 1911, she was again admitted to the hospital. She had then a strongly positive Wassermann reaction (+ + + +). She received two injections of salvarsan (0·5 intravenously and 0·5 intramuscularly). There was again definite improvement, but the nodular lesions did not clear up. She gave a positive von Pirquet reaction and the diagnosis of tubercle and syphilis was evident. In the spring of this year nodules appeared upon the front of the right forearm, and one of these was excised and sent to Dr. Stanley Griffith at Cambridge. He has since reported that he was able to obtain the tubercle bacillus of the bovine type from the material sent.

The case is of interest in the rapid development of the lupus and its tendency to rapid disintegration, and the fact that the presence of both syphilis and tubercle has been proved by clinical tests and by inoculation.

Dr. PERNET said that in his experience this class of case did not do well on specific treatment as far as the lupus vulgaris lesions themselves were concerned; indeed, it seemed to aggravate them.

Microscopical Section from a Case of Extensive Ringworm with Granulomatous Formations.

By J. H. SEQUEIRA, M.D.

THE case was exhibited at the last meeting.[1]

The section showed characteristic granulomatous tissue with a few giant cells. Strands of mycelium in parts showing bead-like segmentation, and conidia bodies stained by the Weigert-Gram stain, were present in the granulation tissue.

[1] See *Proceedings*, p. 33.

Culture of Achorion Quinckeanum (Mouse-favus) obtained from a Girl, aged 5 years.

By J. H. SEQUEIRA, M.D.

THE lesions were minute yellowish cups, six in number, grouped upon the outer surface of the right leg. Each cup was about $\frac{1}{10}$ in. in diameter, and showed characteristic felt work of mycelium. The cultures grew rapidly on the maltose agar medium, and presented a circular, white downy appearance closely resembling the cultures of the *Microsporon Audouinii*. The scalp was unaffected. The source of the infection was not traced.

Case of Syphilis.

By ARTHUR SHILLITOE, F.R.C.S.

THE patient was a man, aged 27, who exposed himself to infection one and half years ago, but did not appear to have contracted syphilis. Last April he married. Mr. Shillitoe saw him with some indurated œdema of the lower part of the scrotum on August 21. He could not detect any sore. The patient also had general erythematous eruption and a specific throat. Both these conditions cleared up, but on November 1 he suddenly developed a psoriasis-like eruption on the flexor surfaces of the arms, on the buttocks and behind the knees. That had now improved. The Wassermann reaction was positive. The wife is some months pregnant, but up to the present time shows no evidence of having been infected. Her blood has been examined by the Wassermann reaction, but at the time of the meeting the result was not known.

Dr. PERNET did not consider the case had any connexion with psoriasis. He regarded it as simply syphilis.

General Small Lichenoid Syphilide.

By ARTHUR SHILLITOE, F.R.C.S.

THE patient was a young man, whom he brought to show how long such small lichenoid eruptions would persist in spite of treatment. This man contracted syphilis early in May, and since the end of June he has been treated by injection of calomel, but nothing seemed to touch the eruption. It was very extensive indeed, and the Wassermann reaction was positive. There had not been any particular local treatment. The hair was now falling off. There was some improvement, but it was a very obstinate case.

DISCUSSION.

Dr. STOWERS suggested that calomel vapour baths would be a useful addition to the treatment of this case, he having seen good results follow this method in similar conditions.

Dr. PERNET said some cases of this kind were very resistant to treatment, even to "606," but he agreed with Dr. Stowers that calomel baths were often very helpful.

Case of Acne Agminata.

By A. WINKELRIED WILLIAMS, M.B.

AT first he diagnosed acne vulgaris, but as he watched the case the amount of scarring was disproportionately great, and the grouping of the lesions below the eyes, despite widespread comedones, made him wonder whether it was not acne agminata. There was no tuberculous history, and the patient had only had the condition about five months. The eruption of most of the lesions was simultaneous, and they similarly involuted, most of them without pustulation, leaving scars larger than the lesions.

Dr. PERNET did not consider that the case belonged to the category of acne agminata. He had seen the cases of acne agminata described by the late Dr. Radcliffe Crocker, and examined them histologically.[1]

[1] H. Radcliffe-Crocker, "Diseases of the Skin," 3rd ed., ii, p. 1097 (*see* fig. 70).

A Peculiar Nævus.

By A. Winkelried Williams, M.B.

The patient was a girl who was sent to him a year ago as a case of congenital keloid, but when he saw her the head was covered with eczema, and afterwards there appeared an extensive unpigmented nævus. When the child was born there was a lesion caused by pressure of forceps, and that might have led to the development of the nævus. It was not growing. The nævus was oblong, parallelogram-shaped, and extended from the middle of the forehead obliquely outwards over the scalp.

Announcement.

The President announced that at the meeting of the Council, which had just been held, it had been proposed that, in addition to the ordinary meetings of the Section on the usual days, there should be a special evening meeting on Thursday, May 16, when a discussion would be held on "Prurigo, Lichenification and Allied Conditions."

Dermatological Section.

January 18, 1912.

Sir MALCOLM MORRIS, K.C.V.O., President of the Section, in the Chair.

Case of Striæ Cutis Distensæ.

By E. G. GRAHAM LITTLE, M.D.

THE patient was a Haileybury boy, aged 16, who in the summer had received an injury to his eye in playing fives, in which he had nearly lost the eye. Strict abstention from exercise had been enforced on him, and he had been at home for several weeks. He had had "mumps," with orchitis, in August, 1911, and had old valvular disease of the heart. During this period of forced inaction he had become much stouter, but he had always been inclined to put on fat. The eruption was first noted on December 8, 1911, when bathing; he might have had it for a few days previously. There were no subjective symptoms associated with it.

When shown the patient had a large number of irregular linear raised red swellings on the outer and posterior surface of both buttocks and thighs. In some places the swelling and redness had somewhat subsided, leaving plane or depressed cicatricial tissue. But in the majority of cases vivid redness and swelling were present; the lines were vertical, zigzag in outline, about $\frac{1}{8}$ in. wide, not painful to touch, and varied from 1 in. to 3 in. in length. Except upon the thighs and buttocks the skin was healthy.

Striæ cutis was a rare affection seen in so early a stage. Several instances of the condition had been recorded as occurring after scarlatina, typhoid, and in only one case, as far as the exhibitor was aware, in sudden obesity. It was a question worth considering whether the toxins elaborated in the specific fevers (and in this instance in mumps

with metastatic orchitis) were instrumental in weakening or destroying the elastic tissue, an accident which, a priori, might be expected to produce the disease. Histologically, absence or destruction of elastic tissue had been reported in the site of the actual striæ.

DISCUSSION.

The PRESIDENT (Sir Malcolm Morris, K.C.V.O.) said he had seen several cases of the kind following typhoid fever; young patients often grew rapidly after that disease, and he reminded members that there were cases of the kind which were independent of infections, such as the striæ following pregnancy. As a rule the condition was not seen as early as this. Probably all members had seen young girls who, at puberty, suddenly became fatter, especially in the thighs and breasts. The red marks passed away, and the lesions became white.

Dr. F. PARKES WEBER said that the patient declared that the development of the excessive "striæ atrophicæ" was preceded by a very severe attack of mumps. In some severe cases of mumps the pancreas was probably involved and the occurrence of nutritional changes was therefore quite likely. Such "striæ" were known sometimes to follow typhoid fever, scarlatina, and might develop after bronchopneumonia and exhausting conditions; they doubtless might likewise be an indirect result (granted a temporary special cutaneous tendency as a personal predisposing factor) of severe mumps. Perhaps the most striking examples of "striæ atrophicæ" were the rare cases in young children, who, owing to some disorder of their "internal secretions," developed a remarkable condition of "plethoric obesity."

Dr. ADAMSON did not believe that these linear striæ were the result of stretching of the skin. He thought they had the same origin as atrophic macules. In cases of macular atrophy, linear striæ were nearly always present as well. In macular atrophy it had been shown that the earliest stage was inflammatory, and that the final atrophy was due to absorption of the elastic tissue. He had often noticed that in linear atrophy, in the early stages, the striæ were red and raised, and that the thinning and whitening of the striæ came later. He had recently exhibited two or three cases of linear and macular atrophy in tuberculous and syphilitic patients, and he believed that the primary cause of these macules was some poison—tubercle, syphilis, or other—which attacked particularly the elastic tissue. Linear striæ, when occurring without macules, were generally associated with some illness—e.g., enteric fever—and it seemed possible that they might also be of toxic origin.

Case for Diagnosis.

By E. G. GRAHAM LITTLE, M.D.

THE patient was a little girl, aged 5. She had at the present time a number of slightly scaly, reddened patches, with the following distribution: Outer and upper part of left thigh, a patch 4 in. by 3 in.; upper and outer part of left buttock, a patch 4 in. by 6 in.; on the posterior surface of left buttock, a patch 1 in. by 1 in.; on the upper and outer part of the right thigh and buttock, a patch 6 in. by 3½ in.; on outer side of right thigh, just below the last mentioned, there was a patch 3 in. by 2½ in. Over the middle of the posterior surface of the right buttock there was a small patch, 1 in. by ¾ in.; on the right side of the abdomen, below the umbilical level, there was a small patch, oblong, 1¼ in. by 1 in.; on the front of the upper arms, about the middle of the prominence of the biceps muscle on both sides, there was a symmetrical patch about 1 in. by 1 in.

The patches had begun to appear about a year ago, and had persisted ever since. There was no disordered sensation in the affected areas. The edges of the patches were slightly more raised and were redder than the patch as a whole.

Scrapings from the surface and from the edge had been carefully examined for ringworm, with a negative result.

The mother had died of phthisis in the last few months; there was a very convincing narrative of a peculiar habit which she had of rubbing the child's limbs with her (the mother's) own saliva. The child was a thin, delicate little girl, but with no signs of definite constitutional disease.

The exhibitor had been inclined to group this case with the cases of circumscribed symmetrical dermatitis, called by Unna eczema seborrhoicum areatum; the redness, large size of the patches, and comparative freedom from scaling differentiated this case, however, from all others he had previously seen, and he accordingly preferred to exhibit it as "a case for diagnosis." No treatment had as yet been adopted for the disease.

The PRESIDENT said parts of the eruption disappeared from the centre of the patches and a fresh ring seemed to start in some of them.

Case for Diagnosis.

By J. M. H. MacLeod, M.D.

THE patient was a young man, aged 30, who had been suffering for the last five years with an eruption which had been diagnosed as syphilis, and for which he had been under almost continuous treatment. In 1907 he contracted a sore on the penis while in Brazil; this was followed about twelve days later by a number of scaly patches about the scrotum, a small boil on the inside of the thigh, and two months later by a scaly eruption chiefly localized on the abdomen. The glands were not definitely indurated, and there was no disturbance of the general health. The affection was diagnosed at that time as syphilis, and it was seen by several competent observers on his return to this country in 1908, and the diagnosis corroborated. When seen in 1911 by the exhibitor he had a widely distributed eruption on the trunk and limbs, consisting of reddish macules or slightly raised papules about the size of a split-pea to a threepenny-piece, which were covered with a delicate scale. These kept on developing, lasted a month or more, and disappeared without appreciable scarring, though here and there a slight change in the texture of the skin could be detected where they had been. In a few of the lesions the thickening was palpable, but the majority of them could not be felt, and there was no definite staining left on diascopy. The lesions were not confined to any special region, being no more numerous on the extensor than flexor aspects, and they were absent from the palms, soles, face and scalp. He had been treated with mercury up to 1910 by the mouth and by injection. This was discontinued by the exhibitor. A Wassermann reaction was done, with negative results, and a lesion from the arm was excised for microscopical examination. This showed a dilatation of the blood-vessels in the papillary and sub-papillary layers with an infiltration of small inflammatory cells, but no plasma cells; in addition were several groups of inflammatory cells more deeply seated in the corium; the overlying epidermis was in a state of parakeratosis. The appearance of the section did not suggest any form of granuloma, but rather a superficial inflammatory condition of the type of psoriasis or a seborrhoide.

The persistence of the eruption, in spite of mercurial treatment and the negative Wassermann reaction, seemed to put syphilis out of court, while the clinical appearances and the histological structure pointed rather to a psoriasiform seborrhoide or an anomalous psoriasis.

DISCUSSION.

Dr. WHITFIELD said he was inclined to regard it as a tuberculide.

Dr. ADAMSON said that although clinically and histologically the lesions in many ways suggested psoriasis, he was inclined to agree with Dr. Whitfield that the eruption was a form of tuberculide. There was a tendency to grouping in places which suggested lichen scrofulosorum, and the grouped papules appeared to be situated upon areas of slightly atrophic skin, reminding him of a case of lichen scrofulosorum with macular atrophy which he had exhibited on a previous occasion (October 20, 1910). That a tuberculide might simulate psoriasis was evident from the fact that the tuberculide exhibited recently by Dr. Bunch had been some years previously diagnosed by the late Dr. Crocker as an ulcerating psoriasis.

Dr. LIEVEN (Aix-la-Chapelle) said the case was certainly not one of syphilis: he regarded it as seborrhœa exfoliativa.

Dr. SEQUEIRA said the patient had had mercurial ointment rubbed in every other day for five weeks on one arm only, but it made no apparent difference.

Dr. STOWERS said that he appreciated the difficulty of diagnosis in the early stage of this case, but, as the result of Dr. MacLeod's recent investigations and the present condition of the patient, he did not regard the eruption as syphilitic. In his opinion it was probably seborrhœic in nature with psoriasiform characters superadded.

Case for Diagnosis.

By G. NORMAN MEACHEN, M.D.

THE patient was a married man, aged 56, an artist's model, who had had malaria in 1886 when in a regiment in Algeria, but who gave no history of venereal disease. Fourteen years ago, after drinking some hot tea, he first experienced a sensation of itching, and noticed that he "came out in irregular pink blotches upon the stomach." Two years after this he became ill with staggering, weakness and faintness, and in 1900 he consulted a physician at the Brompton Hospital, where he was given cod-liver oil and malt. At this time it was observed that the skin around the loins was becoming a "smoky, tawny yellow" colour, and he was informed by two physicians that he was suffering from Addison's disease. Another physician at a third hospital thought that he had neurasthenia.

In October, 1911, he fractured a couple of ribs on the left side, being treated at the West London Hospital, where a "yellow plaster" was applied over the injured area. This caused great irritation of the skin, and shortly afterwards a pink rash appeared upon the abdomen and arms, for which he was given medicine. In November last he consulted the exhibitor at the Blackfriars Skin Hospital, when the chief symptoms were the intense pruritus and the increase of pigmentation of the skin, most marked upon the back of the trunk.

About six weeks ago an irregularly mottled, slightly scaly erythematous eruption appeared on both surfaces of the limbs, the patches being ill-defined and resembling the rash met with in cases of para-psoriasis. The urine contained neither sugar nor albumin.

Further inquiries of the patient elicited the fact that he had taken "a good deal" of arsenic, but not recently. A microscopic section from one of the patches upon the forearm merely showed a fairly extensive small-cell proliferation around the blood-vessels and in the upper parts of the papillæ.

The case excited a considerable amount of interest, few members caring to commit themselves to a definite diagnosis. The biopsy did not, certainly, favour the view, at one time held by the exhibitor, that the condition was a pre-mycotic one.

DISCUSSION.

The PRESIDENT said it was very lichenoid in places. The lesion on the leg was the only one which was raised. He supposed that the question of drug eruptions had been eliminated. He asked whether any member had seen persistent belladonna eruptions, because the colour of those was much more like the colouring of the skin of the Red Indian. He had a case in a person who was poisoned by drinking belladonna in mistake for some other mixture. The danger symptoms all passed away, but she was left with a persistent purply-red eruption all over the body, with practically no itching; this lasted for two or three years.

Dr. MACLEOD said that the fact that the lesions were not palpable seemed to him to put the pre-mycotic condition out of court, nor did he think the case belonged to the "para-psoriasis" group. He regarded it as a persistent toxic erythematous condition more nearly related to urticaria.

Case for Diagnosis.

By J. H. SEQUEIRA, M.D.

THE patient, a married man, aged 58, noticed in August, 1911, a small tumour on the forehead above the right eye, and shortly afterwards a similar small swelling over the left eye. Three weeks later he found a flat swelling in the epigastrium. He had never had syphilis, and the only illness he could recall was an attack of bronchitis and "asthma" a few winters ago. He has four children, in good health, and there have been no miscarriages. There was no history of gout, rheumatism, or cancer in the family.

When the patient was shown at the meeting there were three tumours. One the size of a filbert-nut on the forehead, about an inch above and to the outer side of the right eyebrow. This tumour was movable over the subjacent tissues, of moderately firm consistence, rounded in outline, and the skin over it was of red tint. The patient was positive that the integument was of normal colour when the growth was first noticed. A similar but slightly smaller tumour was present above the left eyebrow at a little lower level. This had the same characters as regards mobility, consistence and colour. In the epigastrium there was a larger flat growth about 3 in. transversely and 2 in. vertically. This was less well defined than the other growths, its surface was of a dull red tint, the colour shading at the margins into the tint of the normal skin. This tumour was not very mobile, but this appeared to be due to the fact that the skin was somewhat stretched. There were no deep connexions.

There had been no antecedent eruption at any time, and there was no pruritus, and there had been none. The lymphatic glands were not enlarged. The spleen could not be felt. The Wassermann reaction was negative; the blood examination revealed no abnormality. There was no evidence of visceral disease.

The case was brought up to obtain the opinion of the members present, the exhibitor having in mind mycosis fungoides, tumour *d'emblée* type, and sarcomatosis. It may be mentioned here there has been no wasting since the appearance of the tumours in August and September last. Arsenic had been given, and the tumours have been exposed thrice to the X-rays, and the patient thought that there had been some diminution in their size, an opinion which the exhibitor was unable to share.

DISCUSSION.

The PRESIDENT said the case reminded him of one he had in the days gone by, in which for a long time the diagnosis was syphilis, but eventually that was altered to mycosis fungoides.

Dr. WHITFIELD did not incline to the view that it was mycosis fungoides; to him it looked more like primary sarcomatosis. Dr. Pringle and he had had a case of primary sarcomatosis of the skin together. This patient was unable to submit to X-ray treatment for six months after the diagnosis was made, and very great increase in the number of the tumours took place during this period. Dr. Whitfield afterwards treated him with X-rays, and he had now been absolutely without symptoms since June, 1910—i.e., over eighteen months. In this case the fact that no reaction was given by the tumours to X-rays was strongly against the diagnosis both of mycosis fungoides and sarcomatosis. He urged that a biopsy be made.

Dr. COLCOTT FOX said his view was that the lesions were most likely lipomata.

Dr. R. A. BOLAM (Newcastle) agreed with Dr. Colcott Fox's idea that they were lipomata. He called attention to the fact the lesion on the abdomen was lobulated, and part of its appearance might be due to treatment.

Cultures of Trichophyton Plicatile from a Case of Extensive Ringworm of the Trunk and Extremities, with Granulomatous Formations.

By J. H. SEQUEIRA, M.D.

THE cultures belonged to the group called "neocrateriforme," and were made by Dr. Sabouraud from Dr. Sequeira's case shown at a previous meeting.[1] The cultures resemble closely those of the crateriform endothrix fungus. The parasite appears to be relatively common in Copenhagen, for Bang has described a series of twenty-eight cases, while Dr. Sabouraud has only seen the fungus in two other instances.

[1] *Proceedings*, p. 33

Extensive Nævus affecting chiefly the Left Side of the Body and partially the Right Side of the Neck in a Girl, aged 14.

By J. H. SEQUEIRA, M.D.

THE nævus was of very similar character to that shown by Sir Malcolm Morris at the last meeting, the lesions consisting of closely set pits apparently of the sebaceous glands, many of which contain black filaments and plugs. The close resemblance of the condition to perforated cardboard noticed by Sir Malcolm in his case was present in this patient. In a few areas the plugs in the glands were horny and projecting above the surface. The areas affected were the neck on both sides, the left side of the trunk, and the left upper extremity. The areas were somewhat band-like and encircled the trunk, ending sharply at the middle line before and behind.

The case was shown by the courtesy of Mr. Bruce Roxburgh, Ophthalmic Surgeon to the London Hospital, the patient being under his care for cataract, upon which an operation had been performed.

DISCUSSION.

Dr. HALDIN DAVIS said that he had had a similar case in a little girl, in whom, however, the extent of the nævus was limited to a narrow area on the neck. He had shown the case to the Section, and a photograph was published.[1] Microscopic section showed that the follicles were really mere crypts caused by dimples in the skin, and they were filled with horny material.

Dr. JAMES GALLOWAY said the President and others would remember the remarkable case brought from Holland and shown at the International Congress of Dermatology in London by the late Dr. Selhorst, of Amsterdam. Dr. Selhorst's case was that of a young woman, aged about 24, in whom an acneiform nævus involved the left side of the chest, including the left breast, the left arm, and the left side of the neck. In this patient's case the condition was of congenital origin, and the same honeycombed condition of the skin as in Dr. Sequeira's little patient was seen in a greatly exaggerated form. The honeycombed aspect seemed to be produced by the presence of crypts or convolutions of the skin filled with sebaceous material and badly formed desquamating epithelium. Dr. Galloway had no doubt that Dr. Sequeira's

[1] *Proceedings*, 1910, iii, pp. 68, 69.

case was of the same nature as the one referred to. A short account of the case by Dr. Selhorst and photographs were published in the *British Journal of Dermatology*, 1896, vol. viii, p. 419. Dr. Galloway thought that the disfigurement produced by the eruption could be a good deal improved by the careful use of solid carbon dioxide.

Dr. WHITFIELD asked whether an examination had been made to ascertain whether the plugs contained the Sabouraud bacillus. If not it would be interesting for that to be done. As regards the treatment, he thought that carbonic acid snow would remove the present mole. He had never found a mole which penetrated beyond the corium. In hairy moles one could even remove the hairs with CO_2 snow, and the hair-roots lay deeper than the mole structure.

The PRESIDENT said it was markedly spinous, and was unilateral. It would be interesting to know if the mother could give a history of what the condition was as a baby. A pyogenic infection might cure it. He did not think Dr. Selhorst's case was tuberculous; the conclusion seemed to be that it was nævus; in that case there was much ulceration and œdema. Members would remember the case of linear nævus which was under his (the President's) care for a long time. The condition there was over the whole of one half of the body, with streaks down the leg and arm. They were more like warty growths, and there were no depressions. Many of the warty lesions were filiform, long, thin, tapering points.

Dr. STOWERS referred to a case exhibited by him in 1907, with characters corresponding in some important respects to the present patient, and bearing upon the remarks of the President. In his case (which was fully described, with illustrations, in the *British Journal of Dermatology*, 1908, vol. xx) a girl, aged 8 years and 4 months, had an unusual development of this disease. The lesions developed from birth to the age of 6 years, and were characterized by marked symmetry upon the lower extremities and trunk before they appeared upon the forearms and hands. The constituent elements of the eruption were papular, with or without scales, and increasing in size became raised, exuberant, indurated outgrowths culminating in large warty developments and spiny prominences. Of these some were single and discrete, while others were compressed into groups, parallel streaks, or ribbon-like bands. They were especially marked upon the upper arms and the contiguous surfaces of the index and second fingers of the left hand. These bands corresponded to the long axis of the limb affected, but their direction upon the trunk was transverse to the axis of the body.

Sequel to Case of Chronic Artificial Skin Eruption.[1]

By F. PARKES WEBER, M.D.

THE patient was a woman, aged about 37, with a long past history of functional nervous affections as well as of malingering. For the last seven and a half months she had had a chronic bullous or eczema-like eruption, with superficial ulceration, on the front of the abdomen. This was apparently due to local irritation of some kind, but the exact cause could not be ascertained. In spite of treatment the eruption continued on and off till December, 1911, when she was again an in-patient at the German Hospital. At that time the eruption (which had persisted for over nineteen months) was definitely bullous, and involved the whole of the front of the abdomen. The skin was, as a consequence, much discoloured, and reminded one of the appearance of the hands in a severe case of epidermolysis bullosa. In short, the eruption, considering its localization, corresponded to no known natural disease; and the past history of the patient made it almost certain that it was artificial, and produced in some way or other by local irritation on the part of the patient herself. A starched bandage was applied to the abdomen in order as far as possible to keep the patient's hands away from the affected skin. The bandage, however, became loosened, and probably (by deflating her abdomen) the patient was able to introduce her hand under the upper anterior portion. At any rate, when the bandage was removed, bullæ were found to have developed over the corresponding portion of the abdomen, and the house physician, Dr. G. Dorner, observed that there were dark specks in the raised epidermis over some of these bullæ. By microscopical examination he found that the dark specks contained minute glistening greenish particles, which he proved to be powdered cantharides, by comparing them under the microscope with the actual powder obtained by crushing up a dried specimen of the blistering beetle, *Cantharis vesicatoria*. The patient, however, in spite of this evidence, would not acknowledge any deception on her part. In some respects the case might be compared to one recently described by Dr. G. Hirsch at a meeting of the Gynæcological Society of Munich (December 15, 1911). Hirsch's was that of a woman who

[1] Shown at the meeting of December 15, 1910 (see *Proceedings*, 1911, iv, p. 43).

within two years had had her vermiform appendix removed, and had had three other abdominal operations performed. On account of menorrhagia she was treated by X-rays and developed ulcers, which gradually spread over her whole abdomen; but these were spurious "X-ray ulcers," and were discovered to be self-inflicted by means of hydrochloric acid.

DISCUSSION.

Dr. GRAHAM LITTLE called the attention of members to a case of artificial dermatitis in a young girl shown by him a few months ago, the sequel of which had been interesting. This girl had been accustomed for many months to produce the skin lesions, probably at night, upon her arms and hands; she had recently been under the care of a Christian scientist, who in the mother's words, by prayer and the "laying on of hands" had completely cured the disease, so that she no longer had the eruption. The child had been recently seen, and the fact of complete freedom from eruption was corroborated—the patient was much more composed, and the anæsthesia of the pharynx and palate, which had been extreme, was much less. The influence of religious suggestion on an impressionable and nervous child no doubt explained this improved state of affairs.

Dr. LIEVEN (Aix-la-Chapelle) said that when the Insurance Act was in full working order in this country there would be many such cases. In Aix one grew accustomed to them in the persons of workers in chemical factories. Extraordinary skill was shown in producing lesions of the hands.

Dermatological Section.

February 15, 1912.

Sir MALCOLM MORRIS, K.C.V.O., President of the Section, in the Chair.

Case of Extraordinary Pigmentation of the Face.

By HALDIN DAVIS, F.R.C.S.

THE patient, J. P., an unmarried female, aged 27, occupied in housework, had been the subject since the age of 16 of a most curious blue-black pigmentation of the face. This colour extended over the whole of the face and the exposed part of the neck, but was not to be observed elsewhere except at the bases of the nails of the fingers; the nails of the toes were free. The colour was most marked over the nose, it was quite apparent in the sclerotics, the cheeks and forehead were obviously affected, the ears perhaps to a less degree. The colour could not be dispersed by pressure. The family history of the patient was uninteresting; her mother died at the age of 49 from some cause which could not be ascertained; her father, brother, and four sisters were alive and well. None of them suffered from a similar pigmentation. Inquiry into the history of the patient herself only elicited the fact that she had had measles when a child. She had not had scarlet fever, pneumonia, bronchitis, rheumatism, or chorea. There was not any history of the application of carbolic acid preparations at any time. For the last eleven years the colour of her face had not altered. When she was admitted into Victoria Park Hospital she had suffered from pain after a meal for three months. She was also subject to dyspnœa and sweating on exertion, occasional palpitation and giddiness. At intervals she suffered from frontal headache. Her general nutrition was good. Nothing abnormal was discovered in her lungs. As regards her heart, the apex-beat was in the fifth intercostal space in the nipple line, and there was a slight systolic bruit in the mitral area; otherwise the

heart sounds were normal. There was no clubbing of the fingers. As regards the digestive system, the tongue was furred, the teeth were bad. There was no enlargement of the liver or spleen.

The urine was straw-coloured, acid in reaction; it contained no abnormal constituent; the specific gravity was 1007. There was no sign of alkaptonuria. The blood count showed red cells 5,280,000, white cells 6,000, percentage of hæmoglobin 98, index 0·9. The differential count showed polymorphonuclear leucocytes 76 per cent., lymphocytes 20 per cent., transitional cells 2 per cent., eosinophile cells 2 per cent. Spectroscopic examination of the hæmolysed blood showed the two oxyhæmoglobin absorption bands. On the addition of ammonium sulphide the spectrum of reduced hæmoglobin was at once obtained, showing that neither methæmoglobinæmia nor sulphæmoglobinæmia was present.

The diagnosis lay between argyria, ochronosus, methæmoglobinæmia, and hæmochromatosis. Against each of these possibilities strong arguments might be advanced. The diagnosis of argyria was negatived by the fact that careful inquiry failed to reveal any possibility of the ingestion of any silver compound. As regards ochronosus, the general appearance was very suggestive of that condition, and before the examination of the urine the exhibitor had considered it such, but the fact that the urine was normal and that there was no history of carbolic acid being used at any time put it practically out of court. The spectroscopic examination of the blood negatived the diagnosis of methæmoglobinæmia. The remaining possibility was hæmochromatosis, although the absence of enlargement of the liver and spleen and the normal character of the urine, together with the very local distribution of the pigmentation, were against this diagnosis also. The existence of digestive disturbances gave it, however, some support, and, in the absence of any other more probable alternative, was the most plausible provisional diagnosis.

DISCUSSION.

Mr. McDONAGH remarked that he did not believe it was a case of ochronosus—first, because no other member of the family was affected; secondly, because there was no reducing agent in the urine; and thirdly, because the pigmentation was general and did not pick out the cartilages, which are usually affected alone in ochronosus. He believed it to be either a case of hæmochromatosis or methæmoglobinæmia, the latter diagnosis having already been suggested by a member present.

Dr. MACCORMAC said it looked like a case of methæmoglobinæmia, or sulphæmoglobinæmia. It was very much like a case shown before another Section by Dr. Essex Wynter.[1]

Dr. GORDON R. WARD remarked that much depended upon the amount of ammonium sulphide used in doing the test. In one case too much was added, and the mistake was only rectified some time afterwards. A better plan was to pass carbon monoxide through the blood and then examine spectroscopically. This would decide whether methæmoglobin or sulphæmoglobin was present. He had no doubt that one or the other was. The patient's admission that she had been taking "headache powder" suggested the former. He did not believe it was pigmentation in the nails, as the colour disappeared on pressure, which pointed to the colour being in the blood.

Small Linear Nævus on the Palm of the Hand of a Girl, aged 5.

By S. E. DORE, M.D.

THE nævus extended from the top of the hypothenar eminence nearly to the tip of the ring-finger of the right hand, and consisted of small, hard, raised warty growths. The mother attributed the condition to the fact that she had rubbed some "eggy" water on the side of her body when she was carrying the child, and also that she had had an urgent desire for a bunch of grapes at this particular time.

Two Cases of Alopecia Universalis.

By E. G. GRAHAM LITTLE, M.D.

THE first was that of a young woman, aged 23, who had been confined two years previously, and began to develop, shortly after the confinement, some patches of alopecia areata upon the scalp. The confinement had not been prolonged or in any way otherwise remarkable. The loss of hair steadily increased until the present stage was reached, in which she has no hair at all on any part of the body. The scalp, eyebrows, eyelashes, axillary and pubic, as well as the downy hair all over the body, was completely absent. She appeared to be otherwise in very fair health.

The second was that of a mechanical engineer, aged 30, who was sent to the exhibitor by Dr. Bertram Thornton, of Margate. The

[1] *Proceedings*, 1908, i (Clin. Sect.), p. 48.

patient began to notice progressive baldness at the top of the scalp six years ago. He had not had any preceding illness to account for the loss of hair, which became gradually universal, the downy hair disappearing as well as the pigmented hair. With the exception of a very few scattered bristled hairs about the base of the penis he had no hair at all upon his body. His general health appeared excellent.

Universal alopecia, in the exhibitor's experience, was a rare affection. He would place the incidence at about one in two thousand cases of general skin diseases—a figure which approximates very closely to the statistics given by Radcliffe Crocker.

DISCUSSION.

The PRESIDENT (Sir Malcolm Morris) thought such cases were more frequent than was generally supposed; he knew a number of people in various parts of the country who were in that condition, and had remained so for years. He did not believe it affected women more than men. He knew of several cases in people in prominent life. The prognosis he considered very doubtful.

Dr. SEQUEIRA said he had several people attending his clinic at the present time with complete alopecia, and he was interested in Dr. Little's remarks as to it following pregnancy and delivery. He had a patient who, after her first confinement, lost the hair of her scalp and some from the eyebrows, but the hair grew again. She became pregnant a second time, and again lost her hair, and again after the third pregnancy, but this time it was not replaced. He believed the condition to be more common in women than in men.

Dr. PERNET agreed that these cases should not be utterly given up, not even after six years. He remembered a bad case which he injected with pilocarpine locally in the scalp, but it did no good.

Case for Diagnosis.

By E. G. GRAHAM LITTLE, M.D.

THE patient was a woman, aged 40. She had been subject for the past three and a half years to recurrent eruption similar to the present outbreak. Up to last Christmas she had been free for some months; she then began to develop the present condition. She had wholly upon the extensor surfaces of the forearms and hands a large number of circinate lesions varying in size from a threepenny-bit to an area of about 6 in. by 4 in. The smaller lesions were very like erythema iris,

but persisted considerably longer than those in that disease. The lesion spread by peripheral extension, the edge being always vividly red, raised and œdematous even in the largest areas, where the central parts showed little or no change except a bluish-red pigmentation; the edges were still vividly red and raised. Upon the backs of all the fingers, along their whole length, the skin was scaly and red, and very much resembles lupus erythematosus, but there is the same œdematous edge separating the diseased from the healthy skin. In addition to the arms, where the disease was most active, there were similar but smaller erythematous vesicular lesions upon the nose and cheeks, and upon the front aspect of both legs. The patient had complained from time to time of obscure arthritic pains. She had no albumin or sugar in the urine.

DISCUSSION.

Dr. COLCOTT FOX suggested that the eruption belonged to the type described by Unna as eczema seborrhœicum. He noted that the patient had chronic pityriasis of the scalp.

The PRESIDENT said he would lay stress on the lesions on the right arm; it had not reached the stage of blistering.

Dr. SEQUERIA agreed with Dr. Pringle's idea that it belonged to the group classed as toxic erythema. It recalled to mind one which he showed before the Society in which there was characteristic lupus erythematosus, and also lesions which were characteristic of erythema iris.

Dr. PERNET said he had no doubt the case was one of lupus erythematosus and of the type described by French observers as lupus erythematosus iris.

Dr. ADAMSON said that he regarded Dr. Little's case as a characteristic erythema multiforme with all its features exaggerated. The distribution, the red, raised expanding rings—some like "herpes iris"—the associated joint pains and the recurrent attacks, were all typical. Dr. Liveing, and later Dr. Sequeira and Dr. MacLeod, had called attention to the resemblances between erythema multiforme and lupus erythematosus. He thought that such cases as the one now exhibited were on the borderland between erythema multiforme and extensive lupus erythematosus, or, rather, that extensive lupus erythematosus often closely simulated erythema multiforme, although there were usually somewhere more typical lesions of lupus erythematosus and evidence of atrophic scarring—often on the scalp. None of the lesions in the present case suggested those of lupus erythematosus, none of them showed atrophic scarring, and there was no scarring on the scalp. He thought this case was certainly a true erythema multiforme and not an extensive lupus erythematosus.

Leucodermia Syphilitica with Central Atrophy.

By J. E. R. McDonagh, F.R.C.S.

Q. P., a woman, aged 24, contracted syphilis, Easter, 1911. A rash appeared all over her body, some of which has persisted, and since infection she has been troubled with sore throats and bad headaches. The patient attended a London skin hospital, and was given an ointment. Most of the rash has now disappeared, except for some papules scattered about the trunk.

The leucodermia colli was well marked, and consisted of both the macular and rosette forms; in the centre of the leucodermic areas were to be found papules; some of the papules had disappeared, leaving areas of hyperpigmentation, while others had been followed by atrophy.

The atrophic areas were depressed, the skin over was thinned and rolled like cigarette-paper, and the vessels underneath were plainly visible; there was also some hyperpigmentation.

This case was of interest, showing that the hyperpigmented spots in the depigmented areas were due to the syphilitic papules, and that true atrophy could follow such papules.

An Unusual Case of Cicatrizing Folliculitis of the Scalp in a Woman, aged 55.

By Sir Malcolm Morris, K.C.V.O., F.R.C.S.Ed.,
and S. E. Dore, M.D.

The patient presented two bald patches of two years' duration in the lower occipital region. On the left side, just above the nape of the neck, there was a circular definitely circumscribed and deeply infiltrated area about the size of a florin. The surface of the patch was roughened and slightly brownish in colour, somewhat resembling a mole. The hairs, which were buried in the thickened mass, and slightly projected above its surface, were easily extracted, and were seen to be distorted in shape with atrophied roots and brush-like extremities. The root-sheaths were not swollen, as in some cases of pseudo-pelade, there was

no perifollicular erythema, or pustulation, suggestive of folliculitis decalvans (Quinquaud's disease), nor were there the masses of granulomatous vegetations, or puckered scar tissue with projecting tufts of hair, characteristic of the so-called acne cheloid. The patch on the right side apparently presented a further stage in the development of the process. It was situated rather higher up on the scalp, behind and a little above the ear, and was more elongated than that on the left side. Its surface was atrophic, smooth, slightly depressed, and devoid of hair, but at the margin there was a definite perifollicular infiltration, and the hairs had a similar appearance to those of the opposite side. Both patches gave rise to considerable irritation.

Although the case was unlike acne cheloid in many particulars, the presence of the perifollicular infiltration seemed, in the view of the exhibitors, to exclude an ordinary case of folliculitis decalvans and to bring it within the category of the former disease.

DISCUSSION.

Dr. DORE said he had stained some of the hairs from the case, and they were similar to those of alopecia areata, having atrophied roots and pointed or brush-like extremities. Staining showing the presence of cocci and microbacilli, and a hair planted on proof agar gave an almost pure culture of the bottle bacillus.

Dr. COLCOTT FOX suggested that the diagnosis of lupus erythematosus should be considered in the case, in the presence of persistent inflammation in one patch, leaving atrophy. Rare cases of that kind did occur, localized to the scalp. The patient admitted that she had poor circulation.

Dr. PERNET was in favour of lupus erythematosus. Moreover, the patient's poor circulation and the symmetry of the lesions supported that view.

Dr. DYSON asked whether the President had seen the patch on the right side in its original state before atrophy had taken place. Its present appearance was to him very suggestive of a patch of lupus erythematosus.

The PRESIDENT replied that he only saw the patient a few days ago. There had been no treatment, but she had rubbed the area a good deal. There was no lesion in any other part of the body. It might be a very unusual case of sclerodermia; it was, in his opinion, too hypertrophied for lupus erythematosus, and there were no bright red points around or in the scar. He thought the case was allied to the acne cheloid group.

Alopecia Universalis with Dystrophy of the Nails.

By J. H. SEQUEIRA, M.D.

The patient was a sturdy little boy, aged 4. The mother stated that the child was born with the usual amount of hair, but that when he was six weeks old the hair of the scalp, eyebrows, and the eyelashes came out, and since then the boy had been completely bald. Both parents were in good health and there was no history or evidence of syphilis. There were two other children; one has had rheumatism and the other has been quite healthy. There was no history of any similar affection of the hair and nails in any member of the family. There have been no miscarriages, and the patient was born at full term.

The child presented the usual features of complete alopecia. All the nails of both hands and feet were narrow, laterally curved, and elevated from their beds at the distal extremities. Under the free margins there was a brownish-black horny mass. There were no other abnormalities. Thyroideum siccum in tablet form had been given, but, so far, without improvement.

Case for Diagnosis.

By A. WINKELRIED WILLIAMS, M.B.

A middle-aged, thin, underfed man, a very moderate smoker, abstainer from alcohol, careful in his diet, with no history nor evidence of syphilis, has suffered from dyspeptic troubles in the past; five months ago he experienced discomfort in the tongue, which has continued. Present condition: The tongue showed some general dissecting glossitis and a very peculiar area in the centre—i.e., an oblong patch extending from the middle line to a third of the dorsal surface on each side, the mucosa in the centre was pale and slightly atrophic; the well-defined border was deep red and much deeper than the rest of the surface and stood out prominently. It has remained very much the same since he first noticed it. The diagnosis seemed to rest between lupus erythematosus limited to the tongue, syphilis, and possibly an early stage of leucoplakia. The exhibitor inclined to the diagnosis of lupus erythematosus.

Since the meeting the Wassermann test has been done and the result proved negative.

The PRESIDENT said he had not seen lupus erythematosus of the tongue alone which he could be certain was that disease—i.e., in the absence of any lesion elsewhere. It did not look to him like syphilitic disease.

Case of Morphœa Guttata.

By A. WINKELRIED WILLIAMS, M.B.

A stout man, aged 35. The disease has existed five years, and extended during the last few weeks. The condition at first sight resembled a pencilling of the skin with carbolic acid—i.e., a number of mustard-seed-sized circular spots with a pink area around. There were two patches, one on the chest, the other on the back in the clavicular regions; both extended across the middle line. The patch on the back was in shape an irregular horse-shoe loop, the front patch was roughly ovoid. They consisted of a number of closely studded white dots, the majority having a slightly pigmented point in centre (a follicle): the older dots were level with or below the level of the surrounding skin; many of those at the border were slightly raised and hard—a few have become confluent, forming white streaks; nearly all were surrounded with a reddened areola. The subjective symptoms were *nil;* there was no history of syphilis. He had a sore throat, which was treated by the cautery when the eruption first evolved. There was a history of gonorrhœa.

The PRESIDENT said the lesion was very superficial, but it had an erythematous border in parts and a depressed centre.

The Practical Use of Eucerin as an Ointment Basis.

By Dr. EUGEN UNNA (Hamburg).

EUCERIN—a complex of free alcohols of the oxy- and iso-cholesterin group ($C_{26}H_{44}O_2$, $[C_{26}H_{43}O_2]O$, &c.)—is the actual water-binding component of lanolin. It was discovered and isolated from lanolin by Liefschuetz whilst he was testing the latter substance for its capacity of taking up water by means of the "aceto-sulphuric acid reaction," and

has since been thoroughly investigated by P. G. Unna. Lanolin deprived of eucerin has lost its power for binding water, whilst pure eucerin can absorb up to 700 per cent. of water. Besides the increased water capacity it possesses the following advantages over lanolin: It is quite odourless and not sticky; it can be sterilized and kept permanently; it is as easily rubbed in as lard. The cooling effect of eucerin cold cream is brought about more by rapid evaporation of water than by contraction of vessels, and is thus less irritating to the tissues than the same caused by astringents. The new ung. glycerini cum eucerin contains 400 per cent. glycerine, whereas the ung. glycerini used hitherto was able to combine only with 40 per cent. glycerine.

Eucerin may be equally applied to the cutis and mucous membranes, and is also used in subcutaneous injections with salvarsan and Hg. salicyl. Combined with liq. aluminii, liq. plumbi, witch-hazel or chamomile, it acts well in moist affections of the skin, whilst in dry diseases it is used pure or as ung. glycerini cum eucerin.

Anhydrous eucerin should be distinguished from eucerin containing water. The latter can be mixed with all common drugs—zinc, bismuth, iodine, lead, &c.—whilst anhydrous eucerin is best employed as a basis for reducing agents—e.g., ichthyol, resorcin, pyrogallol.

It may be added that pure eucerin has been used in the Hamburg hospitals as a specific for ichthyosis.

Dermatological Section.

March 21, 1912.

Sir MALCOLM MORRIS, K.C.V.O., President of the Section, in the Chair.

Case of Raynaud's Disease associated with Calcareous Degeneration.

By HALDIN DAVIS, F.R.C.S.

THE patient, a married lady, aged 34, had suffered from chilblains all her life and from Raynaud's disease for many years. During the last seven years, however, hard swellings or nodules have appeared from time to time on the fingers of both hands. These nodules, which at first are quite granular, gradually increase in size until they stretch the skin, which becomes shiny over them, and ultimately gives way, with the formation of a small opening. Through this opening there then pours a constant discharge of calcareous material in tiny pieces not larger than millet-seeds. Commonly the swellings when once opened become infected, and continue to discharge pus mingled with calcareous material for an indefinite time. This secondary inflammation makes the lesions very painful. Two fingers, one on each hand, have been definitely shortened owing to the loss of tissue from long-continued suppuration. Not only have the fingers been affected, but a similar swelling has also been formed on the right elbow, which after attaining a greater size than the swellings on the fingers has gone through similar changes, and has now been discharging for about ten months. At the present time, in addition to three swellings of considerable size on the fingers, there are numerous small and painless nodules which are evidently the first stages of new lesions of a nature exactly similar to the former. On examination of the fingers by the X-rays the calcareous deposits may easily be seen on the screen.

The toes are unaffected. The patient's general health is good; she has a baby aged 10 months, which she was able to suckle without difficulty for over six months, and which she only weaned because her

doctor thought that it was making her fingers worse. In this connexion it is interesting to note that the fingers first became troublesome when she was nursing her first and only other baby, which is now aged 8. Both the children are quite healthy.

Since the case was shown Dr. Parkes Weber has drawn my attention to an exactly similar case which he published in conjunction with Dr. Scholefield.[1] His patient was a single lady, aged 50. His article is illustrated with skiagrams of the hands, and he has given references to the other cases of the same affection previously published. There seem to be only about four papers on the subject. Dr. Parkes Weber was also good enough to get Dr. Dorner to examine chemically the calcareous material derived from my case, and he finds that it consists of calcium carbonate and phosphate. No uric acid or urates have been detected.

DISCUSSION.

Dr. ADAMSON said that he had not seen a case of this sort, but that it seemed to resemble some cases which had been described of recent years in France as "subcutaneous calcareous granulomata" (Dufour, Milian), and in Germany as "chalky metastases in the skin" (Jadassohn). Histologically the lesions in these cases had been found to be granulomata with giant cells, with the central part of the lesion occupied by chalky matter, and it had been suggested that they were infective granulomata. Another view was that the chalky deposit was the result of some metabolic disturbance, and that the granuloma resulted from the presence of this deposit. Full references to the literature of these cases, of which about a dozen had been recorded, would be found in the latest edition of Clifford Allbutt and Rolleston's "System of Medicine."

Dr. WHITFIELD said Dr. Briscoe had a unilateral case of multiple calcareous nodules, with a definite history of trauma. It had been apparently a septic lymphangitis, which calcified, but the process was progressive. The nodules consisted of lime salts. The patient was treated for a long time with phosphoric acid, which prevented any more lesions from forming, though it did not take any away. This case might be worked out on Joulie's method. In reply to Dr. Pernet, Dr. Whitfield said that after the extensive use of Joulie's method he could only report one favourable result.

Dr. DAWSON said calcareous deposits in the skin were not very uncommon. At present he had a patient with a plaque on the forehead, which he cut out, and a large one the size of a penny in the hip. It was still there, and had not caused trouble. That which he cut out was composed of gritty material.

[1] *Brit. Journ. Derm.*, 1911, xxiii, pp. 276-281.

The PRESIDENT (Sir Malcolm Morris, K.C.V.O.) said his opinion was that tophi in gouty people were not now seen as commonly as formerly. When he was younger he frequently saw people with gouty deposits—in the ear, for example; a well-known member of the medical profession had gouty deposits in the ears, and could take out of his ear a piece of chalk and write on the blackboard with it.

Dr. WHITFIELD agreed with the President's remark as to the comparative rarity of gouty tophi at the present time.

Mr. MIDELTON said he believed it was the opinion of most spa practitioners that there was not now so much arthritic gout seen as previously; the disease seemed now to be attacking the nervous system more.

Case of Congenital Xanthoma.

By J. L. BUNCH, M.D.

THE patient, a boy, first came under my care at the Queen's Hospital for Children in December last. He was then aged 6 months, and the history was that when a few weeks of age a small yellowish tumour had made its appearance on the scalp, and had gradually increased in size. It had been painted with iodine, and, when seen, was about the shape of a small bean, and was covered with a fine scale, no doubt due to the treatment. Soon after the appearance of the first tumour others began to come, both upon the scalp and upon the face, chest and shoulders. The mother is sure that no swelling has at any time diminished in size spontaneously, or shown any signs of a tendency to disappear. While the child has been under my care several fresh swellings have shown themselves, and even during the past ten days a new one of small size has come on the arm. These swellings are from the very first yellow in colour, with a smooth, slightly glistening appearance, they are firm to the touch, and are evidently situated entirely in the skin. The largest lesion is situated on the scalp, it measures about $\frac{3}{4}$ in. in its longest diameter, is flattish on the surface, and has not quite the same elastic feel that some of the other tumours have. The most noticeable and most disfiguring one is situated on the margin of the right ala nasi; it is roundish, yellow, and projects prominently from the skin, as if a pea were placed upon it. There are several others on the neck, chest, back and arms, varying in size from the large one on the scalp to the size of a millet-seed, and of different degrees of prominence. One has been excised for microscopic purposes,

and some others treated by solid carbon dioxide. The urine contains no sugar.

Sections showed a tumour involving the cutis and subjacent tissue and pressing out the overlying epidermis and flattening the papillæ. The tumour consisted of a large number of cellular elements, some of which contained xanthomatous pigmented material and a few giant cells associated with newly formed connective tissue fibres. Some of the capillaries appeared to be filled with cells resembling those which lined the capillaries, and which were, therefore, of an endotheliomatous nature. Some of the cells were granular, but these granules were not always pigmented. At the sides of the tumour the elsewhere flattened epithelium tended gradually to regain its normal characteristics, until it faded off into that of the surrounding healthy, normal skin.

Dr. BUNCH added that the structure was mesoblastic, very similar to that shown in the drawings of sections which Mr. MacDonagh figured in his paper in the *British Journal of Dermatology*,[1] which included the description of a case which Dr. Bunch had shown recently at a previous meeting.[2] In that case there were more than a hundred such tumours present, and they were of a much redder tint than those shown to-day. The sections of that case differed from the one now under the microscope in that they showed no giant cells or hyaline masses with sharply defined nuclei which resembled giant cells. He desired to draw attention to the difference which these cases showed from the ordinary xanthoma planum, and also to thank Mr. MacDonagh for cutting the sections.

Case showing an Abnormal Condition of the Nails of the Hands associated with Secondary Carcinomatosis.

By E. GREAVES FEARNSIDES, M.B.

(For HENRY HEAD, M.D., F.R.S.)

R. G., AGED 39, a married woman, had been under observation at the London Hospital under the care of Dr. Henry Head since November, 1910. The patient states that she was in-patient at the German Hospital in May, 1910, and that for abdominal tumour and uterine hæmorrhage an operation was performed. Three months after the operation she developed ascites, and for this was admitted (in November, 1910) to the London Hospital under the care of Mr. Jonathan Hutchinson. Whilst an in-patient paracentesis was twice per-

[1] *Brit. Journ. Derm.*, 1912, xxiv, pp. 85-99.
[2] *Proceedings*, p. 23.

formed. On the first occasion 13 pints, and seven days later 15 pints, of amber-coloured fluid were obtained. The fluid removed was found to contain an excess of endothelial cells.

In August, 1911, she was first seen by me. At that time she showed a considerable degree of ascites and much fairly generalized bronchitis; the glands of her left groin were greatly enlarged and adherent to subjacent tissues; the glands of her right groin were also considerably infiltrated. She was gravely wasted. Her liver was not palpable, and there was no enlargement of the axillary or cervical



Abnormal condition of the nails, associated with secondary carcinomatosis.

glands. She expectorated freely. An examination of the sputum failed to reveal the presence of any tubercle bacilli.

In October, 1911, two glands were removed from her right groin and sent to the Pathological Institute (S.D. 1059, 1911). On section these cut like a potato. The surface of the larger was in places white and smooth, in others granular and showed several small cystic spaces with ragged margins. The surface of the smaller was smooth, flat, firm, and pearly white. On microscopic examination these glands show many cystic spaces into which project numerous villous processes. The processes and the rest of the wall of the cysts are covered by one or more layers of cubical or columnar epithelium. The pathological

diagnosis of the gland is "secondary, papillary, cystic, cubical and columnar-celled carcinoma of the glands of the groin." Whilst under treatment the bronchitis has cleared. During the last four months her finger-nails have become thickened, irregular, opaque, and horny. The change was first noticed just in front of the nail-bed. She has not lost flesh.

At the present time she is thinly covered. There is considerable ascites. The glands of both groins and both axillæ are greatly enlarged, firm, hard, and adherent to surrounding tissues. The liver cannot be felt. The heart is not enlarged. There is no abnormal dullness over the chest, and but few adventitious sounds are audible over the lungs. The urinary and nervous systems are unaffected. The portions of the finger-nails which have grown during the last three months are thickened, opaque and horny. The rate of growth of the nails in recent times has been slow. The distal portions of the nails are fairly normal. The nails of the toes are to some extent affected but not to any grave degree. The skin elsewhere shows no gross abnormalities.

A Wassermann test performed on March 6 gave an entirely negative reaction.

Dr. PERNET pointed out that on the nail of the little finger there was a distinct transverse white band. He remarked that in a particular case [1] which the late Radcliffe-Crocker brought before the old Society—one of acanthosis nigricans—Dr. Pernet had pointed out a rather broad transverse white band of nails. The interesting point was that acanthosis nigricans was certainly almost always associated with some malignant disease. Dr. Crocker's case had some sort of abdominal growth (? pyloric malignant disease), and he died. He was not operated upon and there was no necropsy.

Hebra's Prurigo in a baby Girl, aged 15 Months.

By T. COLCOTT FOX, M.B.

THIS child is stated by the mother to have suffered from whooping-cough and varicella since Christmas. The prurigo eruption began about two months ago. Intensely itching typical papules are seen disseminated chiefly on the legs and arms, and the groin glands are palpable.

[1] *Brit. Journ. Derm.*, 1899, xi, p. 116.

Dr. Fox said he thought the case interesting to bring before the Section, because he thought it supported the opinion he had long held, that this type of prurigo began as seen this day, and was not preceded by urticaria or the lichen urticatus of Bateman. He thought he had obtained similar evidence in early cases in older children, and some years ago he had recorded cases of lichen urticatus lasting years, and he had never seen one become a prurigo of Hebra. The mother of this baby was certain that no antecedent urticaria or lichen urticatus ushered in this prurigo eruption. The mother was confident on this point without any prompting on the part of the exhibitor, and adhered to it on questioning.

Dr. STOWERS recalled the series of cases which the late Mr. Morrant Baker and he collected for the purpose of demonstrating that prurigo (or Hebra's prurigo, as it was called) existed among us as a substantive disease, although generally overlooked in consequence of the secondary dermatitis which supervened. He was glad to know that Dr. Fox's views corresponded with his experience, and hoped that special attention would be paid to the characteristic features, which were well marked in the case exhibited.

Case of Dermatitis Herpetiformis.

By Sir MALCOLM MORRIS, K.C.V.O., F.R.C.S.Ed.

THE patient is a female, aged 48, who came under my care first in January, 1904, when she was aged 40, for dermatitis herpetiformis. Her skin is xerodermic, and she had suffered from rheumatic fever on three occasions, the last time eight years before I first saw her. The type of the attack has varied. At the commencement there were erythematous patches and vesicular groups of lesions on the arms and legs, especially round joints, but later vesicles and bullæ. She began to take arsenic in July, 1904, and since that time has taken the drug in varying doses according to the severity of the eruption. When the arsenic was left off the eruption became worse, and when the dose of the arsenic was increased the eruption began to abate.

The disease has been quiescent for about a year. She has had during that time slight groups of vesicles on the fingers only. She has not taken any arsenic during the quiescent stage. She has suffered from slight chronic arsenical poisoning as a result of the amount she has taken. She has typical arsenical palms and soles and she has pigmentation on the skin. At the end of 1905 she had bronchial catarrh

and conjunctivitis, and the arsenic had to be stopped for some weeks, but during that time the eruption was severe. Since that time smaller doses of arsenic held the eruption in check.

DISCUSSION.

Dr. WHITFIELD said he had a case like this, which he had been seeing for five or six years. He thought it was uncommon for dermatitis herpetiformis to be absolutely controlled by arsenic, but he was able to regulate his case with mathematical accuracy. On 7½ minims per diem she was no better; on 15 minims she was a little better, and on 22 minims she was well. He had tried many other remedies, but nothing else controlled it. She had slight arsenical palms and marked arsenical neck, and his difficulty was to know whether he ought to allow her to have the skin disease or the arsenic, with its disabilities. The case had been worked out by Joulie's method, but it was of no use. The Joulie ratios were disturbed in all urticarial conditions, to which this disease was more or less allied. These patients showed a very low phosphate ratio. That was put right in his case, but it made no difference to the skin disease.

Dr. PERNET said that in severe cases with intense, maddening paroxysmal itching, insomnia, &c., and when the patient was in a very bad way, lumbar puncture was indicated.

Dr. COLCOTT FOX said the cause was very obscure. His brother always thought the disease was neurotic. Two of his original cases died in asylums. He (the speaker) did not know whether the cause was a chronic toxæmia or a neurosis.

Case of Xantho-erythrodermia Perstans (Crocker-Pernet).

By GEORGE PERNET, M.D.

THE patient was first seen by me in 1903. The complaint had then been present some four years. I labelled it in my notes at that time as an anomalous condition, with perhaps some affinities with lichen variegatus (parakeratosis variegata and so forth), though the condition differed from the latter in various ways (distribution, appearances, &c.). On November 9, 1904, the patient was kind enough to come before the members of the old Dermatological Society of London, when I demonstrated the case under the title of xantho-erythrodermia perstans, a name suggested by me, and for which I make no apology, as I consider it appropriately descriptive in the present state of our knowledge of this obscure condition. The details of the case as then shown will be found

at large in the *British Journal of Dermatology* for 1904 (vol. xvi, p. 457). This case was included in the ten instances described in the synthetic paper of the late Dr. Radcliffe-Crocker, who adopted the name I had suggested—viz., xantho-erythrodermia perstans.[1] I may say he saw my case and thoroughly examined the patient, who has been good enough to let you see him again to-day. Another case (No. 8 of Dr. Radcliffe-Crocker's paper) I saw and followed up with my old friend and teacher at the hospital. Appended to his paper will be found a histological note on a biopsy of skin I made at the time,[2] but little was gathered from my microscopical examination. The patient before you to-day is practically in the same state as when I saw him first nine years ago. A course of X-rays he had at that time had no effect on the complaint. Fortunately he suffers no inconvenience whatever and is in perfect health.

Case for Diagnosis.

By J. H. SEQUEIRA, M.D.

THE patient was a married woman, aged 40, who had had four children. Two had died in infancy from "bronchitis" and two were alive and in good health. The youngest child was aged 8. Since the birth of this child the patient had had two miscarriages respectively at the fourth and third month. Five years ago she had suffered from pneumonia and pleurisy. She was admitted to the London Hospital on March 9, 1912, with the history that a rash had appeared upon her body three months before. The eruption appeared to have been of an erythematous character, and was said to have followed the taking of conger-eel. Her hair had been falling rapidly since the outbreak. The eruption had steadily spread and no part had disappeared.

On admission there was an eruption covering the chest, abdomen, and the back, the upper limbs as far as the wrists and the thighs down to the knees on the anterior and inner surfaces. In the more central regions the eruption consisted of closely placed atrophic, oval or rounded, pearly white depressed spots surrounded by a narrow erythematous margin. Each spot was about $\frac{1}{3}$ to $\frac{1}{2}$ in. across. On the extremities the central white areas were not present, the rash consisting of smaller

[1] *Brit. Journ. Derm.*, 1905, xvii, pp. 119-134.
[2] Loc. cit., supra, xvii, p. 134.

macular erythematous spots which became slightly scaly soon after admission. The face was quite free, and there was no eruption, cyanosis, or other change on the skin of the extremities.

Before admission there was a good deal of irritation, though there had been none since. The hair was very scanty and had been falling rapidly. The scalp was slightly scurfy.

On the front of each leg there were scars of old ulcers, some of which suggested tertiary syphilitic lesions. The Wassermann reaction, however, was negative, and beyond the miscarriages there was nothing to suggest syphilis.

Soon after admission the temperature rose on one occasion to 100·3° F. without apparent cause. The bowels acted regularly. The catamenia had been irregular for some months. The urine (catheter specimen) had a specific gravity of 1015, and contained a trace of albumin. There was no sugar. The blood examination showed: Red cells, 4·18 millions; hæmoglobin, 70 per cent.; colour index, 0·8; leucocytes, 8,000; polynuclears, 68 per cent.; eosinophiles, 0·5 per cent,; small lymphocytes, 6·5 per cent.; large lymphocytes, 14 per cent.; large hyaline cells, 11 per cent.

There was no other evidence of visceral disease, and the mouth was unaffected. The case might be classed as an atrophying erythema, and the condition of the skin was probably the expression of a grave toxæmia. The patient was obviously very ill, and she was brought to the meeting with some difficulty, as she was unable to walk owing to weakness.

The absence of any affection of the face and extremities, the exhibitor considered, placed the case in a somewhat different position to the acute forms of lupus erythematosus.

DISCUSSION.

Dr. PERNET thought the case came into the category of lupus erythematosus, on account of the albuminuria, the cold extremities, the rises of temperature, &c. With regard to the origin of the condition, in the paper which he wrote on acute pemphigus[1] he mentioned the case of a man who developed acute pemphigus after eating putrid conger-eel.

Mr. MACDONAGH said there was a case in St. Bartholomew's Hospital in 1907 absolutely identical with this one. It was under Dr. Drysdale, and was admitted for skin trouble associated with periodic rises of temperature. The skin condition consisted of an erythema, which gave way to discrete papules, which in time became atrophic in the centre. The hair, as in the case shown, also fell out. During her stay in hospital patient not only got rises of tempera-

[1] *Brit. Journ. Derm.*, 1896, viii, p. 209 (footnote 24).

ture, but attacks of erysipelas and herpes developed in other parts of the body, all of which improved under quinine. Some months later she was admitted with pneumococcal pericarditis, for which she was operated upon, and died. The diagnosis was lupus erythematosus, followed by acute pneumococcal infection and death.

Case of Lichen Planus Annularis.

By J. H. SEQUEIRA, M.D.

THE patient was a woman, aged 56, a private maternity nurse. She was married and had had nine children, six of whom were alive. There were no miscarriages. The skin affection had been present for three or four years. It began upon the left leg with intense itching, and gradually involved the right leg and then the forearms. She was treated at Guy's Hospital two years ago, and had improved after some months' treatment. She was transferred to Dr. Sequeira by Dr. Theodore Thompson on February 20.

The eruption was widely spread over the extremities and presented a number of annular areas about ½ in. across, formed of smooth, shining, flat-topped papules. Between the rings there were numerous isolated papules and occasionally irregular plaques formed by the aggregation of numbers of the papules. There was a copious eruption of small white papules on the buccal mucosa.

There were no general symptoms except headache and pain in the back, which had persisted since the menopause, three years ago.

Dr. Sequeira commented upon the number of cases of lichen planus which he had seen recently both in his hospital and private practice.

DISCUSSION.

Dr. GALLOWAY said that he had drawn attention two or three years ago to the occurrence of lichen planus in patients suffering from the chronic forms of glycosuria. Whenever the skin disease was found to occur in a stout patient, he thought the possibility of glycosuria should immediately arise in the mind of the physician and the requisite investigation be made. Recently he had had the opportunity of seeing several cases of lichen planus within a short space of time, and one of the most widespread attacks occurred in a lady aged about 50, who was both stout and had glycosuria.

Dr. COLCOTT FOX said a remarkable case was demonstrated by Dr. Cavafy at the old Dermatological Society of London, showing unquestionably individual papules becoming circinate, but confluent patches of papules becoming circinate were more frequent.

Cases of Double Cervical Ribs associated with Vascular Phenomena suggesting Raynaud's Disease.

By E. G. FEARNSIDES, M.B., and J. H. SEQUEIRA, M.D.

CASE I.

DR. FEARNSIDES (for Dr. H. HEAD) showed a case of *double cervical ribs associated with vasomotor disturbances (Raynaud's phenomena) of left forearm and hand and of right hand, with slight wasting and weakness of muscles of left hand.*

R. D., a married woman, a "cutter out by trade," aged 41, has been under observation at the London Hospital under the care of Dr. Henry Head since July, 1911. She complains of pains in the left forearm shooting down the inner side of the limb into the hand, tingling in the tips of the fingers of the right hand, and attacks of redness, blueness and lividity of the forearm, arm and fingers on the left side, and of right hand, accompanied by local sweatings of the affected portions of the limbs, and general nervousness.

The patient states that as a girl she was subject to "dead fingers" of her hands, but that the left hand was always more liable to these than the right. In youth and early married life she was only subject to these attacks of "dead fingers" when the weather was extremely cold. In December, 1910, in consequence of family troubles, she had to change her occupation. Almost immediately afterwards she became subject to sharp pains shooting along the inner side of the left forearm, dragging sensations in the left axilla, and feelings of numbness and pins and needles in the fingers of the same side. Shortly after this she noticed that the nail-beds of the fingers of the left hand had altered, the lunulæ of the nails had become reddened, and she became subject to attacks of pain in the fingers of the left hand. In January, 1911, she had a "nervous breakdown"—with great weakness and an inability to stand —of mental origin. She attended as an out-patient at the National Hospital, Queen Square. In February, 1911, she was sent thence to a convalescent home. Since that time her general condition has improved, but the local pains and vasomotor disturbances (attacks of blueness, greyness and lividity of the left hand, and to a less severe degree of the right hand) have become more and more troublesome.

Lividity, with vasomotor spasm and pain, can now readily be elicited from the left hand and forearm by exposing the hand to relative cold. The small muscles of the left hand and the muscles of the inner side of the left forearm have, during the time the patient had been under observation, become definitely weak and slightly wasted.

Present condition: Introspective; emotional. Whilst in-patient in January, 1912, the patient had two "minor hysterical attacks," with emotional display, temporary blindness, and complaint of headache and choking, followed by amnesia of the attack itself. Speech unaffected. Headache chiefly occipital, not constant; occasionally also vertical "opening and shutting" headache. Lower limbs unaffected. Slight but definite wasting of muscles of thenar and hypothenar eminences, of interossei on left side. Left grip feeble, right fair. All muscles act on volition, though the power of the small muscles of hand is small on the left side. Electrical reactions: all muscles react normally to coil and cells; no fibrillary twitchings seen. To tests with cotton-wool, heat, cold, vibrations of a tuning fork, and passive movements, no discoverable sensory loss. To pin-prick, loss of acuity in sensation along inner border of left forearm; edges indistinct and cannot be mapped. The tendon reflexes on both sides from arm and leg are all extremely ready. The plantar response on both sides is flexor in type. Fundi unaffected. Cranial nerves good. Sphincters unaffected. In general the left hand is blue, and has a sodden, moist, clammy appearance. On exposure of the limb to relative cold the hand becomes mottled, blue or purple, and the finger-tips assume a grey-blue colour. The change in vascularity on exposure is limited to the lower half of the left forearm, the left hand and the fingers of this side. Recently a similar but less marked mottling has appeared under similar circumstances over the right hand and wrist. The pulses in the two radial arteries are equal and synchronous. The subclavian vessels in the neck are not over prominent. Urine natural. Hæmoglobinuria never observed. There is no palpable cervical rib on either side, but in the skiagraph on each side there appears a well-developed cervical rib.

Case II (Dr. Sequeira's Case).

The patient, a married woman, aged 29, has had two stillborn children, one at full term and one at six months. She had always had good health. For the past eight years she had noticed a mottling of

both arms extending from the upper arm to the finger-tips. Both arms were affected equally. There was no pain, and no uneasiness, and there was no apparent alteration of sensation. The muscles of the hands were not wasted. The mottling was curiously located to the inner side of both hands and forearms; it had a purplish-blue colour. There was no general mottling of the extremities, though the patient states that occasionally she had had some lividity about the knees. The curious distribution of the cyanosis, in view of the cases which had recently attracted attention in neurological circles, led the exhibitor to have a skiagraph taken. This showed that the patient had a rudimentary but well-defined cervical rib on each side.

The association of cervical ribs with vascular phenomena was now a well-established fact, and it suggested the examination of the neck by radiography in doubtful cases of Raynaud's disease.

DISCUSSION.

Dr. GALLOWAY desired to support the observations made by Dr. Sequeira and Dr. Fearnsides. Some of the members present might remember the case of a young man with supernumerary cervical ribs, who came from time to time to Charing Cross Hospital, and who had been the subject of more than one demonstration. This patient, a young man, was originally sent to Dr. Galloway on account of what was supposed to be a pulsating tumour on the left side of the neck above the clavicle. The tumour turned out to be the subclavian artery supported on a well-developed cervical rib. This patient suffered from unusual "neuritic" sensations in the left arm and hand, especially felt on the outer and inner borders, and there was possibly also some loss of power in the muscles of the thenar and hypothenar eminences. When the arm was in the dependent position there was also a tendency to stagnation of the blood in the extremities. The phenomena of interference with the nerves and with the blood-vessels in the posterior triangle of the neck by a cervical rib were well recognized, and attention had been drawn to them in various interesting papers by Sir William Osler and others. The symptoms were sometimes so pronounced that they simulated such conditions as erythromelalgia and even Raynaud's disease, and in the diagnosis of these two latter conditions the possibility of the occurrence of a supernumerary cervical rib should always be borne in mind. The symptoms associated with interference of the blood supply through the subclavian artery and the return of blood through the subclavian vein could easily be understood, but the reticulate erythema shown in one of Dr. Sequeira's cases could only be looked upon as the result of long-standing congestion of the skin such as might occur from many causes, and could hardly be considered as a vasomotor disturbance due to the influence of the cervical rib. In Dr. Galloway's patient already referred to there was a supernumerary

cervical rib also on the right side which appeared to give rise to no disturbance. Since the use of X-ray examination many cases of cervical ribs had been proved to exist which appeared to give rise to no trouble.

Dr. F. PARKES WEBER said that Dr. Fearnsides's case was very similar to one which he showed at the Medical Society of London on April 26, 1909. In his (Dr. Weber's) case there were cervical ribs on both sides, but the phenomena in the hand muscles were much more marked on the right than on the left side. The cervical rib on the right side was afterwards removed, and though the result from the operation was at first not satisfactory, there was some improvement later on. The patient's brother and sister were both found to have cervical ribs, though they had not any atrophy of hand muscles. Dr. Sequeira's case looked different from that of Dr. Fearnsides; the mottling or "livedo annularis" was not present in Dr. Fearnsides's case or in his own; in Dr. Sequeira's case there was no wasting in the thenar or hypothenar eminences or in the interossei; and there were no sensory symptoms (pain or paræsthesiæ) whatever. Dr. Weber thought that "livedo annularis," such as was present in Dr. Sequeira's case, was not dependent on the presence of cervical ribs, though the localization to symmetrical areas on the upper extremities was very striking.

Sections of the Skin of a Kitten affected with Microsporon Ringworm, and a Culture from the Hair of a Child infected by the Kitten.

Shown by A. WHITFIELD, M.D.

THE sections were stained with lithium carmine, and after-stained by Gram's method, and decolorized with hydrochlorate of aniline dissolved in aniline oil. The specimens showed that the fungus had the ordinary arrangement of the microsporon as regards the mosaic of spores surrounding the hair and lying within the internal root-sheath. Invasion of the hair-shaft was, however, very slight, the fungus maintaining an almost purely ectothrix position. The skin before being cut and stained showed no characteristic bald or partially bald places, and the hair was not broken. Microscopically it could be seen that only a very small proportion of the hairs was infected, differing in this respect very materially from the condition that obtained in the child. The culture showed a peculiar silky growth, with very marked radiating striæ, and absolutely no folding of the surface, thus differing from both the *Microsporon Audouini* and the *Microsporon lanosum* of the dog.

Clinically there were some points which had led Dr. Whitfield to suspect the kitten and have it examined. The chief of these was that the disease had started in the child with a very heavy infection of the glabrous skin in the form of large rings. This was, of course, quite unlike the course of events in the ordinary microsporon infection.

DISCUSSION.

Dr. COLCOTT COX agreed with Dr. Whitfield's remark concerning kitten ringworm producing tinea circinata of the skin. He possessed a drawing of such a case, with great rings all over the body.

Dr. BUNCH asked Dr. Whitfield what was the relative proportion of kitten ringworm in which the microsporon was present. In a paper on "Ringworm Infection in Man and Animals" he had published reproductions of cultures obtained both from the patient and the kitten (the source of infection), but that was an endothrix, and he would be very interested to know whether Dr. Whitfield had come across a similar case.

Dr. PERNET said he had brought a case of tinea circinata contracted from a half Persian kitten before the old London Society in 1904[1]. In the scrapings from the patient the fungus corresponded to *Microsporon Audouini*, and material he had obtained directly from the kitten (ears) when examined by him showed numerous small spores arranged about the hairs as in *Microsporon Audouini*.

Dr. WHITFIELD, in reply, said he was not at all familiar with ringworm in the kitten, but it seemed a very difficult condition to diagnose. A large piece of the skin of the kitten was sent to him by Professor Hobday, the veterinary surgeon, and it showed no obvious sign of the disease. There were no rings of any kind in it, nor bare patches. On microscopical examination of horizontal sections it was found that about only one hair in twenty or thirty was affected, and it was therefore quite possible to overlook kitten ringworm. Dr. Bunch's case of endothrix in a kitten was, he believed, the only case of the kind on record. He did not know of Dr. Pernet's case.

[1] *Brit. Journ. Derm.*, 1904, xvi, p. 247.

Dermatological Section.

April 18, 1912.

Sir MALCOLM MORRIS, K.C.V.O., President of the Section, in the Chair.

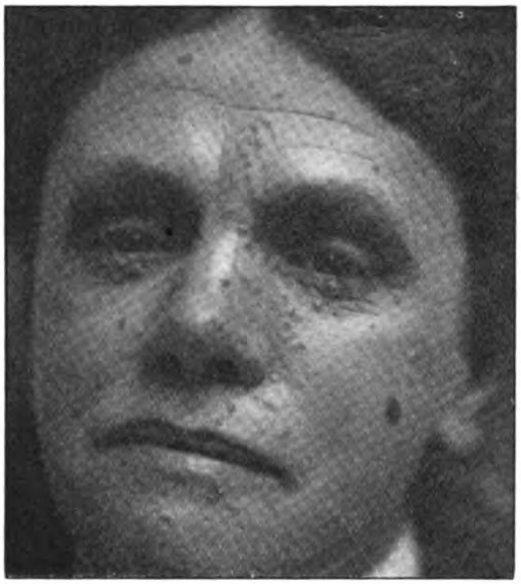
Case of Multiple Benign Cystic Epithelioma.

By S. E. DORE, M.D.

THE patient had been under the care of Sir Malcolm Morris and subsequently under that of Dr. Pringle at the Middlesex Hospital. She was a stout, somewhat anæmic-looking woman, aged 54. The first thing noticed was what she described as a "little blackhead" in the centre of her left cheek six years ago. This slowly increased in size until a small raised tumour was formed, which, during the past four years, had ulcerated and healed from time to time. During the past eighteen months a large number of small growths (about twenty in number) had appeared on the forehead, nose, eyelids, cheeks and chin. Two of these were larger than the others and had undergone ulceration; one (previously mentioned), in the centre of the left cheek, was about the size of a shilling, and a second, on the lower eyelid, about the size of a hazel-nut, had formed more recently. The small growths were grouped around the nose, eyes and mouth, and varied considerably in character and size. The majority were small, flat papules or nodules 2 mm. or 3 mm. in diameter. They were slightly yellowish or of the same colour as the skin, and oval, circular, or irregular in shape. The lesions were, for the most part, discrete, but some had coalesced, forming slightly red patches scarcely raised above the level of the normal skin. The nodules were solid and firm to the touch, but one or two showed central suppuration and a few were umbilicated. There were no milium-like bodies or dilated capillaries. The two larger tumours had the pearly edge and superficial ulceration like that of rodent ulcer, with which they appeared to be clinically identical.

In addition to the lesions on the face there was a small, raised tumour below the left breast and another on the right shin; neither of these had ulcerated. The patient also had a molluscum fibrosum on her right shoulder, and her brother had a similar tumour on the back.

Microscopical sections were made from a small lesion on the forehead, and showed that the new growth consisted of masses of epithelial cells, which retained their prickles, and which appeared in some of the sections to be in direct connexion with the basal layer of the rete. There were a few commencing cysts, but these were not a marked



Case of tricho-epithelioma.

feature of the growth. Dr. Whitfield kindly looked at the sections and agreed that the appearances were characteristic of tricho-epithelioma.

Clinically, the case seemed to resemble those described by White, Stelwagon, Jarisch, and other observers, in which some of the benign epithelial tumours had undergone malignant changes.

DISCUSSION.

Dr. SEQUEIRA said he had had a patient under his care for some years who had dozens of these growths, and he had been struck with the remarkable manner in which they disappeared under X-ray treatment. That man had been attending his department for a long time, and he had an illustration of

his case in his book, showing a large number of growths all over the face. They were all of the same type, and the latter ones after being cut were almost indistinguishable from rodent ulcer.

Dr. HALDIN DAVIS remarked that Dr. Adamson published some time ago a case which had been called multiple rodent ulcer which was very similar to the present case, and which Dr. Adamson showed was really a case of multiple benign cystic endotheliomata. There were seven tumours on the face, some of which broke down and gave rise to ulcers exactly similar to rodent ulcers.

Dr. DORE, in answer to Dr. Colcott Fox, said the patient's brother had a similar condition, and several of the tumours had undergone ulceration as in the present case. The grandfather and another brother were reported to have had single wart-like tumours on the skin.

The PRESIDENT (Sir Malcolm Morris, K.C.V.O.) remarked that many of the clinical features in this case differed from those in rodent ulcer. He did not diagnose it as rodent ulcer, not even the cystic form. He agreed with Dr. Dore's diagnosis.

Dr. DORE, in reply, said the tumours rapidly disappeared in the case of the brother on the application of carbonic acid snow after having been previously treated by Finsen light, radium, and X-rays. In the present case the ulcer on the cheek had been partially removed by carbon dioxide snow, that on the lower eyelid had been treated by radium. Dr. Adamson had suggested that many of the cases of multiple rodent ulcer were originally like this case at the beginning and became rodent later.

Case for Diagnosis.

By WILFRID FOX, M.D.

THE patient was a man, aged 63, a traveller by trade, who presented a practically universal eruption. He was very nervous and shaky, but had had no serious illness of recent years. The exhibitor was doubtful as to whether it should be classed under pemphigus foliaceus or dermatitis herpetiformis. It started on the forehead eighteen months ago with a small pustular lesion; this spread, resembling, according to the patient, a pustular eczematous condition, over the face on to the chest, and after that rapidly on to the rest of the trunk and limbs. There were no bullæ in the early stages at all. When first seen, however, four months ago, he showed definite bullæ on the hands and forearms, the rest of the skin being red and desquamating, and exuding a small amount of foul-smelling moisture. The bullæ were flaccid, and

after they ruptured exfoliation went on as seen in pemphigus foliaceus. There was also definite shrinkage of the skin, and the smell was characteristic of that condition. The nerve signs, however, were rather more in favour of dermatitis herpetiformis, and also the grouping of the lesions. The areas were extremely tender, and he complained of frequent shooting pains before and after the eruption came out, but there was very little itching. There were no lesions in the mouth, nor had there been any intestinal disturbance. *Staphylococcus aureus* and a streptococcus which could not be exactly identified were found by Dr. Slater in the unruptured bullæ. A vaccine had been tried, which appeared to do good for a time, and then had no further effect.

DISCUSSION.

The general opinion of the members present was that it was a case of pemphigus foliaceus.

The PRESIDENT remarked on the fact that he did not now see so many cases of exfoliative dermatitis of various types as he did thirty years ago. He did not know whether they were actually rarer, and if so whether it meant that preliminary treatment was now more thorough.

Dr. SEQUEIRA also believed the cases were not now so common. Since Dr. Savill recorded the series of cases at the Paddington and other infirmaries there did not seem to have been similar epidemics.

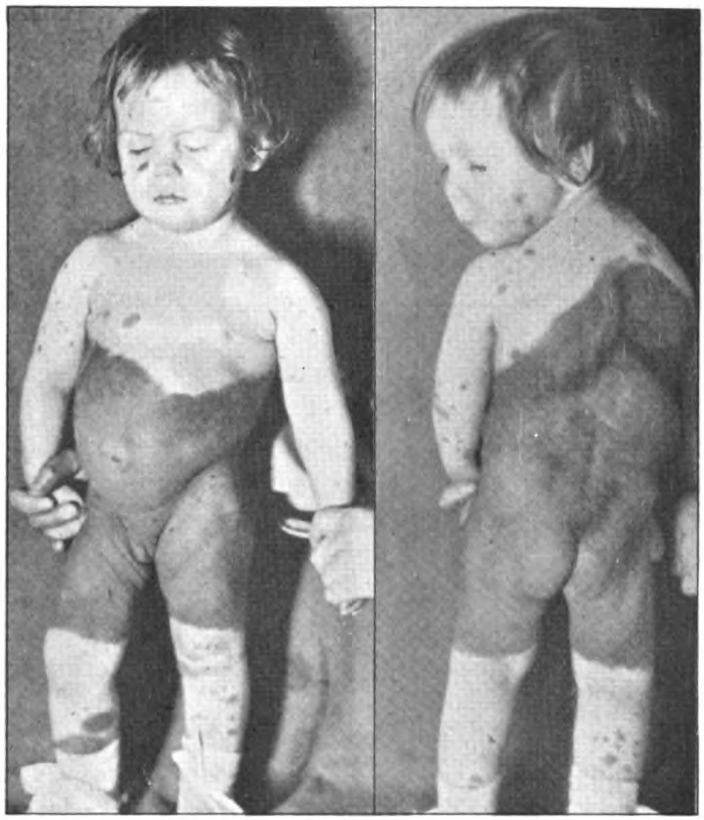
Dr. WHITFIELD, remarking on the comparative infrequency of such cases, suggested that the prohibition of the preservation of milk with borax might be a possible explanation.

Extensive Pigmented Hairy Mole on the Trunk and Small Pigmented Moles on the Face and Limbs in a Female Child, aged 2 years.

By S. E. DORE, M.D.

THE child was the youngest of three, all girls, and was the only one affected in this way. On the trunk the mole occupied the "bathing-drawers area," extending round the body in a wide band from the nipples and scapulæ above to the knees below. The skin was deeply pigmented, and in parts covered with fine downy hair. Over the scapulæ and in the intra-scapular region, however, there was a profuse growth of long

dark hair. Below the angle of the right scapula the skin was slightly raised and thickened with the appearance and consistency of a diffuse lipoma. On the face and limbs there were a large number of scattered pigmented patches of various shapes and sizes.



Extensive pigmented hairy mole.

DISCUSSION.

Dr. DORE had removed one or two of the face lesions by means of carbonic acid snow, but recurrence took place. In answer to Dr. Whitfield, there was a history of a maternal impression, namely, that when three months' pregnant she saw a child run over by a cart.

Dr. WHITFIELD said he asked the question about maternal impression because he had heard a most remarkable history of maternal impression from a doctor who attended the mother of a child with an enormous mole. It was that when the mother was pregnant she was startled by her husband throwing the kitten on to her back in the dark, and she felt the furry thing brush against

her neck. The woman went about for months saying she felt she would give birth to a kitten. At the birth she inquired very anxiously of the doctor if the child was all right. He reassured her, but the child was almost entirely overed with fur.

Cases with Characteristics of Lupus Erythematosus.

By J. M. H. MacLeod, M.D.

(1) *Lupus erythematosus in a Woman, aged 31.*—The disease was of the acute type, affecting the face, ears, and side of the neck. It involved the greater part of the skin of the face, was markedly symmetrical, was absent from the scalp, and presented the characteristic features of the disease. The patient was a delicate-looking woman. Last summer she had a severe attack of gastritis, which was attributed to a septic condition of her mouth, the result of carious teeth, which in consequence had all been removed. Previous to this she had suffered from indigestion and rosacea. While the gastric symptoms were most severe an erythematous eruption had appeared on her nose and spread over her cheeks so rapidly as to suggest an ordinary toxic erythema. This gradually extended till it reached its present distribution, the erythema becoming associated with scaliness and atrophy, and assuming the characteristics of lupus erythematosus. At the present time her gastric symptoms have improved, but she is now suffering from kidney disease, and has albumin in the urine.

(2) *Superficial late Syphilide of the Face and Scalp simulating Lupus erythematosus.*—The patient was a man, aged 40, who had been infected with syphilis about eighteen years ago. When seen by the exhibitor six weeks ago he presented on the face and scalp a widely and symmetrically distributed erythematous scar-leaving eruption. This involved the cheeks, nose, and sides of the neck, where it appeared as pinkish-red, roundish or irregular patches, not definitely raised, and scaly in the centre. The lesions were pinkish rather than brown in tinge, and on pressure with a diascope left little or no brown stain. These patches had appeared first about three years after the syphilitic infection, and had tended to disappear spontaneously, leaving depressed scars. On the scalp the lesions occurred in the form of bald, irregular atrophic areas, the more recent of which were erythematous. When first seen by the exhibitor the diagnosis of the affection from lupus erythematosus was clinically almost impossible, though the history pointed strongly to

its being of syphilitic origin. A Wassermann reaction was done but gave a negative result; this was before treatment was instituted, and could be accounted for by the latency of the disease. After that, mercury and iodides were prescribed, and a marked improvement immediately took place, so that at the time of exhibition the greater part of the erythema had died away. The exhibitor had met with several cases in which the greatest difficulty had been presented in the differential diagnosis between a late tertiary syphilide and lupus erythematosus. This indicated that the toxins produced by the spirochætes might be of such a degree of virulence as to be capable of producing an erythematous scar-leaving eruption simulating lupus erythematosus.

(3) A drawing of a *case of persistent chilblains* on the hand; these had at first only occurred in the cold weather, but had gradually become persistent throughout the year, and had finally assumed a permanent scaly and atrophic condition indistinguishable clinically from lupus erythematosus.

The object of bringing these cases before the Section was to raise the question as to whether it was advisable to continue regarding lupus erythematosus as a definite disease, and not rather as a final stage in a persistent erythematous process, which might result from a large variety of causes possibly in a predisposed individual.

DISCUSSION.

Dr. HALDIN DAVIS asked whether a Wassermann reaction had been done. He was reminded of a case of a soldier who was invalided out and discharged from the Army because he had syphilis, and he brought the case up as one of lupus erythematosus, which looked like syphilis. However, a Wassermann reaction done twice was negative, and it cleared up with ionization. But that was not a test of lupus erythematosus, for, of course, many cases of that disease failed to respond to that treatment. A portrait of the case had been published. A point in which his case differed from the present one was that his own patient denied all history of syphilis, while in the present instance syphilitic infection was clearly proved.

Dr. BOLAM said he had had a similar case which he had watched for five years, in which the same phases were gone through as Dr. MacLeod described: First, puffy red swellings, most noticeable on the scalp, and the case had now a more aggravated condition of scar than the man now shown. The case was first diagnosed as lupus erythematosus. Later he came under care in the condition that this man was in, with a lesion on the chest which one would not hesitate to describe as a tertiary lesion. He gave a positive Wassermann, but had been

most resistant to all forms of treatment. When seen ten years ago on the medical side he was thought to have specific aortitis.

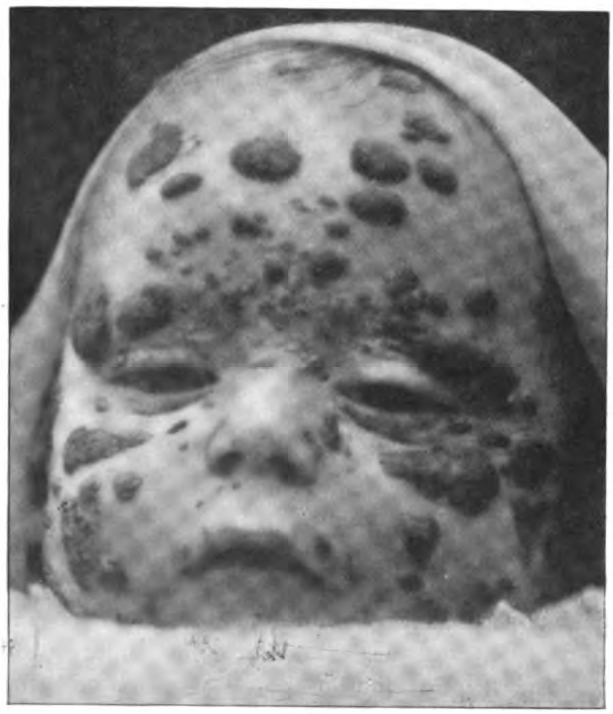
Dr. BUNCH said that, as exemplifying the difficulty of diagnosis in some cases, he had seen a few days previously a patient who had been shown some time ago in this Section as suffering from lupus erythematosus, but some time afterwards the lesions on the face broke down, ulceration took place, and perforation of the palate ensued, and he did not doubt that the patient's trouble was syphilitic all the way through. The Wassermann reaction was at the present time positive, but it was, of course, impossible now to say what it was when the patient was exhibited previously.

Dr. MACLEOD, in reply, said the Wassermann reaction was negative, but it had only been tried once. The lesions continued with scarring two years after the appearance of the chancre.

Case of Bromide Eruption.

By G. NORMAN MEACHEN, M.D.

THE patient, a private case, was an infant, aged 6 months, who was vaccinated on March 7. Five days afterwards a severe convulsion



Case of bromide eruption.

occurred for which a medicine had been given. Two days after this "spots began to appear upon the face and thighs." It transpired that the mixture contained 2 gr. of potassium bromide, to be given every four hours, this being continued for five days. The total amount of bromide administered amounted, therefore, to 1 dr. The child had been losing weight, and the lesions were still making their appearance when the case was sent to the exhibitor.

On examination, the eruption was pretty widely distributed over the face, scalp, trunk, and lower third of the outer sides of the legs. Upon the face the lesions were oval, dark brown, and fungating, while others were condylomatous. On the legs more recent pustular lesions as well as nodules were seen. The condition of the eruption in many parts tallied with that described by Pini under the name of "Bromoderma nodosum fungoides."

Fibromata in a Girl, aged 17.

By Sir MALCOLM MORRIS, K.C.V.O., F.R.C.S.Ed.

THE patient had had fibroma since infancy, but he did not regard the condition as Recklinghausen's disease; there was no pain. Other lumps were now appearing very rapidly in various parts of the body, including the limbs and chest, which latter situation was an inconvenience, as she might soon be wishing to wear evening dress. He wished to hear whether any member had had success in this condition from treatment by fibrolysin or anything else, and whether anything could be done to prevent the further formation of these tumours. Some cases had been recorded in which there was an increase of the tumours at puberty and shortly after. A specimen from one of the tumours was exhibited under the microscope. The growths seemed to be soft fibrous tissue. He had tried arsenic without effect; there was no involvement of the mucous membrane. He had seen a case in which fibrolysin was tried for extensive keloid, but it did harm. Had anyone had benefit from thyroid internally? He considered it stood out from the group of Recklinghausen's cases because of the absence of subjective symptoms.

DISCUSSION.

Dr. MACLEOD said that in view of the way in which keloids cleared up after the application of radium it might be well to try radium here. He did not think that fibrolysin would be beneficial, as the results of injecting it in keloids were most disappointing.

Dr. KENNETH WILLS (Bristol) said he had tried X-rays on one case of the kind and there was some absorption in six months, but in the case of three or four large pedunculated masses it seemed to have little effect.

Dr. WHITFIELD said he regarded the case as one of Recklinghausen's disease—neuro-fibroma. He regarded the fact that one could push the tumours backwards and forwards through a hernial opening of the skin as characteristic.

Dr. PARKES WEBER said he thought that all cases of multiple molluscous fibromata belonged to the same class as typical (complete) cases of Recklinghausen's disease, in which there were spots of cutaneous pigmentation and tumours closely connected with nerve-trunks in addition to multiple molluscous fibromata (subcutaneous tumours). In the present case there was a greater vascular element than usual, so that some of the subcutaneous tumours looked almost like venous angiomata, and the skin over them was very rich in capillaries. This appearance was quite different from the bluish appearance of small (commencing) molluscous fibromata when they were beginning to distend the skin covering them.

Case of Sclerodactylia with Subcutaneous Calcification.

By R. E. SCHOLEFIELD, M.D., and F. PARKES WEBER, M.D.

THE patient, E. M. D., is a rather thin, unmarried woman, aged 50. As a child she used to be subject to redness of the hands and to chilblains in her hands and feet. At about the age of 20 she began to be troubled with her fingers "going dead" (local syncope); that is to say, they used to become white, and afterwards she had a disagreeable sensation of "pins and needles" when the normal colour was being regained. Since the age of 35 the skin and soft parts of her fingers have been gradually becoming harder and stiffer, so as to constitute a condition of sclerodactylia. From time to time indolent, painful ulceration has occurred on the fingers, chiefly at the tips, giving rise to a gritty discharge. About three years ago a similar area of swelling and redness appeared on the left olecranon. This area ulcerated and was scraped by

Dr. Scholefield, who examined the gritty material removed, but with negative result in regard to the presence of urates.

Condition of patient in June, 1911: The ends of the fingers have the characteristic appearance of sclerodactylia, and in addition to that there are scars from which gritty material has been extruded. The terminal segments of some of the fingers appear shortened. The flesh over the flexor aspects of the terminal phalanges of the second, fourth and fifth fingers of the left hand seems particularly altered and feels quite hard, as if subcutaneous calcification had taken place. Indeed, the tip of the little finger is white and looks as if some calcareous material was shortly going to be extruded. Owing to the thickening and hardening of the soft parts, movement in some of the finger-joints has become diminished. There is no true dissociation of sensation anywhere, but all sensation is somewhat blunted over the portions of the fingers where the skin has been callous.

The presence of calcification in the fingers is confirmed by examination with Röntgen rays, as it also has been by means of incision. Skiagrams of the left hand (fig. 1) reveal considerable calcification in front of the terminal phalanges of the second, fourth and fifth fingers, and likewise a little at the tip of the thumb. In skiagrams of the right hand one can make out a little subcutaneous calcification in several fingers, less than in the left hand, but not so confined to the terminal segments. The distal portion of the terminal phalanx of the right middle finger has disappeared (fig. 2).

The patient's feet show nothing abnormal either by ordinary examination or by skiagraphy. There is apparently no sclerodermatous change anywhere except in the hands (sclerodactylia). The pinnæ of the ears show no concretions of any kind. There is no thickening of the ulnar nerves. There is slight redness and swelling over the right olecranon. This has only recently been observed, and resembles the redness and swelling over the left olecranon, which, as has been already stated, occurred three years ago, and resulted in a discharge of gritty material. Skiagrams of both elbows show nothing abnormal except a minute calcareous speck over each olecranon. The knee-jerks are natural. There is no evidence of any disease of the thoracic or abdominal viscera. The urine is free from albumin and sugar. No examination of the patient's metabolism has yet been made. There is no evidence of any congenital or acquired syphilitic taint.

Since the above notes were written the patient has suffered a good deal from indolent painful ulceration of the left index finger, with

discharge of gritty material. Near the right olecranon, also, a little (doubtless calcareous) nodule can now be felt in the subcutaneous tissue.

That the subcutaneous concretions in this case are not of the nature of gouty (tophaceous) deposits is abundantly proved by their opacity to Röntgen rays, and by the fact that Dr. Scholefield examined the gritty



FIG. 1.

Skiagram of left hand.

discharge from the left olecranon with negative result in regard to the presence of urates. Moreover, some gritty particles, recently discharged from the tip of the left index finger, have been kindly examined for the authors by Dr. G. Dorner. His examination shows the presence of calcium carbonate and phosphate and the absence of any uratic salt.

The appearance of the fingers to some extent reminds one of the

stumpy, thickened fingers of the so-called "Morvan type" of syringomyelia, but there is no "dissociation of sensation," and the ulceration (giving rise to the gritty discharge) has been associated with great local tenderness and has been therefore quite different from so-called "painless whitlows."

The case is very similar to the case shown by Dr. Haldin Davis at the last meeting of the Dermatological Section, under the heading "Raynaud's Disease associated with Calcareous Degeneration." The

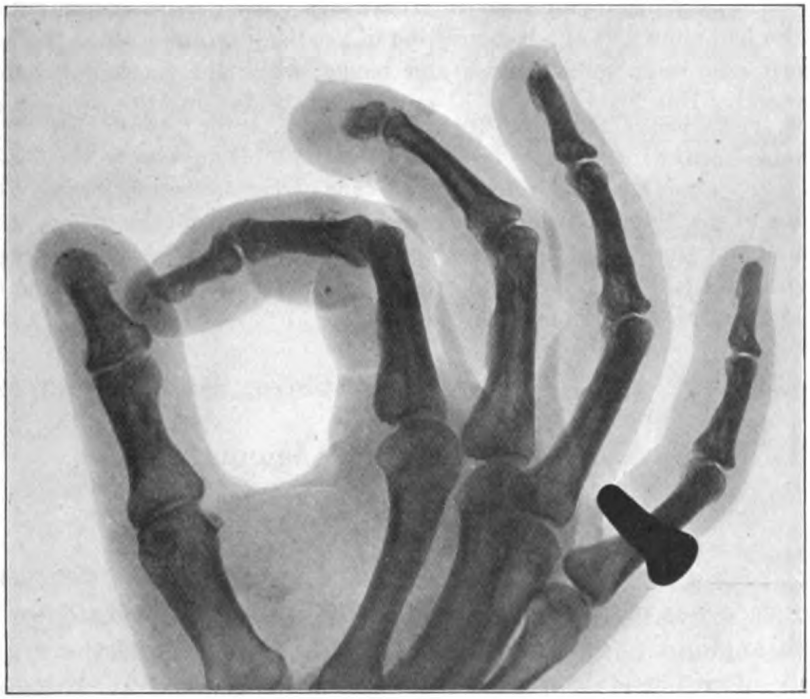


FIG. 2.

Skiagram of right hand.

literature on the subject was referred to by Dr. Scholefield and Dr. Weber in their original account of the present case,[1] but in addition to that Dr. G. Norman Meachen has kindly informed the authors of an evidently similar condition in a woman, aged 59, shown by him on April 23, 1903, before the Dermatological Society of Great Britain and Ireland.[2]

[1] *Brit. Journ. Derm.*, Lond., 1911, xxiii, p. 276.

[2] *Brit. Journ. Derm.*, Lond., 1903, xv, p. 216.

DISCUSSION.

Dr. SCHOLEFIELD said he had tried to remove a nodule from the ring finger, expecting to find a sequestrum. He thought it would save some of the time which was occupied in exfoliation by ulceration. But it was found to be calcification of subcutaneous tissue, and could not be wholly removed. It was scraped with a sharp spoon. The portion removed was of the size of a three-penny piece, and consisted of hard, calcareous material intimately united with the subcutaneous fibrous tissue.

Dr. HALDIN DAVIS said that this case was clearly of the same nature as the one he had shown at the last meeting of the Section, except that the lesions in his own case were much grosser and bigger, while the hardening here was more general. But his case was of only seven years' duration, whereas this present case had lasted twenty years.

Dr. NORMAN MEACHEN referred to a case of this affection shown by him before one of the old societies in 1903,[1] in which the tips of the fingers were so hide-bound and anæsthetic that one or two members present seriously suggested the diagnosis of leprosy. He had no doubt, however, that his case came under the same category as that now exhibited.

Case of (Infective) Angioma.

By J. H. SEQUEIRA, M.D.

THE case was of the type described by Sir Jonathan Hutchinson as infective angioma. The patient was a girl, aged 20, and the eruption was noticed on her right upper arm when she was aged 2. It had continued to spread very slowly, and now reached from the clavicle to the hand. It consisted of minute red and purplish-red spots about the size of a grain of cayenne pepper. In part the lesions were aggregated to form patches, but in the main consisted of minute, closely set spots. A fuller account of the case with report on the histology will be published shortly in the *British Journal of Dermatology*.

The PRESIDENT said it seemed like a spreading nævus called by Hutchinson infective angioma. In these cases touching with the actual cautery did good.

[1] Vide *Brit. Journ. Derm.*, 1903, xv, p. 216.

Case for Diagnosis.

By J. H. SEQUEIRA, M.D.

THE patient, aged 28, a dock labourer, was only seen that morning, with a large pustular swelling on the right upper maxilla. Five weeks ago a small pimple was said to have appeared under the right eye, and that gradually spread, and during the last fortnight other spots had appeared on the face and neck. At first there was some pain and itching, but there was none now. There was no glandular enlargement, nor disturbance of general health, and no history of syphilis. Preliminary examination of the fluids, done hurriedly, revealed nothing which would help the diagnosis. The fluid from one of the lesions was glairy; no fungus was found. Owing to the patient having refused a further examination and insisting on leaving the hospital, a further report on this case is unfortunately impossible.

Case of Tropho-neurotic Separation of Nails, followed by Alopecia Areata.

By F. PARKES WEBER, M.D.

THE patient, a rather delicate-looking married woman, aged 37, was shown at the meeting of the Dermatological Section, June 15, 1911.[1] About three months previously a skin eruption had appeared on her hands and fingers, and deep grooves like exaggerated "Beau's lines" had formed. Somewhat later the toe-nails were affected similarly, but to a lesser degree. Under mild arsenical treatment the abnormal condition of the nails disappeared; but about five weeks ago alopecia areata of the back of the hairy scalp commenced to be noticed. The arsenical treatment, which was discontinued in August, 1911, is to be tried again.

Dr. DORE said he had a case of simple separation of the nails, and Dubreuilh published a series of cases which he attributed to a tropho-neurosis. In those cases and in his own case there was no alopecia; in the latter the condition cleared up under arsenical treatment.

[1] *Proceedings*, 1911, iv, p. 137.

Case for Diagnosis.

By A. Whitfield, M.D.

The patient was a girl, aged 18, who complained of loss of hair and an eruption. The trouble had been in existence for two months, and had begun after the girl had taken a course of "Antipon" to reduce her weight. The scalp was nearly bald, and the skin there very harsh, but not reddened or atrophic. The face showed a generalized diffuse red congestion, affecting the cheeks, forehead, ears and root of nose, leaving the chin and circumoral region free. There was practically no infiltration present, no atrophy, and only a very slight surface scaling.

The arms and chest were the seat of a very intense perifollicular congestion, with slight infiltration. The resulting eruption was a red punctate rash, forming, especially on the arms, a reticulate pattern. Here and there he fancied he saw a tendency to atrophy, but it was not definite. On the backs of the phalanges the congestion of the follicles was very striking, and there were some black points, similar to those seen in pityriasis rubra pilaris, but there were no dome-shaped papules, and Dr. Whitfield thought that the black points were accidental, and only due to a slight secondary hyperkeratosis. There was no rash elsewhere. The eruption had some resemblance to that of acute lupus erythematosus, to parakeratosis variegata, and to pityriasis rubra pilaris. He confessed himself puzzled, but was inclined to the view that it was a persistent erythema belonging to the class of lupus erythematosus. In this connexion the taking of the "Antipon" was of interest. Dr. Whitfield had examined the blood in many cases of lupus erythematosus, and had invariably found the coagulation time long and the lime salts low. Antipon was said to be a solution of citric acid, and as this drug slowed the coagulation and reduced the lime salts it was just possible that the nostrum was responsible for the outbreak in this case. Following this line of reasoning Dr. Whitfield had ordered her calcium lactate, on which she had improved slightly.

Dermatological Section.

May 16, 1912.

Sir MALCOLM MORRIS, K.C.V.O., President of the Section, in the Chair.

Case of Persistent Circinate Papulo-necrotic Eruption (? Tuberculide).

By H. G. ADAMSON, M.D.

THE patient, E. L., aged 1 year 7 months, was in St. Bartholomew's Hospital under the care of Dr. Herringham, who had kindly allowed him to exhibit the case. The eruption occupied the forearms, hands and fingers, and the legs below the knees and the feet and toes, with a lesion also upon the right cheek and another upon the margin of the right ear. In consisted of papules, which, while expanding into rings, necrosed, and left behind them punched-out scars. The mother said that the eruption had been present on and off since birth, when it had been noticed as "spots" on the fingers and toes. The child was well nourished, but there were evidences of "chilblain circulation" in the cold and dusky hands and feet, and in a marked livedo annularis on the limbs. The case had been under observation for five months, and during that time there had been successive crops of the eruption. At present there was upon both forearms and hands and on both legs and feet an eruption in various stages of development: (1) A few hemp-seed-sized, reddish-brown, firm papules; (2) many split-pea-sized papules, with black crusts over their central part; (3) rings of the size of a threepenny-piece up to that of a florin, made up of a narrow, red-brown, slightly raised, infiltrated margin, enclosing an area of scar tissue; and (4) many purplish, punched-out, vacciniform scars, marking the sites of former lesions. By watching a papule during its evolution it was found that it passed through these various stages in about two weeks. On the right cheek there was a patch of the size of a shilling, which had a raised, nodular margin and a slightly atrophic centre. There were two small crusted nodules on the margin of the right ear.

In addition to the eruption described, there had appeared on the hands and feet from time to time dusky purplish, puffy swellings somewhat resembling chilblains. Sometimes one or more fingers or toes were entirely involved by the dusky, puffy swelling. While these swellings were present the general duskiness of the extremities was more marked.

A von Pirquet test was negative, but after injection of 0·0002 c.c., 0·0005 c.c. and, finally, 0·001 c.c. of old tuberculin there had been a general reaction with rise of temperature to 101·2° F. The Wassermann reaction was negative.

There were four other children in the family, three of whom were well, but the fourth (a boy, aged 4½) had had the same sort of eruption and dusky hands and feet since a few months of age. The eruption had been seen by the exhibitor, and it was identical with that of the child now shown.

The clinical features of the case suggested a tuberculide of the papulo-necrotic nodular type, but there were two unusual circumstances—the eruption had been present since birth, and the lesions had a marked tendency to spread into rings.

Microscopical examination had not helped the diagnosis. Although there were cell infiltrations mainly around the sweat-ducts and glands —as is often the case in tuberculides—there were no giant cells nor "epithelioid" cells, the cell infiltration being made up of fibroblasts (mononuclear round cells) suggestive, perhaps, of a toxic action, but not characteristic of a tuberculide.

Case of (?) Tuberculide of the Type called "Sarcoid" of Darier-Roussy.

By H. G. Adamson, M.D.

The patient was a woman, A. A., aged 35. Six weeks ago she had noticed a spot of the size of a pin's head behind the ankle on the outer side of the left foot. This had increased in size and others had formed, and there was now a group of eight or nine pea-sized nodules occupying an area of about 1½ in. by 2 in. The nodules, though deep-seated, seemed to be in the skin and not subcutaneous. Some were dusky red, but the skin was nowhere broken. The whole area in which they were set was slightly red and infiltrated. There was no tenderness. On the

dorsum of the right foot were two nodules, very hard, deep-seated, and giving the impression of "rheumatic nodules." The patient had a phlyctenular conjunctivitis of three weeks' duration. There was a long scar in the neck where glands had been removed at 16 years of age. A nodule was excised for microscopical examination, but the sections had shown little if any change beyond a very slight cell exudation around the blood-vessels of the corium. Clinically the case seemed to be a "tuberculide" of the type of Darier's "sarcoid hypodermique," but the microscopical examination seemed to negative this.

Case of Lupus Erythematosus.

By J. L. BUNCH, M.D.

THE patient was a nurse, aged 23. Some nine months ago a few small, slightly scaly patches had made their appearance on the backs of the hands, which latter were inclined to a purplish tinge and had a somewhat chilblain appearance. These scaly lesions were originally never larger than a pea, but, as they increased in number, some of them tended to coalesce and thus formed patches sometimes as large as a hazel-nut. The edges of these lesions were fairly sharply defined and slightly thickened, but their chief characteristic was that they went on to atrophy. At the present moment there were numerous small atrophic scars on the dorsa of the hands and fingers, marking the site of old lupus erythematosus lesions, in addition to more recent active foci of disease.

A few of these recent spots had shown a yellowish centre, and the question arose whether these pointed to a secondary staphylococcic infection or to a co-existing folliclis.

Some weeks ago an erythematous, slightly scaly patch had made its appearance on the left cheek, and when shown this was about ¾ in. in its longest diameter, with a somewhat purplish tint in the centre and a pink or reddish edge. This patch showed up to the present no signs of atrophic change.

A week previously a von Pirquet reaction had been done, but this had proved negative. No Calmette reaction had been tried and no injection of tuberculin given.

The PRESIDENT (Sir Malcolm Morris, K.C.V.O.) said he did not doubt that it was an erythematous lupus process in each area. About a year ago he

showed the case of a doctor's dispenser with exactly the same condition on the hands; she had since enormously improved, but was not yet well. Many of the patches on the hands had atrophied as had happened in this case. They were equally localized. To improve the circulation he used a constant current, which often made considerable difference. Such conditions as this, practically on the border-line between two diseases, were always interesting.

Case of Generalized Ichthyosis in a Girl, aged 14.

By J. L. Bunch, M.D.

Soon after birth it was noticed that the skin was harsh and dry, and this condition of the epidermis gradually became more marked until the whole of the trunk, limbs and face became rough, scaly, and markedly wanting in elasticity. When first seen, a month previously, the face and scalp were scaly and dry, the hair was wanting in lustre and harsh to the touch, the trunk and extensor surfaces of the limbs were covered with coarse, irregularly shaped scales, especially well marked on the backs of the wrists, the knees, and the fronts of the ankles. The scales were of a dirty greyish colour and had an unwashed appearance; the natural lines of the skin were exaggerated and the affected areas left dry and harsh. The plate-like character of the scales was suggestive in places of the so-called "alligator skin." The anterior margins of the axillæ and the outer surfaces of both thighs were marked by the presence of warty, thickened, papillary elevations of a dark grey tint, some of which had a spine-like appearance, which strongly suggested a diagnosis of ichthyosis hystrix, and the case was shown as possibly one of the "mixed variety," combining some of the characteristics both of ichthyosis simplex and ichthyosis hystrix.

The history seemed to show that the lesions tended to improve in warmer weather, and in summer the papillary elevations were stated to diminish, if not disappear.

DISCUSSION.

The President said that much good was derivable also from prolonged sulphur baths.

Dr. MacLeod said he had employed thyroid extract with distinct benefit in several cases of generalized ichthyosis at the Victoria Hospital for Children, especially when it was pushed to the point of toleration. The local application which he had found of most use was a drachm of resorcin and an ounce of glycerine and starch.

Case for Diagnosis.

By S. E. DORE, M.D.

THE patient, a woman, aged 34, presented two symmetrical, raised, brownish-red, gelatinous-looking, oval swellings about half an inch in length over the nasal bones on each side, which had persisted without change for eight months. In addition to these there were small symmetrical patches consisting of an aggregation of yellowish papules like those of lupus vulgaris, but disappearing on pressure on the ears just above the tragus and a few inside the concha. Scattered over the upper part of the front of the chest were a large number of flat, shiny, yellowish papules or irregular-shaped patches. There was no infiltration or redness, and these lesions had remained stationary for five months. The patient gave a history of an attack of "itching eczema" all over the body four years previously. She had had five healthy children, one miscarriage between the first and second, and her third child died at the age of 5 months, but there was no evidence suggesting a diagnosis of syphilis, and the Wassermann reaction was negative.

The opinion of several of the members present was that the case was an unusual one of lupus erythematosus.

Tuberculide in a Boy, aged 10.

By S. E. DORE, M.D.

THE lesions began five months ago on the buttocks, where there was a characteristic eruption of acne scrofulosorum. On the legs there were large circular, scaly, infiltrated patches of various sizes. The boy appeared to be in good health and had no enlarged glands or other evidence of tuberculosis. An uncle was said to have tuberculous glands, but he was the only relative known to be affected. The boy was brought forward as a good example of a mixed tuberculide and also for suggestions as to treatment.

Hydroa Vacciniforme vel Æstivale in a Boy, aged 8.

By S. E. Dore, M.D.

The boy presented a vesico-bullous scar-leaving eruption on the face, especially on the nose, cheeks and forehead (fig. 1), on the rims of the ears and dorsal surfaces of the hands (fig. 2). The eruption had occurred every year for the past four years, and persisted from the month of March to September each year. It came out in crops, each crop lasting about a month or five weeks, and directly followed exposure to sun or wind. He had never had an attack in the winter. The lesions began as small vesicles, which enlarged and dried up in the centre, leaving a ring-shaped vesicle at the margin resembling that of vaccinia. Some of the vesicles showed umbilication, some of them were loculated, while others had become pustular or dried up to form brown crusts. Cultures from the vesicles gave a pure growth of *Staphylococcus aureus*.

The boy appeared in other respects to be in excellent health, and was the only member of the family affected in this way. There was no reducing substance or visible pigment in the urine, but the spectroscopic test for hæmatoporphyrin had not been employed.

The President said it was puzzling to account for the involvement of the mucous membrane of the mouth on the idea that it was due to exposure to the cold winds in the spring.

Case of Pseudo-pelade.

By Wilfrid Fox, M.D.

The patient was a young woman, aged 21, who had suffered from the condition for the last four years. She showed one of those scarring alopecias which was certainly neither lupus erythematosus nor acne decalvans, and appeared to be of the type referred to by Brocq under the name of "pseudo-pelade." The areas of alopecia were irregular in shape, mostly about the size of a postage stamp, and situated chiefly in the mid-frontal region, though some of the more recent ones were spreading on to the parietals. They were dead white in colour, shiny, slightly depressed, and with complete atrophy of all the hair-follicles: in some places they ran into one another, in others a few straggly hairs



FIG. 1.
Case of hydroa vacciniforme vel æstivale.



FIG. 2.

separated the areas. During the last four months there had been no fresh foci appearing, but the patient said the areas were in the first place tender and slightly pink in colour, the pink coloration being diffuse over the whole area and not due to individual folliculitis. There were no injected venules present in any of them.

The exhibitor mentioned that he had recently had another case of this same disease in a man who had spent most of his life in exploring in Africa, and this patient put it down to the effects of sunburn.

Case of Eruption due to Ferrous Iodide.

By E. W. Goodall, M.D.

J. W., aged 23, was admitted to the Poplar and Stepney Sick Asylum on February 7, 1912, for mitral regurgitation and nephritis. There were œdema of the lower extremities and signs of heart failure. The nephritis continued up to March 26. On April 3 the patient suffered from vomiting and headache. On April 13 a diastolic murmur appeared, and as previously only a systolic had been present it was thought that malignant endocarditis was setting in.

On the evening of April 12 papules began to come out on the face. Within twenty-four hours they were vesicular. The eruption continued to come out during the next four days. Dr. Goodall saw the patient on April 16. The face and neck to the extent shown in the photograph were covered with a vesiculo-pustular eruption, confluent about the nose, lips and adjacent parts of the cheeks, discrete elsewhere. The colour of the vesicles was a pale yellow, like that of small-pox in the late vesicular stage. On pricking the vesicles a whitish, thick fluid escaped. The discrete vesicles were monolocular and there was no umbilication. Some of the lesions about the nose and lips were beginning to scab. The eruption was painful, especially when touched. There was a good deal of swelling of the eyelids, nose and lips. There were no lesions on the tongue, palate, or buccal mucous membrane. The lesions were very superficial, so that on rolling them between the finger and thumb there seemed to be little or no infiltration of the skin.

On the shoulders and upper part of the back were a number of papules. Most of them were ordinary acne pimples with a central black spot, but there were a few vesicles. There were a few vesiculo-papules on the chest and one or two about each wrist, but only one of

the latter was of any size. It was situated on the inner side of the forearm just above the wrist, and had much the appearance of a small-pox papule just becoming vesicular. There were also several similar lesions on the scrotum, penis, and adjacent portions of the thighs. There were none anywhere else on the lower extremities.

The aspect of the eruption had raised a suspicion of small-pox, and Dr. Goodall was not surprised to hear this when he saw the patient on April 16. The photograph was taken the next day, and by that time a good deal more scabbing had taken place. The eruption gave off an offensive odour. There were four good vaccination marks on the left upper arm, the result of vaccination in 1902.



Eruption due to ferrous iodide.

There had been a little irregular pyrexia from April 1 to April 7, and the temperature again rose while the eruption was coming out, and reached its highest point, 103° F., on the evening of April 13. Then it gradually fell till the death of the patient, which took place on April 18, from cardiac failure. He had for two or three days previously been dull and apathetic.

The only cause Dr. Goodall could find to account for the rash was ferrous iodide; the patient had taken three doses of the syrup of the iodide of iron on each of the two days April 11 and April 12—six doses in all. He therefore took altogether 32·7 gr. of ferrous iodide. He had never, so far as is known, taken iodides before.

Dr. Goodall thought that this case was worthy of record because

it gave rise to the suspicion that it was one of small-pox. Rather more than six years ago he saw a similar case, in which the diagnosis of small-pox was actually made and the patient sent to the small-pox hospital. She was a woman, aged 32, who was the subject of chronic heart disease and right hemiplegia. The eruption came out suddenly after she had taken about 60 gr. of iodide of potassium in 10-gr. doses. Up to eight days previously she had for several days been taking 3-gr. doses of the same drug, but had not shown any signs of iodism.

For the opportunity of seeing and the permission to record the present case Dr. Goodall is indebted to Dr. Charles Spurrell, the Medical Superintendent of the Sick Asylum.

Case for Diagnosis (Ferrous Iodide Rash).

By Sir MALCOLM MORRIS, K.C.V.O., F.R.C.S.Ed.

THE patient had an eruption on the face and legs, and the duration had been three months. There was nothing abnormal to be seen in any other part of the body.

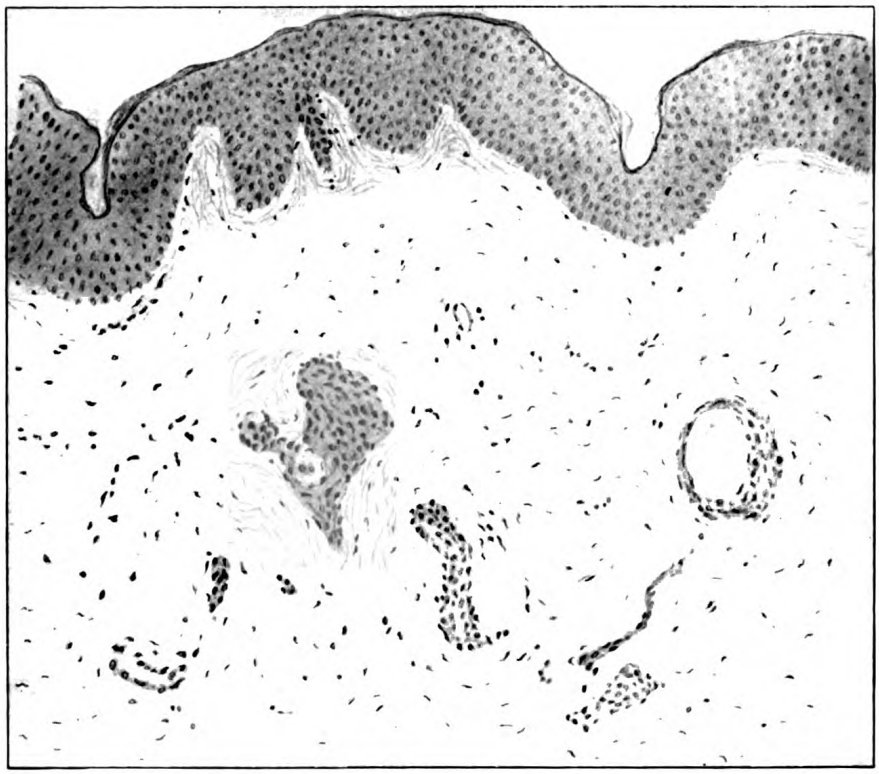
The PRESIDENT said he was not very familiar with these cases of eruption due to iodide of iron. His own case looked so very much like one of specific eruption that at first he was inclined to be deceived. The patient was given one tea-spoonful of syrup of iodide of iron three times a day, and in a week the eruption appeared. The medicine was stopped and on its resumption the eruption came out again. Though iodide of iron must be given very commonly, such an eruption was, in his experience, rare. The Section was much indebted to Dr. Goodall for bringing such a case, as these cases were instructive, and if he had any others of interest the President hoped he would bring them.

Case of Syringoma.

By J. E. R. MCDONAGH, F.R.C.S.

A GIRL, aged 12, came up to hospital complaining of some spots on her chest. According to the mother's history the spots appeared when the child was 1 year old, no other member of the family being affected. The lesions were very faint yellowish-white, raised above the surface of the skin, and varied in size from that of a pin's head to that of a lentil. The lesions were smooth on the surface, skin not movable over them.

They were sharply circumscribed, although the bigger ones were not invariably regular in outline. The lesions gave rise to no subjective symptoms. The area most affected was the chest over the sternum and beneath the clavicles, and in these situations the biggest lesions were to be found. The neck was also involved, lower eyelid on left side, lateral walls of thorax and abdomen and inner sides of thighs. There were none on the back or arms.



Case of Syringoma. Drawing of microscopical section of new growth. (× 120.)

Histology: The epidermis over the new growth, which was situated in the upper portion of the corium, was unaltered. The corium itself was unchanged except for the presence of the epithelial elements from which the new growth arose, and also for a slight increase in the fixed connective tissue cells. The new growth consisted of the following structures:—

(1) Solid cords and nests of epithelial cells.

(2) Cords and nests of epithelial cells which were hollowed out in the centre, the walls of the former being made up of two layers of

epithelial cells, the latter of several layers with the central cells degenerated and staining badly. The hollow cords resembled in every way the normal sweat-ducts.

(3) Small cystic spaces with a colloid content. The walls were made up of one or two layers of epithelial cells; in parts the cells were indistinct and the nuclei when present were elongated. The colloid matter was most marked in those cysts the cells in the walls of which had degenerated, and the homogeneous substance in the centre was probably a degeneration product of those cells.

Case of Maculo-anæsthetic Leprosy in a Woman, aged 25.

By J. M. H. MacLeod, M.D.

The patient was a Creole from the West Indies. Her father was an Irishman and her mother a native of the West Indies. She was the youngest of three children, having a brother and sister who were both married and had healthy children. Her father died when she was young, the cause of his death being unknown to her. Her mother also suffers from maculo-anæsthetic leprosy.

The patient is a well-nourished young woman of medium height and rather stout. The disease was first noticed when she was aged 19, as a reddish patch about the size of a shilling, not unlike ringworm, on the back of the right wrist. This gradually spread, taking a year to reach the size of half-a-crown, when it began to assume the characteristics of of a maculo-anæsthetic patch of leprosy. She was then put under treatment for the disease. In 1909 she came to England, and since then has been under the observation and treatment of the exhibitor. When she was first seen in 1909 in London the following evidences of the disease were present: Extending across the back of the right wrist there was a band of discoloured skin, which varied in width from 1 in. to 2 in. It was reddish-purple in tinge in the centre and brownish at the margins, the border being slightly raised. The affected skin was slightly atrophic, but the lanugo hairs were still present upon it. A smaller patch about the size of a shilling was present on the back of the hand; this was similar in colour and texture to the larger patch. These lesions were definitely anæsthetic, insensitive to pain from the prick of a needle, insensitive to heat and cold, but slightly sensitive to touch. The skin beyond the patches was slightly hyperæsthetic. There was also a small patch on the front of the chest. In addition to the patches on the skin

there was slight wasting of the interosseous muscle between the fourth and fifth metacarpal bones. There was no definite thickening of the ulnar nerve. The patient complained, however, of tingling and peculiar shooting pains along the nerve just as if it had been knocked at the elbow. These sensations were specially marked when she tried to lift anything. These were the only signs of the disease, and otherwise the patient seemed to be enjoying excellent health.

It was decided to treat the case by nastin injections. The injections of 1 c.c. of nastin B1 were begun in March, 1909, and were given subcutaneously into the thighs. There was no definite pain associated with the injection, and there was neither a local nor a general reaction. The treatment was continued for five months, but in spite of it the patches appeared to be increasing in size and the patient complained of numbness of the little finger, which was a new symptom. Consequently, injections of nastin B2 were substituted, but with no more definitely beneficial results. The nastin treatment was continued until July, 1911, and as there had been no marked change for the better it was decided to replace it by injections of a leprolin, prepared by Dr. Bayon from a streptothrix which he had isolated from another case, and which he believed to be the specific micro-organism of the disease. These injections have been continued intermittently since then. Only in one or two instances has there been any reaction from them, and then it was simply in the nature of a slight febrile disturbance, which took place about eight hours after they were given. Recently the disease has remained stationary, and the patches have faded to such an extent that it is difficult to detect them on casual observation.

The case was shown on account of the difficulties which may be met with in the diagnosis of early maculo-anæsthetic lesions, and also from the interest connected with the fact that the patient, while under nastin treatment, remained *in statu quo*, but since Bayon's leprolin has been injected has shown decided improvement. In a disease, however, which is prone to exacerbations and remissions apart from treatment, it is impossible to come to any conclusion from an isolated case, with regard to the merits of any individual form of treatment.

DISCUSSION.

The PRESIDENT said that in one case there was a very great increase of pain from the administration of the nastin. He did not know whether members had tried tuberculin in an anæsthetic case of leprosy. When tuberculin first came in he had a case of anæsthetic lepra in his ward and gave the patient

some tuberculin; after the first injection there was a violent reaction, with large bullæ all over the arms—a very severe attack, such as he had never had before. It had been anæsthesia with macular patches, and nothing else. No further dose was given, and the condition quieted down. Since that time the patient had been living quietly in the country, and the disease had not progressed.

Dr. COLCOTT FOX said he could confirm what the President said by an observation made when Koch's first tuberculin was introduced. By the kindness of the Medical Superintendent of the Greenwich Infirmary, he presented at one of Sir Watson Cheyne's demonstrations an old woman who had suffered from anæsthetic leprosy for many years and in whom the disease appeared quiescent, and only some dystrophies of the extremities were noticeable. She reacted to tuberculin, and a large circinate erythematous eruption evolved, which was apparently of a leprous nature.

Case of Lupus Vulgaris and Scrofulodermia treated by the Pfannenstill Method.

By J. H. SEQUEIRA, M.D.

THE patient was a poorly developed girl, aged 16. Her father was healthy, but her mother suffered from phthisis. The girl has always been delicate. She had pneumonia when young, and had had measles fifteen months ago. The lupus started upon the back of each wrist eight years ago, and the lesions had been scraped at another hospital. They gradually spread after the operation, and eventually reached the elbow and the upper arm. A little later the face was affected. Fifteen months ago she had what was described as being a tuberculous abscess behind the left ear. This was opened. Her hearing became affected, and she was very deaf. Her voice also was husky.

She came to the London Hospital in January, 1912, with extensive lupus of the nasal cavity and of the pharynx and palate. There was also double epiphora. There was extensive lupus, with some central scarring upon the hands and forearms up to the elbow. In February, 1912, the back of the right hand became much swollen and œdematous and a localized purplish-red swelling developed over the back of the right wrist. This was 2 in. in its long diameter and 1½ in. transversely. The swelling evidently fluctuated, and it was opened, and a thin yellowish pus was evacuated. Later, a small oval swelling formed along the line of the lymphatics.

On February 21 she was admitted to the ward, and the arm was

put on a splint. The ulcerated areas in the nose were treated by packing with gauze, which was kept constantly moistened with a solution of peroxide of hydrogen 10 vols. At the same time she was given 30 gr. of sodium iodide *per diem* in divided doses. The tampons soaked with the peroxide were kept constantly in position, except when the patient was asleep. Finding so much improvement of the intra-nasal condition in this as in numerous other cases treated by the Pfannenstill method, the exhibitor had the ulcerated areas upon the forearm and wrist dressed in similar fashion, with great benefit. The case was specially shown to illustrate the value of the method, particularly in the treatment of ulcerative lupus of the cavities. The patient has borne the iodide very well, and to make sure that the condition was not complicated by congenital syphilis a Wassermann reaction was done, with negative result. The patient has put on 7 lb. while in hospital, and her general health has improved greatly.

Dr. PERNET said that last year in Stockholm, in the late Professor Möller's clinic, he had seen the Pfannenstill method employed in lupus vulgaris of nasal cavities. The treatment was giving satisfactory results.

Case of Erythema Nodosum, associated with Mammary Tuberculosis.

By F. PARKES WEBER, M.D.

THE patient was a rather delicately built woman, aged 47, who had previously had no serious illnesses, and had apparently enjoyed good health. She was admitted into the German Hospital on April 6, 1912, suffering from a chronic swelling in the right breast of at least two months' duration, which had not caused her much pain. This was excised by Dr. E. Michels on April 9 and was found to be tuberculous, the microscopical sections containing many characteristic giant cells of the tuberculous type. On admission she was likewise suffering from florid erythema nodosum affecting both legs up to just above the knees, and there was a little pyrexia. The erythema nodosum, which had commenced about March 30, almost completely disappeared by the end of April. The patient had not been taking any drug, such as potassium iodide, which might be supposed to be connected with the onset of the erythema nodosum. There was no history of syphilis. She had had twelve children, nine of whom were still living; no miscarriages. A good deal of attention had been given, especially on the Continent,

to the occasional association of erythema nodosum with tuberculosis,[1] a connexion which had been recognized and described by Landouzy in 1907.[2]

DISCUSSION.

Dr. WHITFIELD remarked that erythema nodosum in association with tubercle was said to be of very fatal omen. In some cases it was associated with severe tuberculous adenitis and pulmonary tuberculosis, but he had seen such cases recover, and he doubted the gravity of the significance.

Dr. SEQUEIRA remarked that erythema nodosum had been known to follow injections of tuberculin, and recalled two cases in which lesions of this type had followed injections of the bacillary emulsion for articular and gland tuberculosis.

Case of Ichthyosis.

By A. WINKELRIED WILLIAMS, M.B.

THIS case showed certain unusual features, which made the diagnosis uncertain. A boy, aged 3½. General health is good. No family history of ichthyosis. Present condition: Trunk, especially the back, shows marked ichthyosis (serpentina type); scalp crusted and covered with scales, which encroach half an inch over forehead and end abruptly; skin of face most exceptionally smooth and soft, in fact is of a notably beautiful complexion; extremities partially covered with scales with areas of smooth skin. The child was born with a "parchment skin," and has never been perfectly free from the ichthyotic condition, which sometimes involves the whole face, despite its present perfect freedom from the trouble; at quite irregular periods the scaly skin clears up (often spontaneously), leaving a perfectly smooth, soft skin. The smooth skin later becomes scaly again, without any erythema or febrile symptoms. The clearing up by a peeling process is aided by ordinary remedies for ichthyosis, such as resorcin and glycerine of starch, &c. The condition is always much worse in the summer.

[1] *See* A. B. Marfan, *Presse Méd.*, Par., 1909, xvii, p. 457; A. Sézary, *Gaz. des Hôp.*, Par., 1912, lxxxv, p. 125; F. S. Meara and M. Goodridge, *Amer. Journ. Med. Sci.*, Philad., 1912, cxliii, p. 393; W. Hildebrandt, *Münch. med. Wochenschr.*, 1907, liv, p. 310; A. Chauffard et J. Troisier, "Erythème noueux expérimental par injection intradermique de tuberculine," *Bull. de la Soc. méd. des Hôp. de Par.*, 1909, xxvii, pp. 7, 772; G. Thibierge et P. Gastinel, "Reproduction expérimentale de certaines dermatoses par l'injection intradermique de tuberculine," *ibid.*, 1909, xxvii, p. 757.

[2] L. Landouzy, *Assoc. Française pour l'avancement des Sci.*, Reims, 1907, xxxvi, p. 337.

Dermatological Section.

June 20, 1912.

Sir MALCOLM MORRIS, K.C.V.O., President of the Section, in the Chair.

Case of Circumscribed Lichenification (névrodermite).

By S. E. DORE, M.D.

THE patient, aged 40, presented a patch of circumscribed lichenification in the right lumbar region which had existed for three years. The plaque showed the characteristic aggregation of glistening round papules, with a certain amount of thickening and slight pigmentation of the skin, but the three zones described by Brocq could not be differentiated. During the past four months the patient had had similar patches on the arms, thighs and legs, but these had recently disappeared. There was a generalized prominence of the follicles, especially on the legs, and the hairs were rubbed away by the constant scratching. According to the patient's statement, the pruritus preceded the formation of the skin lesions.

Acquired Syphilis in a Girl, aged 10.

By S. E. DORE, M.D.

THE child presented a grouped follicular syphilide of fourteen days' duration on the trunk, thighs, upper arms and flexors of elbows. The lesions were most marked on the upper and inner aspects of the thighs, where they had recently appeared; on the trunk the eruption had begun to fade and closely resembled lichen scrofulosorum. There was also a typical syphilitic leucoderma of the neck. The child had a chancre of the vulva, but the first thing noticed by the mother was an ulcer on the dorsal surface on the tongue, which appeared eight weeks

previously. The fauces were also ulcerated and there were condylomata around the anus. The present eruption was preceded by a characteristic papulo-squamous syphilide six weeks ago. A younger sister, aged 5, had condylomata of the anus and lesions in the mouth one month before the present case was affected. Both children gave a positive Wassermann's reaction and spirochætes were found in the lesions.

Case of Multiple Scars on the Body and Limbs, and Partial Destruction of the Ears.

By WILLMOTT EVANS, F.R.C.S.

THE patient was a boy, aged 9. He had been in good health until he was a little more than 2 years old, and then small ulcerations began to appear; these were for the most part on the limbs, but some also occurred on the trunk. The ulcers were almost exactly symmetrical, and most of them were small, less than $\frac{1}{4}$ in. in diameter, but those on the trunk were larger, reaching nearly an inch in size. About a year ago ulceration began on the ears, at the upper part of the helix, and it had steadily progressed.

At the present time the child has no ulceration, but he shows a large number of small pale scars, slightly sunken, on the wrists and ankles especially, but on other parts of the limbs also. On the calf the scars are large, and the same is true of the scars on the loins. The upper portion of each ear has been destroyed to the extent of about a quarter of the pinna, but the surface is soundly healed. Mr. Evans considered that the condition of the ears was not unlike the effect of Raynaud's disease, but that would not account for the scars on the limbs; these reminded him rather of dermatitis gangrænosa infantum, but the symmetry and the situation of the scars were against this diagnosis. The case was under the care of Dr. Harrington Sainsbury, and as it had been seen only the day before, there had been no time to apply a von Pirquet or Wassermann test.

Dr. SEQUEIRA said his opinion was that the lesions were necrosing tuberculides.

Case of (?) Potassium Iodide Eruption.

By A. M. H. Gray, M.D.

The patient, a male, aged 40, suffering from pulmonary tuberculosis, developed a "lump" below the left angle of the jaw in September, 1911, and another below the chin in February, 1912, both of which were "opened" and matter escaped, but the wounds did not heal, and the skin around them became inflamed. He also noticed some "soreness" of the left leg which he thinks commenced at the end of April, but can give no definite account of the character of the lesion present. As the sinuses in the neck did not heal, during May, 1912, he took two bottles of Clarke's blood mixture (each estimated to contain about 52 gr. of potassium iodide), and soon after commencing this mixture further lesions developed on the face, the left leg became much worse, and spots appeared on the right leg.

When first seen on May 30 (he had stopped taking the mixture about one week previously) he was looking thin and ill; he had marked œdema of both lower limbs and abdominal wall and some free fluid in the abdomen. His temperature was raised between 100° and 101° F.; he had dullness, weak breath sounds, and râles at the apex of his right lung, and was spitting up a considerable quantity of thick purulent sputum. His urine was diminished in amount and contained a large quantity of albumin.

Below the left angle of the jaw and beneath the chin were two sinuses freely discharging pus, and the skin round them had the characteristic appearance of scrofulodermia. Three raised tumours, each about the size of a thumb-nail, were also present, one on each side of the chin and the third behind the sinus on the left side; each was sharply raised from the surface of the skin and surrounded by a red areola. On the surface of each was a greenish crust, which, when removed, left a red irregular surface from which small points of pus could be squeezed.

On the left leg below its centre was a pale red patch measuring 10 cm. by 7 cm., which was made up of closely set nodules, each about the size of a threepenny-piece. These nodules had a smooth, flat surface and were only very slightly raised; some were firm to the touch, others showed signs of softening, and one or two had ulcerated.

A few scattered lesions of a similar nature were scattered round the patch and a few discrete lesions were also present on the right leg. In addition a few lesions of a different nature were present on both legs; these consisted of red papules the size of a pea surrounded by a paler areola and surrounding a hair-follicle; in some of these a superficial vesicle or pustule had formed.

During a fortnight's stay in hospital practically all the nodular lesions had broken down and given discharge to a thick brown-coloured material, cultures from which gave a mixed growth of *Staphylococcus aureus* and *albus*. The Wassermann reaction was negative. No local treatment had been adopted except to the scrofulodermatous areas, which had been dressed with weak mercurial dressings, and the rash had been gradually clearing up.

The case was shown to obtain an opinion on the nature of the nodular lesions, as the exhibitor had not seen a description of this type of lesion in potassium iodide dermatitis; the tumour-like lesions on the face and the vesico-papular lesions on the legs appeared to be well-recognized types.

Dr. WHITFIELD said that when the man first came he thought it was possibly a case of glanders by the general aspect. He did not now doubt that it was tuberculous.

Case of (?) Erythema Multiforme.

By E. G. GRAHAM LITTLE, M.D.

THE case was shown for the second time, having been brought before the Section in February last. The condition had persisted very much as it had then shown itself, and the report then presented[1] was still accurate. Seeing Dr. Pringle's case, shown at the present meeting, he was struck with the resemblance in configuration of the patches which both these cases showed, but in Dr. Pringle's case there was apparently no vesication, which was a marked feature in this patient. If the diagnosis of erythema multiforme was accepted, it was certainly very remarkable that the individual lesions had persisted as these had done, certainly for some months, and that the disease had been constantly present, or nearly so, for three and a half years.

[1] *Proceedings*, p. 92.

DISCUSSION.

Dr. PERNET said he still considered the case was one of lupus erythematosus. When the patient was first shown the iris aspect was characteristic, and he then classed it as lupus erythematosus iris of French writers. There was not always atrophic scarring in lupus erythematosus in its early stages. To his eye and touch the present condition did not suggest erythema multiforme, but lupus erythematosus of an unusual type. The latter disease varied greatly in its appearances. Atypical instances needed to be considered. He took the present case on broad lines. He hoped the exhibitor would be able to keep the patient under observation and show her again from time to time.

Dr. WHITFIELD pointed out that there was no sign of scarring left by the disease, although she had had it for a long time. In a case like this that was neither infiltrated nor atrophic, he did not think that the diagnosis of lupus erythematosus was justifiable. Moreover, this disease left a considerable amount of pigmentation, which was not common in lupus erythematosus but quite frequent after erythema multiforme.

Case of Xanthoma Tuberosum Multiplex associated with Peculiar Osteo-arthritic Changes in the Joints.

By J. M. H. MACLEOD, M.D.

THE patient was a man, aged 42. He had demonstrated the case previously at the Society in June, 1910.[1] Since then he had seen the patient at irregular intervals, and his condition had not materially changed since he was first exhibited. The disease appeared to have made definite progress; his general health, however, had remained good except for occasional attacks of bronchitis; there was still no marked swelling of the liver or jaundice, and the urine did not contain sugar, albumin, or excess of urobilin. The eruption was more extensive and the swellings somewhat larger.

Since he was first shown a microscopical examination had been made, and as a result of the study of a considerable number of sections stained in different ways, the exhibitor had come to the conclusion that the earliest changes took place around the blood capillaries about the subpapillary layer and beneath it, and consisted of a cellular proliferation and an infiltration of the lymph spaces between the fibrous bundles in the neighbourhood with a fatty substance which is easily stained with

[1] *Proceedings*, 1910, iii, p. 108.

Sudan III, and readily breaks up into crystals of cholesterin and fatty nodules. This fatty substance is present in the cells around the blood-vessels as well as in those of the endothelium lining them. Owing to the presence of the fat certain of the cells became enlarged and the nuclei divided to form the characteristic xanthoma giant cells, with two or three nuclei to a dozen or more. The presence of the fatty substance appeared to act as an irritant which caused a proliferation of the fixed cells, with the production of fibroblasts which gradually became organized into new fibrous tissue. In the earlier stages the cellular elements predominated, while later the fibrous changes were the most marked feature in the histology. The most important part of the fatty material is known to be a lipoid named cholesterol-fatty-ester. This is believed to be originally in the blood, and has been found there in jaundice and in diabetes. It would seem to pass out of the blood capillaries of the skin so as to infiltrate the lymphatic spaces and the cells. It has been suggested that the reason why this affection occurs most readily about the joints is that there the blood-vessels are subjected to a special strain, owing to the movements of the joints, and more blood flows through them. An interesting paper on this subject will be found in the May issue of the *Journal of Cutaneous Diseases*, 1912, by Pollitzer and Wile.[1]

Case of Alopecia Areata et Totalis Cured by Pregnancy, and Relapsing with the Re-establishment of the Menses.

By G. Norman Meachen, M.D., and F. L. Provis, F.R.C.S.Ed.

The patient was a well-developed but somewhat pale woman, aged 31. She had been eight years married, having had four children and two miscarriages, one of which was a stillbirth. Her youngest child was 10 months old when seen at the Blackfriars Skin Hospital, whither she was sent from the Chelsea Hospital for Women. She had had no previous illnesses, except anæmia when a girl, and there was no history of premature or other variety of baldness in any of her relations. In the sixth month of her second pregnancy she first noticed her hair beginning to fall, and when the child was 5 months old she "hadn't a hair in her head." During the third pregnancy, however, it began to grow again, nearly all of it coming back. Then the stillbirth occurred, and when her

[1] *Journ. Cutan. Dis.*, New York, 1912, xxx, pp. 235-41.

periods returned the hair fell again, this time totally, other parts of the body besides the scalp being denuded. She became pregnant again, and from the first month of this pregnancy *the hair began to return*, and at this time, she states, she "had a splendid crop of hair, except two small patches." This was on June 2, 1911. When her baby was 5 months old the menses reappeared, *and once more the hair began to fall.* Her finger-nails became affected about the same time as the initial loss of hair. The special senses have, apparently, always been normal. All her children have been brought up on the breast, but not for an undue length of time. The bowels are habitually costive.

On March 26, 1912, the whole scalp was practically bare, except for a few scanty strands of rather dark, coarse hair. The eyebrows were completely absent and there were no eyelashes. The remaining parts of the body were not absolutely alopecic. All the finger-nails were lustreless, longitudinally striated, and presented numerous fine pits, especially upon the lunulæ. The toe-nails were similarly affected, though to a less extent. The integument of the scalp is freely movable upon the bone, and it is not cicatricial in any degree. At the nape of the neck are a few erythematous macules, which, she states, "have been there for a very long time." The patient has the characteristic facial expression commonly seen in cases of total alopecia. Several "point of exclamation" hairs were seen. These could be extracted with ease, and presented the usual appearances microscopically. The long black hairs that still remained broke off close to the scalp on pulling them, and did not come out by the root. The patient was attending the Chelsea Hospital for prolapsus uteri. At the time of the meeting the scalp was totally bald, and the other parts of the body were also denuded. The Wassermann reaction had not been taken. The patient had no signs of syphilis.

Instances of loss of hair during the puerperium as a result of the debilitating influence of parturition are, of course, well known, but that pregnancy and delivery should act as a tonic, as it were, to the growth of hair upon two successive occasions, alopecia supervening in between, appears to the exhibitors to be unique; at any rate, they have been unable to find records of a similar condition in dermatological or gynæcological litarature.

DISCUSSION.

Dr. PRINGLE suggested that the word "cured" was rather unfortunate in this case. He would prefer to say that she got well during pregnancy and was bad at other times.

Dr. SEQUEIRA said he had seen several cases in which there had been complete loss of hair after delivery. He had at the present time one woman under care who had completely lost her hair on three occasions. The hair grew between two pregnancies, but after the third the loss of hair was permanent. For some years he had been unable to restore the growth. He had under care another woman who had complete alopecia after delivery, affecting the whole of the body. Her hair was now coming back twelve months after delivery, but there were areas in which the hair had not grown.

Dr. LESLIE ROBERTS (Liverpool) said he had had a similar case in his experience recently. An unmarried lady was under his care some years ago with almost total alopecia areata of the scalp. In course of time she got married and a few months later became pregnant, and while pregnant all the hair came rapidly back. After twins were born she came to him, and though he examined the scalp all over with the lens, it would have been impossible to say that she had ever had alopecia. The hair was vigorous and strong and remained so for a few months, and then began to fall out again. He thought that the connexion between pregnancy and the recovery from alopecia was more than a mere coincidence, but what the connexion was he could not say.

Case of Ichthyosis with Late Development.

By Sir MALCOLM MORRIS, K.C.V.O., F.R.C.S.Ed.,
and S. E. DORE, M.D.

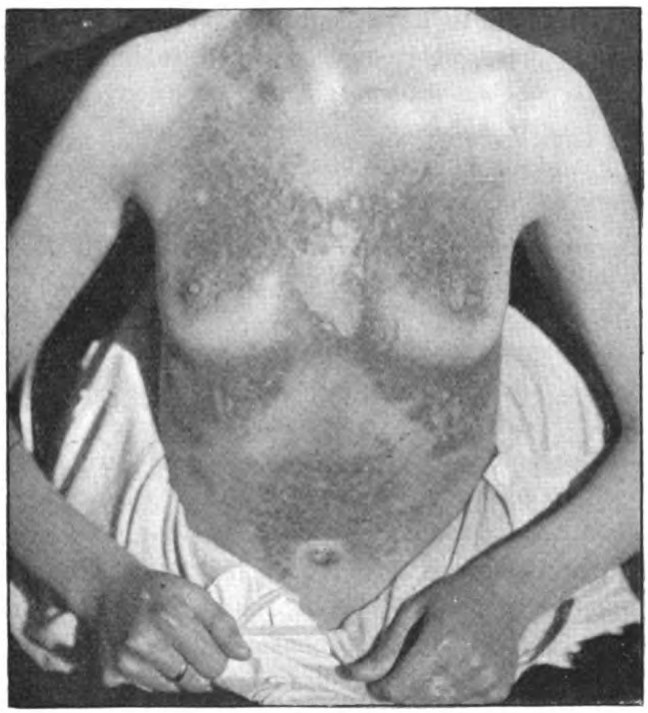
E. F., AGED 14. Family history: The father in good health but said to be highly nervous. Mother has been subject to attacks of eczema since the age of 14. She has always had a dry skin, especially of the hands and feet, and is also said to have had psoriasis. One sister, aged 4, is stated to be nervous but in other respects healthy, and her skin is smooth. Another sister died of a nervous complaint at the age of one month.

Personal history: The patient was born with a smooth skin, but when she was 3 or 4 months of age her mother remembers consulting a doctor on account of roughness of the skin of the child's legs. This roughness has persisted until the present time. The patient is a delicate-looking, nervous child, but her general health is good. She has suffered from measles and other childish complaints and has had adenoids removed.

History of present condition: The present condition was noticed three years ago when the skin of the abdomen became discoloured and

the mother noticed small "dirty marks" on the front of the abdomen, which gradually enlarged and spread upward to the chest.

Present condition (June, 1912): The front of the chest is principally affected, and there are symmetrical, pigmented, roughened patches of skin above the breasts on both sides, semilunar patches below each breast, and a circular patch above the umbilicus. The skin of these areas is papulated as well as deeply pigmented, and the surface can be peeled off, showing white skin beneath. The skin of the arms and legs



Case of ichthyosis.

is roughened by enlarged and slightly reddened follicles and that of the back and thighs is also rougher than natural. The palms are rough and the natural lines of the skin accentuated. The knees are also slightly affected. The patient perspires freely. There is no itching, and she does not suffer any great inconvenience from her complaint.

Dr. PERNET pointed out that there was a certain amount of wart-like development, which he considered brought it into the nævus category. He did not consider that ichthyosis hystrix was the same condition as ichthyosis. Ichthyosis hystrix was a nævus.[1]

[1] *Vide* Pernet, "A Note on Ichthyosis Hystrix," *Brit. Journ. Derm.*, 1911, xxiii, p. 332.

Case of (?) Urticaria Perstans Annulata et Gyrata.

By J. J. Pringle, M.B.

The patient was an intelligent woman, aged 42, who was sent to him on January 25 by Dr. Haines, of Dartmouth Park Hill. She had been seen by him for the second time on the morning of the meeting. The disease was stated to have begun about Christmas, 1910, as "a severe crop of ordinary nettle-rash with white bumps," in which no cause, either external or internal, could be discovered. She was in perfect general health, her bowels were acting freely and regularly, and she was taking no drugs. No dietetic irregularity could be traced. The white "bumps" had become pink, and the eruption had persisted, although with altered characters and in greatly increased abundance, till the date of her first consultation. It was then noted that she presented a very unusual rash, copiously and fairly symmetrically distributed over the trunk, arms, thighs, and legs. The face and hands were unaffected. The primary lesions were tense, hard, œdematous, bright pink nodules projecting rather abruptly from the general skin surface and surrounded by a marked pale, "white" areola. They varied in diameter from a threepenny-piece to a shilling, but the larger nodules showed some central depression, and by their peripheral extension much larger circinate lesions were formed, the edges of which were raised, hard and œdematous, while their centres (by comparison) were sunken and pigmented. When these circinate lesions attained the size of about a florin their margins broke up, so that the largest lesions were mostly reniform, not completely circular. The lesions were so abundant and so "fixed" in type that the possibility of a premycosic condition irresistibly suggested itself. The early manifestations of the disease were stated to be accompanied by sensations of stinging rather than of itching. This case was labelled provisionally an unusual persistent erythema multiforme.

The patient was prescribed free purgation with salines, some dietary restrictions, and salol in 10-gr. doses taken three times daily. She was subsequently given doses of hydrargyrum cum creta twice daily and occasional small doses of arsenic by Dr. Haines.

At her second visit, on the morning of exhibition, it was noted that the eruption had diminished to a remarkable degree, leaving no textural

changes in the skin. The lesions which remained presented the same characters as before, and the pronounced zone of pallor round the lesions, both recent and of older date, indicating persistent vascular spasm, was as prominent a feature as before, and influenced the exhibitor in reconsidering the condition as a persistent urticarial rather than an erythematous one. This view received support also from the patient's repeated and decided statement that the rash had begun like a nettle-rash, which was always worse at the menstrual periods. She also asserted that the larger patches existed for about two months, and then completely vanished. Factitious urticaria was easily evoked, but to no unusual extent. The blood and urine had, unfortunately, not been examined.

Dr. Pringle had never seen a precisely similar case, and the only literature he had the hurried opportunity of consulting was the recent exhaustive article on urticaria by Dr. Colcott Fox.[1] Mention is there made of cases in which "certain wheals may persist an unusually long time, and that in rare cases the majority of the wheals may do so." To this category the case appeared to belong, and this, despite Dr. Fox's wise reservation that "the persistence of a wheal is so contrary to the usual temporary character, that we rightly assume a critical attitude in accepting an urticaria perstans." He continues: "Many cases, however, have been recorded and secondary changes have been described as fibrosis, verrucosity, &c. Boeck relates a case in which they persisted *for three months*, and disappeared under the administration of sodium salicylate, without leaving pigmentation." Dr. Fox further cites such past masters in observation as Kaposi and Duhring, who "hold that the wheal is capable of eccentric expansion, like a ringworm or erythema multiforme; and in such cases ringed lesions may be produced (urticaria annulata gyrata), the usual figured patterns being formed by intersection of the curves." With this very infrequent type of disease the exhibitor thought his case undoubtedly accorded.

DISCUSSION.

Dr. COLCOTT FOX said there were several points of interest about such a case as this, points which did not favour the making of a firm diagnosis. The duration of the patches was a long one for a case of evanescent urticaria. With regard to the diagnosis from erythema multiforme, a phase was described as erythema urticatum where some serous exudation existed, but not in

[1] Clifford Allbutt and Rolleston, "System of Medicine," 1911, ix, p. 214 *et seq.*

sufficient quantity to cause vesication, but which made the surface opaline. In certain cases of erythemata there was an urticate appearance. He was interested in the question whether true urticaria ever produced serpiginous lesions. Many cases were on record of serpiginous urticaria.

Dr. GRAHAM LITTLE considered that the case was practically identical in appearance with one which he showed himself two months ago, and which he showed again that afternoon. It had been diagnosed by some members as erythema multiforme, and by others as a cross between that and lupus erythematosus, and by still others as the latter condition. The lesions had remained ever since he showed her, and they occupied much the same position as in this case, though they had definitely vesicated. He asked whether there had been any vesication in this case. He did not regard this case as erythema multiforme.

Dr. PERNET said he considered Dr. Pringle's case was an erythema multiforme gyratum.

Dr. ADAMSON thought the diagnosis of granuloma annulare ought to be considered. The firm, raised rings were very suggestive of those of granuloma annulare. They were made up of a "necklace" of closely set nodules as in that affection.

Dr. PRINGLE, in reply, said the condition undoubtedly came out at first as "a white lump." Afterwards there was a central red prominence and a white halo round it. There had been no vesication.

Two Cases of Pityriasis Rubra Pilaris.

By J. H. SEQUEIRA, M.D.

Case I.—A. I., aged 35, a laundress, had suffered from the disease since she was aged 14. The family history was unimportant and the patient had had no serious illness. She stated that the eruption first appeared on the face and spread to the neck. The eruption entirely disappeared each spring until this year, but this spring it got worse, invading not only the face and neck but also the limbs and trunk. When shown at the meeting the scalp was covered with thick scales, but the hair had not fallen to any great extent. The face and neck were covered with a diffusive scaly eruption. It was of a pinkish-yellow tint in the face, the surface being covered with large adherent scales. On the chest, abdomen and back the skin was studded with closely set, acuminative red papules capped with adherent horny plugs and scales at the mouth of the hair-follicles. The individual lesions varied from a pin's head to a lentil in size, but the majority were small.

The characteristic rasp-like feel of the skin was best marked on the back and chest. A similar condition affected both the upper and lower limbs, the nutmeg-grater-like surface of the upper arms and thighs being very obvious to the touch. The fronts of the legs were the parts least affected. The dorsal aspects of the skin over the proximal phalanges of the fingers presented groups of well-defined horny papules situated at the hair-follicles. The palms and soles were dry and scaly. The nails at these proximal parts showed some dystrophic changes. There was no evidence of visceral disease. Von Pirquet's and Moro's tuberculin tests were negative. The urine was normal. Blood examination: Leucocytes, 7,400; polynuclear neutrophiles, 56·5 per cent.; polynuclear eosinophiles, 6·0 per cent.; small lymphocytes, 22·5 per cent.; large lymphocytes, 6·5 per cent.; large hyaline cells, 8·0 per cent.; coarsely granular basophile cells, 0·5 per cent. There was slight eosinophilia and some excess of large hyaline cells and a slight diminution of the neutrophile cells.

Case II.—F. C., aged 6. This little girl presented a more severe type of pityriasis rubra pilaris. She had had three previous attacks, which usually began in April. The starting-point in her case was behind the ears. The present outbreak began six weeks ago. The face was covered with thick yellowish adherent scales producing a mask-like appearance. The scaly condition extended over the neck and chest and back. On the abdomen, legs and arms the lesions were essentially follicular, the hair-follicles being red, raised and prominent, and capped with a horny plug or scab. The palms and soles were dry and scaly, and the scalp was covered with thick, rather adherent scales. The eruption was almost universal, but the fronts of the legs and the fronts of the forearms were the least affected. The urine was normal and there was no evidence of visceral disease. Von Pirquet's and Moro's tuberculin tests were negative. The blood count was remarkable: Leucocytes, 5,400; polynuclear neutrophiles, 23 per cent.; polynuclear eosinophiles, 8 per cent.; small lymphocytes, 41 per cent.; large lymphocytes, 26 per cent; large hyaline cells, 1·5 per cent.; coarsely granular basophile cells, 0·5 per cent. The polynuclear neutrophiles were less than half the normal, the eosinophiles were high, but the remarkable feature was the enormous increase in small and large lymphocyte cells. There was no evidence of glandular affection, and it is difficult in our present knowledge of the disease to determine the significance, if any, of this blood count. A biopsy has been made in each case, and the horny layer of the epidermis was found thickened

and considerably heaped up around the mouths of the hair-follicles. These were dilated to form cups which were full of horn. In this horny material there were some sections of small hairs as seen in specimens stained by hæmotoxylin and eosin. There was no alteration in the rest of the epidermis. The sweat-glands were normal and the dermis appeared unaltered. There was no cellular infiltration.

Dr. ADAMSON said that the younger patient shown by Dr. Sequeira had been previously under his care, and that he had exhibited the case during a previous attack in May last year. Photographs were published in the *British Journal of Dermatology*, June, 1911. He wished to draw attention to a point of interest, namely, that during an interval between two attacks of what were typical pityriasis rubra pilaris the child had had an outbreak of typical psoriasis upon the elbows and knees and elsewhere. When this case was exhibited last year Dr. Adamson had recalled the circumstance that Dr. Whitfield had previously shown a case in a child, aged 4½, which at one time resembled follicular psoriasis and at another time was typical pityriasis rubra pilaris. Dr. Graham Little had also mentioned a case of pityriasis rubra pilaris in which the sister had psoriasis. The speaker believed that there was a very close relationship between pityriasis rubra pilaris and psoriasis, and it was sometimes difficult to know whether a case was one of follicular psoriasis or of pityriasis rubra pilaris.

Solar Epitheliomatosis (late Xerodermia Pigmentosa) in a Man, aged 33.

By J. H. SEQUEIRA, M.D.

THE patient had been shown before the Dermatological Society of London seven years ago. Since boyhood he has had pigmented spots and warty growths on the face and the backs of the hands. He now presented the appearance characteristic of xerodermia pigmentosa—permanent freckles, numerous pigmented warts, depressed cicatrices where the warts had dropped off, telangiectasis, and also the scars of several operations for epithelioma. He had been under Dr. Sequeira's care off and on for several years, and numerous epitheliomata had been removed. These, on section, showed characteristic cell-nests. The patient's general health has been good, and at no time has there been any involvement of the lymphatic glands. He is an agricultural labourer, and the development of the lesions is most rapid in the early summer.

DISCUSSION.

The PRESIDENT (Sir Malcolm Morris, K.C.V.O.) asked what was the age at which these cases generally passed from the pigmentary stage into the papillomatous. He recently saw a case from a South Wales town in which the pigmentary stage lasted from early childhood, and the papillomatous stage did not begin until after puberty.

Dr. PRINGLE said he had one case in which the disease began in the first six or seven months of life. The child was the daughter of a nurseryman, and had been allowed to lie a great deal in the sun. At 3 years of age she had already developed epitheliomata.

Dr. PERNET said he remembered Dr. Pringle's case quite well, as it came under his own care afterwards. He had demonstrated that the large tumour between the eyebrows was epitheliomatous.[1] Dr. Pernet had lost sight of the case for many years now.

Generalized Sarcomatosis in a Man, aged 62.

By A. WHITFIELD, M.D.

THE history showed that there had been a tumour of the eye three years before, for which an enucleation of the eyeball had been performed. Details as to the nature of the tumour were wanting, but it was known that the growth was of a malignant nature.

Eighteen months ago a swelling had appeared over the angle of the jaw on the right side, and this was followed not very long afterwards by the appearance of another over the front of the chin, and subsequently at frequent intervals by numerous others until the present condition had been reached.

The original tumour over the right angle of the jaw was at the time of exhibition about the size of a bantam's egg. It was entirely subcutaneous and there was no discoloration of the skin over it. On the chin was another large tumour of about the size of a greengage. This was adherent to, and partly actually in, the skin, which was reddened and pitted like the peel of an orange. Scattered thickly over the body, back and front, and more sparsely over the arms and thighs, were numerous other tumours varying in size from that of a bean to that of

[1] *Vide* Pernet, "Tumour of Xeroderma Pigmentosum" (British Medical Association, Manchester Meeting), *Brit. Med. Journ.*, 1902, ii, p. 1334.

a half plum. Some of these tumours were subcutaneous and had not affected the colour of the skin over them; others had involved the skin and turned it a peculiar bluish-red colour. None showed any sign of pigmentation, though it was probable that the growth was of the melanotic type. The hands and feet were quite free.

There were numerous nodules in the liver, which was considerably enlarged, and there were a good many symptoms of alimentary disturbance.

Dr. Whitfield said that he had seen the patient only once before, by the kindness of Dr. Norman, but that he proposed to take him into hospital and see what could be done for him by means of the X-rays, and possibly by the injection of salvarsan.

(*Note.*—Since exhibition the patient wrote that he would not come into hospital as another doctor had guaranteed to cure him.)

DISCUSSION.

Dr. GRAHAM LITTLE recalled to the Section the case shown by him at the meeting of November, 1910,[1] in which there was reason to regard the sarcomatosis as being primary in the skin. The patient, a young man, aged 25, had been admitted to St. Mary's Hospital shortly after being shown, and on the suggestion of Sir Malcolm Morris was given large doses of arsenic in the form of soamin injections (10 gr. twice a week). Under the treatment the lesions had become markedly diminished in volume, but the man had suddenly died with symptoms, confirmed by post-mortem examination, of general acute œdema of the tissues of the throat and neck.

Dr. WILFRID FOX said that the case he showed with Dr. Ashton Warner about eighteen months ago was somewhat similar to this. In that case they made a biopsy, but the histological structure was not at all characteristic of a sarcoma. The President, however, made a diagnosis of a sarcoma from the clinical aspects. Many of the lesions were of the so-called withering type—that is to say, coming up and disappearing without any treatment at all. The patient died about six months later from mediastinal growths, which at the time he was shown were not evident.

The PRESIDENT said Mr. Golding Bird had removed a sarcomatous tumour from the scrotum, and a few weeks afterwards there was a considerable number of nodules round the area. The patient was then given arsenic in the form of Fowler's solution, because at that time scarcely anything else was known for the condition. All the secondary tumours disappeared under the arsenic. The doses were very large ones, up to 30 minims three times a day.

[1] *Proceedings*, 1911, iv, p. 22.

Case of Developing Systematized Nævus on the Back and Front and Arms in a Boy, aged 8 Months.

By A. WHITFIELD, M.D.

As an instance of the fallacious inculpation of vaccination it was of some interest that the mother attributed the deformity, which was not noticeable at birth, to the effect of vaccination at 3 months old. The back and chest were covered with sepia-tinted wavy streaks and lines without elevation or other textural alteration of the skin. But on both hands, and to a slight extent on the arms, there were lines of verrucosity where the nævus had already become papillomatous. The case was shown as the exhibitor thought it might be of interest to see one of these extensive nævi in its earliest stage of development. He recalled the case shown by Dr. Garrod[1] a few years ago, in which a somewhat similar but more marked condition of pigmentation was present, but without any papillomatous development. In that case Dr. Whitfield had not diagnosed the true nature of the case when he saw it, until Dr. Colcott Fox pointed out that it was an early condition of the extensive nævus.

Dr. ADAMSON said that some years ago he had shown a case of extensive unilateral linear nævus in an infant exactly resembling that now exhibited by Dr. Whitfield, and that although some of the linear streaks became more pronounced and more warty they eventually all disappeared. A supernumerary nipple which was present when the child was exhibited had also disappeared.

[1] *Trans. Clin. Soc. Lond.*, 1906, xxxix, p. 216.

Prurigo, Pruriginous Eczema, and Lichenification.

An Address Introductory to a Discussion on the Subject.

By Sir MALCOLM MORRIS, K.C.V.O., F.R.C.S.Ed.

PRURIGO.

Historical.

HEBRA was not the first to describe the prurigo which is specially associated with his name. It had already been described, though not clearly discriminated, by Willan under the name of *prurigo agria*, and by Cazenave, Devergie, and other French writers under the designation of *lichen agrius*. But Willan's prurigos included all affections in which itching is associated with a special eruption and other phenomena; thus he recognized general prurigos and a number of local prurigos—prurigo of the anus, of the prepuce, of the scrotum, of the urethra, of the vulva, of the pubes; and he ranked them with strophulus and the so-called lichens of the day in the Order of the Papules. To Erasmus Wilson prurigo was simply "a pruritus associated with an organic change in the tissues of the skin," and its pathognomonic characteristic was the pruritus —"a pruritus without obvious or apparent cause." In Wilson's day, therefore, the distinction between prurigo and pruritus had not emerged, and dermatology is perhaps indebted to Hebra at least as much for his insistence upon this distinction as for his classic description of the severe form of prurigo known by his name.

In limiting the term "prurigo," however, to a single type of the affection, Hebra fell short of the truth. To him prurigo was an incurable and ultimately fatal affection, beginning in infancy as an urticaria, which is soon followed by characteristic papules that give rise to intense itching. Enlargement of the lymphatic glands follows, and later the skin undergoes the changes now known as lichenification. Kaposi saw that his master had described only the grave form of a disease which sometimes manifests itself in a milder form, *prurigo mitis*, not necessarily incurable, and showing no tendency to develop into the severer type. French dermatologists were quick to seize upon the same error, and also to question another of the Vienna master's statements—

namely, that the papules precede the itching. On these points they have long been virtually agreed, but there have been, and still are, very considerable differences among themselves. Thus Vidal [11] preferred to style the prurigo of Hebra a lichen, maintained that from the anatomico-pathological point of view prurigo is only a large papule of lichen or lichen only a small papule of prurigo, and declared that it is not rarely cured. Besnier [2] included among the prurigos many other itching conditions which he had separated from the eczema group, qualifying them all as diathetic prurigos because he regarded them as associated with individual conditions of tissues and of organs, provoked or maintained by an abnormal mode of nutrition. To him prurigo was nothing more than an itching dermatosis accompanied by a visible eruption—a position not very dissimilar from Willan's. Brocq [3] differs from both Vidal and Besnier. In his view there are three forms of prurigo: (1) *Prurigo simplex*, with no eczematization or lichenification—the urticaria papulosa or strophulus of many other writers; (2) *prurigo of Hebra*, which may be either severe or mild, and of which the milder forms may begin much later in infancy and may die away, sometimes within a few months; and (3) *prurigo ferox*, in which the papules are larger and harder, the itching is frightful, and the gland enlargement considerable, but in which the lichenification is less extensive than in the prurigo of Hebra. Darier [4] also recognizes three forms of prurigo, which, however, do not tally completely with Brocq's. The first, like Brocq's, is *prurigo simplex*, or strophulus (urticaria papulosa). The second he styles *prurigo of Hebra*, but he regards it as including not only (*a*) the type of Hebra-Kaposi, but also (*b*) a mild French type, and (*c*) prurigo ferox. His second prurigo, therefore, includes Brocq's second and third types. Darier's third prurigo is styled *prurigo vulgaris*, which may be either (*a*) diffused or (*b*) circumscribed. This corresponds with Brocq's *névrodermites* or *prurits avec lichénification*, which that author does not admit into the category of the prurigos, but regards as forms of pruritus.

It was not until the year 1881 that the prurigo so precisely and luminously described by Hebra was recognized as such in England. In the Diseases of the Skin Section of the International Medical Congress held in London in that year, Morrant Baker exhibited three cases which were accepted as examples of true prurigo of Hebra by Kaposi and the younger Hebra, as well as by Unna. The event constitutes one of the landmarks of the history of prurigo in these islands. For, strange as it may seem, in the country of the founder of the Order of Papules, who

had described, though he had failed to give clear identity to the disease, the severe form of prurigo had up to this time been overlooked. That it was, and still is, of less frequent occurrence in this country than on the Continent is no doubt true; but in the discussion which followed Morrant Baker's paper the younger Hebra affirmed that he had seen undoubted cases at St. Bartholomew's.

At the Third International Congress of Dermatology, held in London in 1896, there was a discussion on prurigo which brought into relief the wide differences of view that prevailed on this subject. Paraphrasing a famous apophthegm, it might be said, as many dermatologists, so many views of prurigo. By apt citations from all the leading authorities, J. C. White [12], of Boston, showed that there was the widest divergence on almost every phase of the disease—its age-incidence, its course, the character of the eruption, the causal relation between the neurosis and the papule, and the pathology. He enumerated nine different conceptions of the essential nature of the disease, as (1) a pruritus, (2) a Sensibilitäts-neurose, (3) a Motilitäts-neurose, (4) a pruriginous diathesis, (5) a neurodermatosis, (6) a mixture of lymphatism, arthritism, and nervosism, (7) a vasomotor transudation, (8) a trophoneurosis, and (9) a dyscrasia. To which was added a tenth view, undoubtedly the most modest, possibly the most candid of the series—pathology unknown.

During the intervening sixteen years prurigo has continued to excite the lively interest of dermatologists of all nations, but there is still nothing approaching a consensus of opinion on any of its various aspects. I have already touched upon the different views that prevail in the French school. But I do not know that English-speaking dermatologists are much nearer unanimity. The chief difference between the two schools, indeed, is that we have been rather more sensible of the difficulties of the subject than have our French confreres, and have been less prone to the confident elaboration of theories. On a question so involved in obscurity dogmatism would be eminently out of place, but one may hope that as the result of free discussion some progress, however slight, may be made towards common agreement.

Nomenclature and Classification.

It was Besnier who proposed that the severe type of prurigo described by Hebra should be known by that master's name, although it would have been better, he considered, to find a new designation for

what he considered to be in some sense a new malady. There is certainly need for simplification in the nomenclature of prurigo. That members of the same school of dermatology should regard "prurigo of Hebra" as including and as excluding the severest type of the affection is admirably calculated to produce misunderstanding. Sensible as I am of the obligations we are all under to Hebra in this matter, it appears to me that it would be better to cease to attach his name to the affection, since it has come to be, in a sense, a symbol of confusion and discord. The milder affection which he failed to recognize is as truly prurigo as the one he actually described. They are, in fact, but different types of the same affection. Why, then, not speak simply of *prurigo gravis* and *prurigo mitis?* Prurigo gravis might be held to include the worst cases of the affection, the rare type with which the name of Vidal is linked. But the differences between these two groups of cases, the less severe and the more severe, are, perhaps, sufficiently pronounced to make it convenient to reserve for the worst type of prurigo the expressive name *prurigo ferox*. If other prurigos are to be recognized, they may be similarly qualified. Thus it is believed by some that there is a *prurigo gestationis*, which can be differentiated from *herpes gestationis*, and a *prurigo lymphadenoma*, in which, it is held, true prurigo papules are present. To me, however, the evidence for these so-called prurigos seems to be by no means conclusive, and I am not prepared at present to recognize more than the three forms of the affection mentioned above—prurigo gravis, prurigo mitis, and prurigo ferox.

With regard to classification, I cannot regard as a prurigo any itching affection which does not present an eruption of discrete, hard papules, of the kind and in the situations to be presently described, followed sooner or later by the peculiar roughening and thickening of the skin known as lichenification. Besnier claimed that there was a prurigo senilis, a prurigo of the scrotum, of the anus, of the vulva, and so forth; but such affections are now regarded as belonging to the great pruritus group, and it is convenient, I think, to limit the term "prurigo" in the way suggested. From prurigo, thus understood—the prurigo simplex of Brocq and of Darier, which appears as the first of their three types, and is identical with the lichen urticatus of Bateman—the urticaria papulosa of Kaposi, Duhring, and other writers, is excluded by the absence of lichenification, while the prurigo vulgaris which is the third of Darier's types, and is regarded by Brocq not as a prurigo but as a pruritus with lichenification, is excluded by the absence of the characteristic papules. I confess that I have sometimes been disposed to admit this last affection

into the group of prurigos, but further consideration has led me to adhere to the definition of prurigo formulated above. A classification which rests on a purely clinical basis can, of course, make no pretension to finality; but so long as the ætiology of prurigo is veiled in its present obscurity, and the pathology is little more than a collection of rival theories, no other mode of classifying the affection is possible.

Symptoms.

Whether of the mild or of the severe type, prurigo usually begins in early infancy, between the eighth and the twelfth months of life. Hebra and Kaposi [6] went further, and taught that it always begins in this period, but cases have been reported commencing between the ages of 10 and 15, and even later. According to Hebra and Kaposi, again, it appears first as an urticaria, which manifests itself in the form of wheals, of itching, of excoriations, and of insomnia, and as an urticaria it persists until about the beginning of the second year, when the characteristic papules begin to appear. In this interpretation of the first signs of prurigo I am unable to acquiesce. It is not unlikely that one form of urticaria, namely, urticaria papulosa, forms a connecting link between urticaria and prurigo, but, like Besnier, the late J. F. Payne, Colcott Fox, and other dermatologists, I have never seen an urticaria develop into prurigo. The papules are hard, small, often perceptible only to the touch, pale or reddish in colour, and distributed principally on the extensor surfaces of the limbs, the lower part of the abdomen, the sacral region and buttocks, and the back and front of the chest, but sparing the joint flexures. They give rise to violent itching and, when subjected to the irritation of scratching, become reddened and increase in size. When a papule is excoriated serum and blood exude, which quickly dry into a brown crust. Other lesions appear, which may resemble those of eczema (except that the flexor surfaces are still usually spared) and of urticaria, and finally the skin becomes lichenified. One crop of papules succeeds another, and the disease becomes chronic, but usually there is some abatement in summer, with exacerbation in winter. In severe cases the integument takes on a brown colour, there is desquamation, the hairs are extruded, there are pustules and sores, and the femoral and axillary glands enlarge and may go on to suppuration.

One peculiarity of the disease is that after the third year of life it undergoes no further evolution, so that there is no essential difference

in pathological physiognomy between a patient aged 3 and a patient aged 30. Another peculiarity is that prurigo mitis never develops into prurigo gravis. In the former type, as a rule, the papules are less numerous, the eruptions less frequent, the itching is much less intense; the lesions, too, may be limited to the lower limbs. In the severest and rarest form of the disease, which may be called prurigo ferox, the papules are much larger, varying in size from a small pea to a small cherry (Brocq), are noticeably raised above the level of the skin, and give to the touch a sensation of hard nodosity; the colour varies from a pale pink to a vivid red. They are crowned with a large thick-walled vesicle, sometimes filled with a purulent liquid. Disseminated in no traceable order over the body, and even upon the face and scalp, they are most numerous upon parts exposed to friction. They are accompanied by itching of the intensest kind, and the patient, in his frenzy, seeks relief by excoriation that may not stop short of tearing off pieces of flesh. The lymphatic glands are usually much enlarged. Lichenification is present in all cases, but is not so widespread in prurigo ferox as in the other forms of prurigo. As in the other types, the symptoms are worse in winter than in summer.

Hebra, and after him Kaposi, held that the papular eruption precedes and is the cause of the itching, and the former considered it probable that the itching is due to irritation of the papillary nerves set up by the serum which quickly accumulates in each efflorescence. Cazenave was one of the first of the French school to maintain that the itching precedes the papule, and this view, which prevails generally among French dermatologists, was crystallized by Jacquet into the epigram: "Ce n'est pas l'éruption qui est prurigineuse; c'est le prurit qui est éruptif." A clinical experiment of Jacquet's has been adduced by Besnier in support of the theory that the itching and the papules are independent of each other. The right arm of a girl who had for two years suffered from classical prurigo, and who presented daily upon the trunk and the limbs a discrete eruption of typical papules, was occluded with prepared wadding and a bandage, the bandage being removed each morning for inspection and then re-applied. The itching continued, but not a single papule appeared, while each day, on the left arm, there was an eruption of from three to six fresh papules. The left arm was now similarly occluded, when, though the itching continued, there were no fresh papules, while crops appeared daily upon the right arm. An interesting experiment, certainly; but if this question is ever decided it will be by a concurrence of clinical testimony rather than by the

experiments of a single observer — experiments, too, which set up abnormal conditions. Jacquet's experiment, I suggest, proves too much, just as his epigram expresses too much. It has been repeatedly noticed—I am not sure, indeed, that this is not common ground—that the papule, when it has appeared, is a centre of itching. If this be so, it follows that whether the papule or the itching comes first, the two are not independent, as Jacquet's experiment is intended to prove. I would go further than this and say that experience inclines me to the belief that Hebra was to some extent, at any rate, right in his view that the papule precedes the itching and scratching. In many cases the papules appear at an age so early that scratching is hardly possible; the papules, moreover, occur in situations in which they are not accessible to the infant's fingers, and they have been observed to effloresce in groups, as though in obedience to internal impulse rather than in response to external influences such as scratching or friction. But the question is one in which none of us can profess to have had more than a limited experience; while, owing to the early age at which the affection usually begins, it is exceptionally difficult to accumulate satisfactory evidence. Since others who have had cases of prurigo under observation are satisfied that the itching precedes the eruption, I am prepared to regard it as possible that both views are correct. I cannot regard the papule as more than a focus of the itching. The actual cause of the affection, whatever its nature, lies behind the papule. Neither the papule nor the itching, whatever the order of their appearance, is more than a manifestation of the disease—the one a sign, the other a symptom. Is it not, therefore, conceivable that, owing possibly to the accidents of the individual case, the itching may sometimes come first, and in other cases the papule? This view may at least claim the advantage of reconciling the apparently conflicting testimony of observers who are equally competent and equally veracious.

Ætiology.

The exciting cause of prurigo has been found in bad hygiene, in overcrowding, in defective alimentation, in digestive troubles, in gastro-intestinal fermentation, in dentition, in auto-intoxication, &c. Brocq admits the influence of these various factors in provoking and maintaining attacks of the disease, but denies that they are essential causes. The true cause he finds in hereditary cross-breeding (*un métissage héréditaire fort complexe*), in which four influences co-operate

—namely, (1) the neurotic condition of one or both parents, either of long standing or operating during the mother's pregnancy; (2) lymphatism, either idiopathic or due to tuberculosis or syphilis; (3) auto-intoxications, originating in arthritism and aggravated by bad hygiene and by life in crowded cities; (4) chronic intoxications, in which alcoholism and caféism take the principal rôle. More shortly, he describes the prurigo of Hebra as a cross-breeding (1) of arthritism, (2) of neurosis, (3) of lymphatism, tuberculous or due to hereditary syphilis, and (4) of hereditary intoxications (alcoholism and caféism). In his view, it is on the soil thus prepared that the occasional causes enumerated above, and especially defective alimentation, digestive troubles, troubles of the nervous system and bad hygiene, play their part. Darier's ætiology is much more simple, and as much less confident. He attributes the disease to hereditary influence and, perhaps ("peut-être") to gross errors of alimentation in early infancy; and he mentions that he has seen several cases in which prurigo coincided with asthma. Kaposi contented himself with pointing out that prurigo is met with much more frequently among the poor than among the rich, and more seldom in healthy than in feeble, badly nourished and neglected, or scrofulous infants. We may, however, in my opinion, safely include heredity among the ætiological factors of this disease. It is difficult to believe that from a neurosis so severe and inveterate, and usually (though not invariably) manifesting itself in early infancy, hereditary influence is absent. But it requires more mental enterprise than I am conscious of possessing to adopt the elaborate theory of Brocq, which looks as though it were less an induction from ascertained facts than a speculative endeavour to assemble all the hereditary influences which could possibly count in this connexion. That this ingenious and distinguished author handsomely meets the craving for a comprehensive ætiology cannot be denied; but I know of no other merit to urge in favour of this theory. We are on firm ground in recognizing the influence of the occasional causes which he enumerates. That they are more than predisposing causes is, I agree, doubtful, for in some cases they cannot be traced, and this is another reason why it seems almost inevitable to regard heredity as at least one of the causes, and possibly the essential cause, of prurigo. There is, indeed, one other possibility to bear in mind—that prurigo may be due to a micro-organism. Finding the changes in the epidermis and the hair-follicles to bear some resemblance to those produced by micro-organisms in certain infectious diseases of the skin,

Unna suggests that the disease may belong to the microbic group. I know of no facts, however, beyond that just mentioned, to support this hypothesis; and prurigo has always been regarded as non-infectious.

Pathology.

The principal theories of the nature of the papule of prurigo are: (1) That of Riehl—that they are spastic œdematous papules of the cutis, closely allied to urticaria; (2) that of Auspitz—that they are pseudo-papules, depending on the contraction of the arrectors; (3) that of Caspary—that they are epithelial papules due to acanthosis. More recently Leloir and Tavernier claim to have observed a degeneration of the prickle cells with the consequent formation of cysts containing a clear fluid, some altered epithelioid cells and leucocytes, and their finding has been confirmed by Kromayer and other pathologists. Hebra was the first to teach that the papule of prurigo has analogies of structure with the vesicle; but the view of Leloir and Tavernier is that it is *sui generis*. Darier states that he has failed to observe the intra-Malpighian cavity described by Leloir and Tavernier, nor has he found the œdema reported by Riehl; but he agrees with Caspary that the papules are the expression of an acanthosis. Unna [10] reports, with Riehl, that there is a spastic œdema of the cutis, and that the prurigo papule has an urticaria-like basis, but there is also, he says, a proliferative inflammation of the vessel sheaths as well as still more characteristic changes in the epidermis—a degeneration of the prickle cells into a pulpy mass, forming a vesicle, and later, in some instances, an impetigo pustule, which, however, contains no staphylococci. These vesicles he identifies with those found by Leloir, but unlike Tavernier, he could determine no connexion between them and the sweat-pores. Unna, continuing to play the part of reconciler, holds that Auspitz and others who have described changes in the hair-follicles are in some measure correct. He found the arrectors in some of the follicles thickened, and persisting in spastic contraction, so that the hair-follicle was erected and the point of insertion of the muscle in the papillary body appeared funnel-shaped. He is unable, however, to agree with Auspitz that the characteristic lesion of prurigo is a mere pseudo-papule due simply to a contraction of the arrectors, for the same hair-follicles show proliferative and exudative inflammatory changes and necrosis, so that the contraction and enlargement of the arrectors is nothing more than an accompanying or secondary symptom. The epithelial proliferation described

by Caspary he compares with the acanthoses present in the neck of the follicle and in its neighbourhood in certain infectious diseases of the epidermis.

It will thus be seen that Unna has found something to agree with in most of the prevalent theories of other pathologists. But considerable differences still remain, and it is not unlikely, as he suggests, that the workers whose investigations he discusses had before them entirely different conditions.

Prognosis.

Hebra declared that prurigo, as he understood the term, was incurable. Kaposi concurred, although he held that if treatment is begun in early infancy the disease may be so favourably influenced that at times the patient may believe himself to be cured. Even in cases of moderate severity, he believed, there is no hope of cure, while cases of the mild type are only curable if treatment begins in early infancy and is perseveringly applied. There are grounds, I think, for giving a rather more hopeful prognosis in cases of the mild type. Not seldom, under judicious treatment, such cases recover during childhood or in adolescence. At best, however, the affection is a serious one, entailing much irritation and nervous depression and suffering; at worst it is one of the most distressing affections in the whole range of dermatology.

Diagnosis.

When it has reached the typical stage, prurigo, in the sense in which the term is employed in this paper, is as a rule easy of recognition. The positive characters are the usual origin of the affection in infancy, its persistence, the poor general health, the preference displayed by the papular eruption for the extensor surfaces of the limbs, and the immunity enjoyed by the bends of the joints. The glandular enlargement, in association with the eruption, is one of the distinctive features. In the early stage the diagnosis from urticaria papulosa is, however, exceedingly difficult, and it may be necessary to defer judgment. In later stages, also, the characteristic lesions of prurigo may be masked by eczematous crusts or by pustules, &c., and the eczematous phenomena may even extend to the parts spared by the prurigo. In cases thus complicated the diagnosis as between prurigo and such conditions as chronic eczema, chronic urticaria, scabies, and pruritus may have to be postponed until the secondary lesions have

healed. The distribution of ichthyosis is similar to that of prurigo, and the former disease, like the latter, may be complicated with eczema; but in ichthyosis the prurigo papule is absent, and the skin is extremely dry and scaly; and it is seldom difficult to distinguish between the two conditions.

Treatment.

This must be mild or vigorous according to the intensity of the affection, but whether this be of the one type or of the other, the measures must be applied again and again, as the symptoms reappear. As intimated under prognosis, treatment in the early stages is of special importance. The complicating lesions having been dealt with, the indications, as Colcott Fox [5] says, are to improve the patient's nutrition by good hygiene, generous diet, and cod-liver oil; to control the itching by baths medicated with starch, sulphurated potash, creolin or izal, and to dissipate any co-existent eruption. All irritating preparations must be avoided. The external remedies employed by the Vienna school are sulphur, tar, soap, and naphthol. The sulphur is used in the form of soap, of solution of sulphur of potash, or of thermal waters; the tar, either pure or mixed with olive or cod-liver oil. The tar is used especially to control the itching. Brocq holds that the best of the topical remedies is cod-liver oil, one recommendation of which is that it can be used in all stages of the affection, even the most acute. If the surface is smeared with the oil the applications must follow each other quickly; and he considers it better to envelop the parts in an impregnated many-folded bandage covered with an impermeable varnish. He speaks highly of a preparation of cod-liver oil and white wax in the proportion of three to one, used as a pomade, with or without covering. Cod-liver oil may also be used in the form of ointment, with which, when there is intense itching, β-naphthol or carbolic acid may be incorporated. Colcott Fox has had excellent results from the use of a carbolic acid ointment, Wolff from alternate injections of carbolic acid and pilocarpine. In severe and rebellious cases the patient may be soaped in a warm bath, and a tar preparation or Vleminckx's solution applied, the patient continuing in the bath or lying in bed with the application still on. If such vigorous measures as these be employed they must, of course, be carefully watched. Friction with Wilkinson's ointment, ten or twelve times repeated, is also said to give relief in bad cases. Occlusive dressings, such as Unna's glyco-gelatin, or caoutchouc, have been found useful in preventing the eruption, though occlusion, as

Jacquet's experiment shows, fails to influence the itching. Radcliffe-Crocker's [8] chief means of treatment were rest in bed, the application of naphthol ointment, and the administration of cannabis indica. Thyroidin is well spoken of as favourably influencing the eruption, at least temporarily.

Thibierge [9] has employed lumbar puncture in a long series of cases of pruriginous dermatoses, and while he reports temporary benefit in some "diathetic prurigos" and in dry chronic eczemas, he has been unable to draw any conclusion from the few cases of prurigo of Hebra which he has submitted to this treatment. The itching affection in which he has found it to yield the best results is lichen planus. Bayet [1] has used radium extensively in a number of pruritic affections, including Darier's third prurigo, with almost constantly good results, even in refractory cases. The itching disappeared almost suddenly, and the relapses were few and easily amenable. But his report does not include cases of prurigo proper. Brocq believes that he has seen cases of even rebellious prurigo mend under radium therapy, and I am disposed to think that radium and the X-rays may prove to be the least unpromising methods of treating this affection. It will be seen that there is as little concurrence of opinion in treatment as in other phases of this baffling affection. I have thought it desirable to enumerate the chief remedies that have been well spoken of, for in prurigo, as in other pruritic affections, the same agents yield different results in different hands, or in the same hands in different cases, and the practitioner, when he fails with one, must try others.

Pruriginous Eczema.

Just as urticaria papulosa, as mentioned above, is a link between prurigo and urticaria, so do the pruriginous eczemas form a chain which connects prurigo with eczema. These allied affections may appropriately be briefly discussed before passing on to consider the lichenification which is common to them and to so many other pruritic conditions.

Pruriginous eczema corresponds with certain of Besnier's diathetic prurigos. With Brocq, I do not admit their right to be regarded as forms of prurigo, though Unna holds, on both clinical and histological grounds, that the changes which take place in the skin in these forms of eczema, and which he interprets as an increased vascular tone, suggest that both processes have a common basis. The itching which

characterizes these eczemas is so intense that their right to be qualified as pruriginous is incontestable, while the distinctly eczematous lesions which effloresce at times of exacerbation and are mingled with the equally unmistakable marks of lichenification fully justify their being ranked among the eczemas. The lesions are of the most varied kind—erythematous, urticarial, papular, vesicular, impetiginous, with a preponderance of the urticarial element, and thus they extend by almost imperceptible gradations from eczema on the one hand to prurigo on the other. From true prurigo they are differentiated by always having an antecedent history of eczema, by running a less regular and more varied course, and by not being limited to the extensor surfaces, though it is these surfaces that are most affected by the itching, the lesions on the flexor surfaces corresponding rather with those met with in papular squamous eczema of the ordinary type. It is usual to find scales, crusts, and vesicles in situations where the scratching has been most unrestrained. There is seldom profuse "weeping," but in rare cases the whole surface of the skin may be moist. As a rule there is some glandular swelling, less pronounced, however, than in prurigo.

There is, perhaps, no pruriginous affection, not even prurigo ferox, in which the itching is more intense and intolerable than it frequently is in this group of eczemas. Such itching, as I have heard patients declare, is far harder to bear than pain; the pain of excoriation, indeed, is the only assuagement that can be found. Not seldom the pruritus is psychical; often, too, it is periodical, undergoing nocturnal or seasonal accessions and remissions, in some cases for long periods together, with singular regularity.

Frequently, though by no means invariably, the affection begins in infancy or in early life. The patients often have a flabby skin with more pigment than normal. Sometimes the eczema is in close relation to dentition: it may also be associated with asthma, with uterine disorders, with mental strain or shock, with insomnia, migraine and other nervous phenomena, or with xerodermia. I have known a distinct alternation observed time after time between asthma and pruriginous eczema, as though the one or the other were a necessary manifestation of the underlying morbid state. The itching may be due to disorders of the secretions which so far defy detection, or to the influence on the nerve-centres of morbid conditions of the blood, or to changes in the nerve-endings of the skin, or to hereditary cutaneous irritability, or to microbic agency, or—as is indeed more probable—to a combination of two or more of these pathological states. Whatever the cause, there

is between the itching and the consequent lesions the reciprocal reaction already noted in connexion with prurigo. The itching provokes scratching, the scratching sets up lichenification, which irritates the nerve-endings and provokes further itching; and the main object of treatment is to break this vicious circle by reducing the lichenification and restoring the damaged tissues to the normal state. The most effective means of attaining this end will be considered in the next section.

Lichenification.

The word "lichenification" was applied by Brocq in the year 1891 to the peculiar roughening and thickening of the skin which so frequently follow scratching; but the changes themselves had been clearly described long before. Brocq expounded his theory of lichenification and proposed the term in a lecture delivered on May 29 and June 3, 1891, and published in that year, and it was not until May 12, 1892, that Besnier proposed the term "lichenization" for the condition. Brocq was therefore first in the field, and his also is the more appropriate name, since there is implied in it the idea that the changes in question, instead of being the direct consequence of the itching, or of the primary lesions, are *made*—caused by the scratching. Lichenification, as Brocq says, is not pathognomonic of a definite morbid state, but rather a general process which may develop without a preceding dermatosis, or may occur in the course of such dermatoses as prurigo, chronic eczema, pruriginous eczema, psoriasis, psoriasiform parakeratosis, pityriasis rubra, lichen planus, certain artificial eruptions, and, according to Sabouraud, chronic streptococcic infections. Lichenification, then, may be, on the one hand primary, or on the other hand secondary, as when it supervenes upon an anterior dermatosis. Primary lichenification may be due to so simple a cause as slight habitual friction, as in a case mentioned by MacLeod [7], that of a timekeeper in a factory, who, spending his time leaning on a counter checking the time of the workmen, got into the habit of rubbing his arm on the counter, with the result that a patch of lichenification appeared on the ulnar aspect of the left forearm. Primary lichenification may also be due to friction of corsets, hernial bandages, and the like, or to contact with irritant liquids, or secretions, as in the thighs of women who do not keep themselves scrupulously clean. Brocq's great example of primary lichenification is the group of conditions which he styles *névrodermites* or *prurits avec lichénification*—the chronic or papular eczema of the Vienna school,

the *lichen simplex chronique* of Vidal, the *prurigo vulgaire* of Darier. This pruritus may be either circumscribed or diffuse. In the former the only cutaneous lesion is the lichenification, and this is usually true also of the latter, though rarely there may be urticaria as well as itching.

Clinical Features.

The first change in the skin is a darkening of the colour. On close inspection the integument is seen to be finely grained, and then flat, imperfectly delimited, somewhat shiny, pseudo-papules appear. These various lesions become more pronounced, both in colour and in form. The skin is infiltrated with embryonic elements, grows thick and hard and rugose, the normal striæ become exaggerated and cross each other, so that the surface is broken up into a network of square, lozenge-shaped or polygonal "meshes," presenting some resemblance to the glossy facets of a miniature mosaic. Sometimes there is a covering of fine scales. The surfaces affected by the process vary greatly in extent, and the borders are ill defined.

Lichenification is seen in its most typical form in the cases which Brocq designates *névrodermites*. In the circumscribed form the affected surface is about as large, on an average, as the palm of the hand; in the diffuse the lichenification occupies very extensive and imperfectly delimited areas. In *circumscribed* lichenification the patch is usually somewhat oval in shape, but the form varies greatly in different cases, as also does the site, but the favourite situations are the neck, the upper and inner parts of the thighs, the antero-extensor aspect of the legs, the lumbar region, the scrotum or the vulva, the female waist, the axillary and popliteal spaces, the plantar and palmar surfaces. The face is usually spared, but MacLeod has seen characteristic lesions on the lower eyelids and behind the auricles. The patches may be single or multiple and are frequently two or three in number; occasionally they are symmetrical, especially when they are in the neighbourhood of folds. A complete plaque, according to Brocq, presents simultaneously three concentric zones: (1) An irregular external zone, some two or three inches in breadth, made up of tiny papules, varying in colour from *café au lait* to clear brown or to brownish-yellow; (2) a middle or papular zone, pinkish in colour, composed of glistening papules irregular in contour, often flat-topped, and varying in size from a pinhead to a small lentil; (3) an inner zone, or zone of infiltration, usually oval in shape, in which the process of lichenification is seen at its height. It is in this

third zone that the infiltration, the thickening, and the quadrillation of the skin are most marked, and, except in regions where there are abundant secretions, the surface is covered with fine scales. These, then, are the zones of a *complete* plaque. But much more frequently the plaque is *incomplete*, and is composed only of discrete, more or less developed papules, or of a zone of infiltration. The itching is worse towards evening and soon after the patient has gone to bed. It is often intermittent and at times may be completely absent. In some cases it is so intense as to induce nervous crises, and the patient is only calmed when he has excoriated the seat of the pruritus. When a plaque is about to disappear the itching becomes less severe and then entirely ceases. On an average a plaque continues for several months, and it may subsist for years.

In *diffuse* lichenification the process is much the same as in the circumscribed form. The regions most affected are the arms and the forearms, the thighs, the upper part of the thorax, and sometimes the flanks, the lower part of the abdomen, the legs, and even the face. When the limbs are affected the lichenification is frequently symmetrical.

Pathology.

The histological structure of the skin in lichenification is less altered than one would suppose from the marked changes on the surface. Darier interprets the process as essentially a hyperacanthosis, an active proliferation of the rete mucosum, with considerable lengthening of the papillary processes, and a cellular infiltration, moderate in degree, in the papillary layer.

Ætiology.

In lichenification, as in the itching which precedes it, heredity is not improbably a factor. Women are more liable than men, possibly because of their inferior nervous stability, and it is most often met with in adults. Why it is not a sequel of long-continued scratching in all pruriginous affections, and why some subjects of the affections in which it usually appears escape, are obvious questions. To the first, Brocq's reply is that some cutaneous affections may so modify the vitality or nutrition of the tissues that lichenification is easily produced, while in others the resistance of the integument to trauma appears to be normal, or even augmented. To the second question he finds the

answer in personal idiosyncrasy. Some subjects, he says, seem to be more predisposed than others to lichenification; he thinks it possible, indeed, that in lichenification individual predispositions may play the capital rôle. For the production of lichenification, then, not only itching and scratching are necessary, but the affection which is the cause of the itching should predispose to the lichenification, and the patient should also have a similar predisposition. It need hardly be remarked that the very tentative terms in which this explanation is couched show that it is nothing more than a speculation, however plausible. In the present state of our knowledge it is certainly difficult to understand the incidence of lichenification, except by assuming a predisposition either set up by the affection, or pre-existing in the patient, or both the one and the other. With that, for the time being, we must be content.

Diagnosis.

Lichenification, as a rule, is easy of recognition. It is most closely simulated by the lesions sometimes met with in the genito-crural region in women affected with gleet, but, according to Brocq and L. Bernard, the surface of the skin in these cases is more velvety. In some cases it is difficult to distinguish between lichenification and patches of lichen planus, and there is much to be said for the view that the two processes are intimately related. Brocq, however, contends that in lichenification the initial lesion is essentially unlike that of lichen planus, in which the typical lesion, instead of being a macule, or an ill-defined flat or round papule, is a flat polygonal papule, not seldom umbilicated and lilac-tinted, with puncta and milky lines on the surface which have no analogies in lichenification.

Prognosis.

Untreated, the patches may persist for many months, or for years, or, the itching and scratching ceasing, may gradually disappear.

Treatment.

The large question of the treatment of the pruritus which precedes lichenification is foreign to my present purpose. The lichenification itself may be treated either with X-rays or with radium. Some dermatologists prefer the former; I have had my best results with the latter. I need only cite one case, that of a woman, aged 36, who had suffered for seven years from intolerable irritation, originating in a severe nervous

attack; she was unable to sleep, and was emaciated and neurotic. In shape the lichenified area was roughly triangular, with the apex at the nape of the neck and the base-line connecting the spines of the scapulæ. At the Radium Institute an apparatus 4 sq. cm. in extent, containing 80 mg. of pure radium bromide, and shielded with $\frac{1}{100}$ mm. of aluminium, was applied for ten minutes to successive patches until the whole area had been treated; the exposures were twice repeated at intervals of four weeks, with the result that, save for a very small patch at the nape of the neck, the lichenification disappeared. There was great improvement, also, in the patient's general condition, and she gained 7 lb. in weight.

REFERENCES.

[1] BAYET, A. "Le traitement des névrodermites par le radium," Bruxelles, 1910.
[2] BESNIER, E. "Sur la question du prurigo," *Trans. of the Third Internat. Congr. of Derm.*, Lond., 1896, p. 24.
[3] BROCQ, L. "Traité elémentaire de dermatologie pratique," Par., 1907.
[4] DARIER, J. "Précis de dermatologie," Par., 1909.
[5] FOX, T. COLCOTT. "Prurigo," in Allbutt and Rolleston's "System of Medicine," 1911, 2nd ed., ix, pp. 269-75.
[6] KAPOSI, MORITZ. "Pathologie et traitement des maladies de la peau," traduction par E. Besnier et A. Doyon, Par., 1891.
[7] MACLEOD, J. M. H. "Pruritus," in Allbutt and Rolleston's "System of Medicine," 1911, 2nd ed., ix, pp. 275-300.
[8] RADCLIFFE-CROCKER, H. "Diseases of the Skin," Lond., 1905.
[9] THIBIERGE, G. "La ponction lombaire dans les dermatoses prurigineuses," *Bull. et Mém. Soc. méd. des Hôp. de Paris*, 1905, 3me sér., xxii, p. 918.
[10] UNNA, P. G. "Histopathology of the Diseases of the Skin," translated by Norman Walker, Edinburgh, 1896.
[11] VIDAL, E. "Considérations sur la prurigo de Hebra," *II Internat. Derm. Congr. abgehalten in Wien im Jahre*, 1892, p. 192.
[12] WHITE, J. C. *Trans. of the Third Internat. Congr. of Derm.*, Lond., 1896, p. 46.

CONCLUSIONS.

(1) Prurigo is a distinct papular affection, and not an urticaria, though in an early stage there may be difficulty in distinguishing it from that disease.

(2) The term "prurigo" should be reserved for pruriginous affections presenting the characteristic papule and followed by lichenification.

(3) For the avoidance of confusion the name of Hebra should be dropped out of the nomenclature of prurigo, and the various types of disease should be qualified as *Prurigo mitis*, *Prurigo gravis*, and *Prurigo ferox*.

(4) Though prurigo usually begins in infancy, it may begin much later in life.

(5) Prurigo mitis never develops into the severe forms of the disease.

(6) The papule of prurigo may in some cases follow, and in other cases precede, the itching.

(7) Hereditary influence in the ætiology of prurigo is doubtful except as a factor in a family neurosis, and established facts do not appear to sustain Brocq's elaborate theory of a complex hereditary cross-breeding.

(8) While prurigo is always an intractable affection, the prognosis in prurigo mitis is not necessarily hopeless.

Questions for Discussion.

(1) Should Hebra's name be dropped out of the nomenclature of prurigo?

(2) Should the term "prurigo" be limited to affections presenting the papule described by Hebra and the subsequent lichenification?

(3) Does prurigo begin as an urticaria?

(4) Does the itching precede the papule, or the papule the itching, or is there no invariable order?

(5) Is prurigo curable?

(6) What are the best methods of treatment?

(7) Why is not lichenification always a sequel of much scratching?

(8) Does the personal idiosyncrasy count for more or for less in lichenification than the influence of the pruritic affection?

(9) Is heredity a factor in the causation of lichenification?

(10) If pruriginous eczema, while true eczema and not true prurigo, is a link connecting eczema with prurigo?

(11) If lichenification may be either primary, as in the *névrodermites* of Brocq, or secondary, as in prurigo, chronic eczema, psoriasis, pityriasis rubra, lichen planus, &c.?

(12) If to explain the incidence of lichenification it is necessary, in the present state of knowledge, to assume a predisposition either set up by the affection or pre-existing in the patient, or both?

DISCUSSION.

Dr. COLCOTT FOX said he was very much interested in the President's remarks about what was a very important factor for the older dermatologists in the Section, namely the exhibition of cases of prurigo by the late Mr. Morrant Baker at the International Congress in 1881. At that date much interest was taken in these cases. In England at that time much confusion existed about the nature and diagnosis of prurigo, and nobody seemed to have very decided views about it. Mr. Morrant Baker brought up three magnificent cases, and at once Professor Kaposi, of Vienna, said "That is our prurigo." That made a great impression on the younger men, and they forthwith set to work on a surer foundation to observe such cases in the future. The epoch was similar in importance to that in 1889 in Paris, when a case of pityriasis rubra pilaris was shown, and long years of confusion ended by Kaposi saying "That is my lichen rubra." With regard to the terms employed for these diseases, in the old literature much confusion existed between the terms "prurigo" and "pruritus." But one word which had been a standing source of annoyance to him was "pruriginous." He did not know whether that term arose from the French "prurigineux," but if so it was a bad translation. He did not know whether it meant a pruritic eruption, or one of the type of prurigo. In using such terms the user should be careful to define what he meant. The group prurigo was not common, but cases of it were met with from time to time. His experience was that, on the whole, it was a very well defined disease, and that it was fairly easy to diagnose, except under certain special circumstances, for instance when all the skin was secondarily thickened with lichenification. When that was so it was impossible to make the diagnosis until the lichenification had cleared up. He had not found a necessity for dividing these cases up into any special types, though he admitted that one found marked differences. There was great variation in intensity and in duration. The disease might be lifelong; on the other hand, he had found it beginning later in life than one was formerly taught to expect, and it did not always begin in infancy. There were comparatively mild cases, and some cases were most intense. Vidal established and individualized a special form which he called prurigo

ferox, but he did not remember having had a case of this. Besnier wrote a most instructive paper as a result of his experience, and that author seemed disposed to include prurigo in a great mass of eruptions in which the leading feature was, expressed shortly, toxæmias of various sorts. Lichenification, eczematization and all sorts of eruptions occurred in the skin, and he (the speaker) thought it was too early to admit prurigo into such a vast chaos. He wished to say a word or two about the common disease known as lichen urticatus, which was first described by our countryman, Thomas Bateman. It was a common disease and very interesting. The French school were now beginning to include it with prurigo, giving to it the name "prurigo simplex." He objected to the name "prurigo" for it, and the word "simplex" was a very inadequate qualification. The commonest elementary lesion was a central prurigo-like papule centring in an erythematous red *tache*. He wished to remind the meeting that these lesions showed a strong tendency to become evolved late in the day and at night, and the *tache* portion was evanescent, so that in the middle of the day often only the papule persisted. Sometimes it was difficult to say definitely whether the condition seen was prurigo or not. In order to show how multiform this eruption might be, he mentioned that the red *tache* surrounding the papule might have in it a little serous exudation, not enough to vesicate, but to make the surface opaline, and it was called a wheal. The same thing occurred in erythema multiforme, the papules taking on this opaline tint, when it was called erythema urticatum. When the opaline character was there and a little more raising up and swelling existed it looked like urticaria. But that was not all, for in rare cases, especially involving the hands and feet, the serous effusion might be so intense that bullæ, looking like pemphigus, might be formed. And even that was not all, for the central papule, under certain conditions, might vesicate also, and when it was surrounded by these red *taches* it was almost indistinguishable from varicella, and that was what Sir Jonathan Hutchinson called varicellar prurigo. It was true that this lichen urticatus might come out after any constitutional disturbance—amongst other things varicella—but he had traced these so-called varicellar prurigo cases intermitting with the other typical forms, one week being of the common form, and next week the varicellar form. It was only one more stage for these vesicular central lesions to become puriform, and then the characters were altered, and pustules occurred, and crusting. He mentioned this because it was a multiform eruption with many phases, and he hesitated

to include it under prurigo. And even if the term "prurigo" were applied, "simplex" in this connexion was quite inadequate. He sometimes thought that the old disused term "strophulus" might be applied. There was much to be said in favour of Bateman's old name at the time that he gave it, for the lichen was the papule which he noticed, and there was urticaria. He did not think it was getting much further to call it prurigo urticatus. He objected to Kaposi's name "urticaria papulosa," as also he did to the term "prurigo simplex." With regard to lichenization and eczematization, all had followed with great interest what was said in the President's paper, and he considered it was a real condition, and what had now been written had cleared the ground of many difficulties in the study of skin disease. He agreed with all that had been said about secondary processes in the skin being peculiarly frequent in very inveterate cases of prurigo.

Dr. JAMES GALLOWAY said that the argument that he intended to bring forward as to the ætiology of prurigo might seem to be a little far-fetched, and might be criticized as being an argument derived from simple analogy—not a good process of reasoning; but in regard to the difficult subject of prurigo and its allied skin lesions, he was not aware that there was very much firm ground for argument other than those of analogy in the consideration of its ætiology. Making use of this method, he wished to say that the disease of which he had had some experience, and which most closely resembled the clinical aspects presented by true prurigo, using the word in the traditional sense ascribed to Hebra, was the very remarkable and serious pruriginous state occurring in certain cases of Hodgkin's disease — generalized lymphadenoma. In this condition it was well known that rarely nodules and plaques of the lymphadenoid tissue developed in the skin. Such a case was recently in the Westminster Hospital under the care of Dr. Hebb and Dr. Colcott Fox, which he (the speaker) had had the opportunity of seeing. In these cases little or no pruritus might be experienced, but in other cases of lymphadenoma, probably still more rarely, an eruption occurred, characterized by the most terrible pruritus; small papules made their appearance on the skin, the points of which were torn off by the patient, and the greatest exhaustion resulted from the itching, the restlessness, and the merciless scratching and rubbing of the skin which was the consequence. Such a case in a woman had recently been under his care in Charing Cross Hospital, and he now presented a water-colour drawing, showing the characteristic features of this disease. The

blood-crusted, intensely pruriginous lesions, the pigmentation of the skin and the enlargement of the lymphatic glands of the groin and the axilla presented a clinical picture simulating in a remarkable degree the the severest cases of prurigo. Anyone seeing such a case for the first time might well say this was a disease very like true prurigo, but no more could be said at the present time than that the likeness was, perhaps, only apparent. In the case to which he alluded the disease was a true lymphadenoma, with pruritus of the utmost intensity and numerous prurigo-like lesions of the skin.

Such developments in the course of Hodgkin's disease were very rare, and true prurigo, also, in this country was very rare, so that the foundation of any argument based on clinical observation must be a small one. But after seeing such a case the thought could not help presenting itself that the pruritus of Hodgkin's disease closely resembled prurigo, being probably due to the poison associated with the lymphomatous growth; in prurigo also a severe toxæmia existed, causing the papular lesions, the changes in the skin, the enlargement of the lymphatic glands, the pigmentation and its other phenomena. It was the custom to ascribe the causation of true prurigo to such influences as insufficient food, neglect of personal cleanliness, absence of hygienic conditions of life. These might all produce severe forms of toxæmia, bringing about a very similar result to the toxæmia of Hodgkin's disease. It should be noted that the papular lesions occurring in the pruritus of Hodgkin's disease apparently did not consist of lymphadenoid tissue such as in the disseminated skin lesions of that malady. The papular lesions of Hodgkin's disease seemed to be inflammatory in character. He considered pruritus and the lesions of the skin to be concurrent signs and symptoms of the toxæmic process.

He would like to say a few words on another subject raised by the President in his paper—namely, on the question of lichenification. He agreed that the processes underlying the changes, very roughly described as eczematization and lichenification, were common to many diseases of the skin observed clinically. The President had suggested the question whether there was any hereditary or underlying tendency which made certain people especially prone to develop lichenification. His opinion was that the processes of eczematization and lichenification were both probably associated with what one might call in a general way "septic conditions of the skin," meaning by that the presence in undue numbers of micro-organisms, possibly with their virulence in a low degree of development, but capable of producing inflammatory

reactions. In eczematization their pathological virulence increased and produced easily recognizable inflammatory changes. In lichenification, on the other hand, the process was not so acute, but stimulation of the skin by friction in any way might produce a certain degree of inflammatory thickening. He thought also that the persons liable to suffer in this way were those whom we loosely described as possessing seborrhœic conditions of the skin. The majority of such patients did not develop the well-marked manifestations of the disease, such as acne vulgaris, to any noticeable extent, but the cutaneous surface harboured more bacteria than was normal, or, at any rate, had less resistance to those present.

Dr. LESLIE ROBERTS (Liverpool) said true prurigo was very seldom seen in Lancashire, and what little he had seen of the disease had been in Vienna. He would like to echo part of the teaching of the great Hebra on the matter, and to beg that in taking views of prurigo speakers would not confine their attention to the lesions merely, or to any special part, but look at the whole condition as a syndrome, or as a group of symptoms, direct and collateral, by which one would gain a definite picture of a certain type of disease, a type which was certainly associated with very marked subjective symptoms, and which was followed by a lesion which, in itself, was not particularly characteristic. But there was one feature which he did not think they had quite sufficiently impressed upon themselves. It was what Hebra himself desired to impress upon his students—namely, that which presented certain characteristic details. If one looked at people suffering from prurigo it would be found that they were generally patients in the early periods of life, that they exhibited malnutrition, defective education, and they probably had a defective mental organization. And when one stripped and examined the whole body it would be found that a very remarkable fact controlled the distribution of the disease. It was that the severity of the disease increased in proportion to the distance from the scalp. The lesions were very slight on the face, a little more marked on the trunk, and became accentuated on the lower part of the abdomen, more so on the thighs, and attained their maximum development on the legs below the knees. Having been taught in Vienna to pay attention to that fact, it appeared to him to be one of the most striking features in the clinical history of the disease. He would like to refer to the subject Dr. Galloway touched on—namely, lymphadenoma associated with pruritus. He remembered a case when he was studying

under his old master, Sir Malcolm Morris, twenty-three years ago, that of a patient with lymphadenoma associated with intolerable pruritus. From his recollection of that case it did not tally with Hebra's prurigo; it presented none of the classical features of that disease. Therefore, he hoped members would have the matter clearly in their minds and not confuse pruritus and pruritic affections with that unfortunate name "pruriginous." Prurigo he believed to be a more or less definite type of disease. He did not lay very much stress on the subdivisions "mitis," "gravis" and "ferox," but he believed that the disease which Hebra described was more or less clearly defined, a definite simple type. Therefore he did not think one could dogmatize on this condition, because nothing was known about the ætiology—i.e., as to whether the pruritus preceded the lesion, or whether the lesion preceded the pruritus. All that could be done was to trust our eyes, and seek all the aid possible from physiological chemistry and any other science which would help in the ætiology.

Dr. WHITFIELD said that he would first deal with the question of the diagnosis between the early stage of prurigo (Hebra) and that of lichen urticatus. It appeared that no one, in England at least, was familiar with the former disease at its onset, almost all the cases having been in existence for some years when first seen by the skin physician. He had, however, rather lately seen a case in a boy in which the disease had been present only about eighteen months and not in a very severe form. What struck him most was the marked pigmentation which was already present even in this mild and somewhat early case. He felt sure that he would have the members present in agreement with him when he said that it was not uncommon to see cases of lichen urticatus which had persisted with very short remissions for a period longer than that quoted in this case of prurigo. Yet he did not remember that he had ever seen a case of lichen urticatus in which diffuse pigmentation, other than that over recent scratch-marks, had existed.

Next he would turn to the symptom-complex which Sir Malcolm Morris had alluded to under the name of "pruriginous eczema." He believed that this was the disease which the French authors described as "*Prurigo à papules grosses.*" The disease was usually of very long standing and was extraordinarily refractory to treatment. As far as he was able to judge the initial lesion was a large papule about the size of a lentil, and consisted of a tense œdema. This was soon

scratched, and exuded bloody serum which dried to a reddish scab. He had had for some years under his care a gentleman suffering from this disease who had also been under Sir Malcolm Morris, and whose tragic history he had often discussed with Sir Malcolm. When he had first seen this patient he had on several occasions examined his blood coagulation time and had invariably found it very slow. He had therefore given him doses of calcium lactate, with such marked success that the patient had written to him to say that he had found the cure. . This was unfortunately not the case, and the benefit derived, though very striking, was not maintained. The patient went at one time to Llandrindod Wells, and again got so much better that his doctor there had written to Dr. Whitfield to say that he was quite well. Again the improvement was not maintained and he became as bad as ever. Some time later he came to London and was taken into a nursing home, and Dr. Briscoe worked at him very carefully by means of Joulie's phosphoric acid estimations. He was found to be very low in both acid and phosphates, and was treated with the appropriate doses until his balance approached the normal closely. This was associated with so much benefit to his health that all his friends remarked upon his appearance, though the actual itching was not equally improved. After a time he slid back into his old state and finally died of pneumonia. He had always been a sufferer from asthma. The history of this case and the temporary benefit derived from both calcium salts and phosphoric acid at different times were very striking, especially as those who had dealt much with blood coagulability and Joulie's method would know that, as Briscoe first pointed out, a low phosphoric acidity was almost invariably associated with a slow coagulation of the blood, and that the administration of phosphoric acid to these patients resulted usually in a hastened blood coagulation. Dr. Whitfield said that in the coagulation the influence of lime was only a reaction almost at the end of a long series of chemical changes. Nevertheless, there was no doubt that within certain limits they could influence the coagulation time by the administration of calcium salts. He thought, therefore, that upon some such lines as those introduced by Wright lay the future investigation of the obscure group of diseases under discussion.

Finally, he would like to allude for one moment to the eruption known as lichenification. He regarded it as a degeneration or premature senility of the skin. It was found most frequently where the skin became senile first or where it was exposed to continued trauma

or infection. If one looked at the back of the neck, a site of predilection for lichenification, in old people it was wonderful how wrinkled and rugose it became. This was probably the result of friction and constant movement. Again, the leg below the knee was peculiarly prone to early senility, and here again lichenification was frequent. It occurred also in the fork, but in this case it was probably more often the result of infection and the irritation of decomposing secretions. As regards treatment, he thought there was probably nothing so good as X-rays. Of radium he had practically no experience, as he did not possess a sufficient amount to treat large areas. He would point out, however, that the advantage lay with X-ray treatment on account of its superior rapidity, it being applicable to large areas at the time, and above all owing to the accurate way in which they could measure the dosage and therefore take up a very small amount of the patient's time.

Dr. STAINER said that when he diagnosed Hebra's prurigo he preferred, when teaching students, to see a generalized eruption, consisting of discrete, shotty papules, with plenty of blood-crusts and glandular enlargement. He had been in the habit of taking notes of the prurigo cases which he saw, and he could give notes of eight consecutive cases. Their ages were: a girl aged 4, a boy aged 6, two boys aged 9, two boys aged 11, a man aged 26. In connexion with the last of these he had the note: "Started at about 14 years of age; no seasonal change—i.e., no difference at any time of the year. Never well; bad itching at night; glands normal." These normal glands at once sounded contradictory, and showed the difficulty of diagnosis. He agreed with Dr. Leslie Roberts that these cases were invariably the subjects of malnutrition, and were miserable specimens of humanity. The children seemed to get over the condition, as he had not seen it often in adult life. With regard to lichenification, he would like to refer to the cases with single marginated patches. He took some observations on that condition, and he had a series of charts, about twenty in number, recording the distribution on the body, and the answers to the questions put to the patients. In three-fourths of the cases the patch was either on the back of the neck or on the inside of the thigh. Where he got a definite answer to his question, it was always that the itching started first, and the rubbing produced the eruption.

Dr. GRAHAM LITTLE said that for many years he had taken an interest in prurigo, which he still preferred to speak of as the prurigo of Hebra, because one knew what that meant. It really did represent a clinical complex. His attention was first specially called to this disease by the difficulty he had in making a diagnosis between it and the disease lichen urticatus, which he saw in such large numbers at the Children's Hospital, Shadwell. With regard to No. 3 of the conclusions, he personally had not been convinced that one could distinguish between lichen urticatus in children of a certain age and the prurigo of Hebra. And his conclusion to that effect had been based upon the extreme difficulty which one experienced in getting a diagnosis expressed at meetings of societies, especially at the old Dermatological Society of London, where he showed many cases with the express object of finding out how the senior men would label certain cases. He concluded that no senior would commit himself to the diagnosis of prurigo of Hebra unless the disease had lasted a certain number of years in the particular patient. He could recall a number of cases, not commencing in infancy, in one of which the condition came on after an attack of scarlet fever in a child aged 6. The first case he saw was at St. George's Hospital, under Dr. Cavafy, who admitted the case, with a very learned note, such as Dr. Cavafy could write. The case came under the care of the senior physician of the hospital, who controverted the diagnosis and said it was scabies. The case prospered exceedingly under the treatment for scabies, and a lecture was given on the case, ridiculing the dermatologist's diagnosis. But it was a case of the prurigo of Hebra, and the eruption returned shortly after the patient went out. From that experience it was evident that the main treatment was rest—in fact, keeping the patient in bed; nothing did so much good for Hebra's prurigo as that. He had known many cases which had been admitted to the wards without the diagnosis being made, and which were greatly improved by the rest in bed. He was interested in Dr. Whitfield's remarks about the Joulie methods; the administration of phosphoric acid was sometimes very valuable. He had satisfied himself that in many cases, not only of the prurigo of Hebra but in other similar conditions, the phosphoric content in the urine was very small, and the administration of phosphoric acid was very beneficial for the relief of the itching. With regard to Question 9 —heredity—it was an extraordinary fact that of the cases of the prurigo of Hebra, not more than one case in ten was found in the British-born — the vast majority were in aliens. That was his

experience at the East London Children's Hospital, Shadwell, where, of course, he had good opportunities for noticing that. He practically never saw the disease at his West End clinic at St. Mary's Hospital. He regarded the racial factor as a very strong one in the causation of prurigo. It was not always easy to differentiate between pruritic eczema and Hebra's prurigo. He had a case which he showed at the Dermatological Society of London ten years ago in connexion with which the subject was very much debated. That woman was still attending his department, and her eruption was in an unaltered condition. He could not yet make up his mind whether it was pruritic eczema or the prurigo of Hebra.

Dr. J. M. H. MacLeod said that he quite agreed that prurigo should be regarded as a distinct affection. He considered that Hebra's name rather complicated the conception of prurigo, as except in Vienna he had never seen a severe case such as Hebra described, and the cases which were generally met with here were of a much milder type.

In his experience prurigo was an exceedingly rare disease, and during the last five years in the Skin Department at Charing Cross Hospital and the Victoria Hospital for Children he had only seen three cases, and they were of a mild type. Two were in young adults and one in a boy, aged 10, in whom it had begun when he was about 5 years old. In neither of these hospitals were there many aliens among the out-patients. He considered that it was doubtful if prurigo ever began in infancy, and that the disease which occurred in infants seemed to him to be papular urticaria.

Prurigo appeared to him to be a peculiar type of reaction of the skin, occurring in a predisposed individual, due to some toxin, probably of autogenous origin, circulating in the blood. In the first instance a toxin of considerable virulence might have been necessary to produce the reaction, but once it had occurred the organism became so sensitized that even slight defects in metabolism or functional derangements of some organ might lead to a fresh outbreak or exacerbation of the condition, till it eventually became a veritable habit of the skin called forth on the slightest provocation. He considered that the question of the priority of the itching and the papule was almost impossible to decide, and that both appeared to him to be symptoms, the one objective and the other subjective, a common cause, and to develop about the same time. He did not consider that scratching or rubbing produced the papules in prurigo.

With regard to lichenification, he believed that a special irritability of the skin was necessary, and that that might be the result of the presence of some itching disease like eczema, or be due to a peculiarly neurotic state of the patient. He agreed that alterations in the skin from malnutrition, old age, or disease might render it more liable to occur, but did not consider it to be essential. He believed that the itching preceded the lichenification, which was the direct result of rubbing. He also had obtained excellent results in the treatment of the affection with the X-rays.

Dr. BUNCH said that even at a children's hospital as large as the Queen's Hospital for Children, where thousands of cases passed through the hospital in the course of a year, the number of typical cases of prurigo was astonishingly small when contrasted with the cases of urticaria. But on looking up the case-sheets of such prurigo cases, it was found that on their first attendance a year or more previously, when the baby was quite small, a diagnosis of urticaria papulosa was, as a rule, made. Indeed, he was not altogether clear as to the exact point at which a prurigo becomes a prurigo; one essential point seemed to be that the disease must have already had a very considerable duration in the patient. Any method of classification or definition which tended to narrow the group of diseases now known as prurigo would be welcome, and he was glad to see that the President proposed to exclude prurigo simplex and the névrodermites. But he was not so satisfied to admit as genuine cases of prurigo those in which the papule preceded the itching. Such an admission would make it very difficult, if not impossible, to exclude from the prurigo group those cases of (originally) papular eczema which showed well-defined lichenification. Thus, in the future, the cases diagnosed as prurigo would greatly increase in number and the cases of papular eczema would largely diminish. Especially would this be the case if it was admitted that prurigo might start late in life. He had copied out of the latest text-book on skin diseases, which he had received only a few days ago, this sentence: "Some of the older writers have loosely applied the designation 'prurigo' to a variety of itching dermatoses." Modern dermatologists must at all costs avoid this pitfall.

Dr. DYSON (Manchester) said that very few cases of prurigo were seen in Manchester, for in the last three years he could recall not more than three instances of it, and the subjects in all these cases were foreigners. Two were children and one was an adult man. He did not think

papular urticaria was less common among the English than among foreigners; but prurigo was certainly mostly seen in aliens. He did not consider that one of these conditions led into the other; urticaria did not precede prurigo. In the case of urticaria the localization was general over the whole body. The localization in the cases of prurigo which he had seen was very definitely on the extensor surfaces, and that was not a peculiarity of urticaria. A question in the paper was: "Does the itching precede the papule, or the papule the itching, or is there no invariable order?" He did not see how that could be answered, because prurigo could not be diagnosed unless the papule were found, and the history was not reliable, because for it one had to depend on the statement of the patient. For lichenification to take place he thought it required some peculiarity in the skin of the patient. Though he could not say he agreed with it, Dr. Galloway had made a suggestion which deserved consideration, namely, that it might be the seborrhœic type of skin which was prone to lichenification. For lichenoid patches he agreed that the best treatment was by means of X-rays.

Dr. Douglas Heath (Birmingham) said he would like to thank the officers of the Section for having extended an invitation to a provincial colleague to take part in this discussion. He thought everyone would agree that a difficult subject had been most ably handled and its conclusions crystallized into shape by Sir Malcolm Morris. The ground covered by him was so very wide that he (Dr. Heath) should carefully avoid being drawn into the conflict of dermatological nomenclature inexactitudes, and should only briefly epitomize a few of his own impressions and observations. In the first place, he had never seen urticaria papulosa (lichen urticatus) develop into prurigo mitis or gravis, and although severe cases of the former disease might closely resemble prurigo, he thought there were always points of difference present by which the two affections could be differentiated. The papule of prurigo was, he thought, nearly always more ill defined and less superficially set than that of lichen urticatus. It did not also so commonly present the yellow point or small vesicle on its summit, and the erythematous zone temporarily present in lichen urticatus was commonly absent in prurigo. The extensor distribution was so well maintained in prurigo that he doubted if it was ever departed from in the mild cases, and it was always highly characteristic in the more severe forms. Prurigo was essentially a regional disease. Urticaria papulosa was a disease

of irregular distribution, and its efflorescence was also variable within certain limits. In prurigo gravis he believed most of the more severe manifestations were due to secondary pyogenic infections as a result of scratching, and his experience taught him that lichenification in this disease, like glandular swellings, could be largely prevented by efficient antiseptic treatment. He always used mercurial lotions and ointments himself, as he did in the treatment of impetigo. Itching in prurigo usually, he thought, began with the starting papule, but lichenification also, he thought, induced itching howsoever produced. With regard to the ætiology of prurigo, he should like to put forward the suggestion—speculative though it was, and the evidence in its favour perhaps only slender—that diminished sweating and sebaceous excretion was in some way the cause in certain types of children. There was no doubt that the skin in prurigo was dry and xerodermia-like. This moderately or severely lichenified skin when hot could obtain no relief for its tense blood-vessels. Everyone was familiar with the discomfort to patients of a non-sweating skin, and in certain persons this discomfort always caused severe itching, which was relieved when sweating took place. In extensive xerodermia, when sweating might be practically absent over large areas of the body, a prurigo-like eruption with deeply seated papules might be sometimes seen. Dr. Heath had several times seen this eruption, and there was no doubt from the scratching that took place that the disease was highly irritable.

In the children of the poor in large cities, washing, except of the face and hands, was often considered by the female parent a quite unnecessary and superfluous performance, and contact with soap and water was rarely indulged in. Added to this the clothes were rarely changed, and the skin had to carry on all its functions against all these difficulties. Did it ever fail to do so, and, failing, did some other development follow? Miliaria from over-wrapping was familiar to all. Had prurigo a somewhat similar origin in a thickened skin, but deeper down? In chronic eczema dry thick papules, never going on to vesicles, appear from time to time, which, like those of xerodermia, resemble prurigo. Why does pilocarpine relieve this phase? Was it for the same reason that this same drug sometimes relieved prurigo? Why did thyroid extract sometimes do so too? (Wolff and others.) How was it that prurigo practically never occurred in the children of the well-to-do, in spite of their patent food and chemist-shop diet? The children of well-to-do Jewish parents did not seem to develop the disease.

It might be argued that the dry, rough, non-sweating skin was the

secondary condition in prurigo—but was it? Was not the papule eruption the *post hoc?*

Sweating was, Dr. Heath thought, generally more free on the flexors than on the extensors. If sweating failed it would probably first lessen on the extensor aspect of the limbs. In xerodermia this was certainly the case. Prurigo practically never attacked the axillæ or the bend of the legs or arms. Was it because sweating was so free in those regions?

Dr. Heath did not wish to maintain that internal causes, such as auto-intoxication, played no part in the ætiology of prurigo, but he suggested that an excretory failure—a dysidrosis—might exist with a failure in the elimination of some toxic bodies.

Did pathological findings in any way support such a hypothesis? Tavernier had found that the vesicles were in relation to the sweat-pore in prurigo, and Unna had observed proliferative changes in the vessel sheath.

Lichenification did not always result from itching; it had been proved to follow pressure and friction on the skin. Lichenification when once set up caused an aggravated itching. Was excretory action—sweating, &c.—practically absent in lichenification? Dr. Heath thought it was.

Dr. G. PERNET said that Sir Malcolm Morris had summarized the complex and chaotic question in such a complete and excellent manner that he had included practically all that could be said on the subject. With regard to the prurigo of Hebra, Dr. Pernet had an opportunity some years ago,[1] when working with the late Dr. Radcliffe-Crocker, to keep under observation in bed in the hospital a very characteristic case of that complaint. Other cases had also attended the hospital, but owing to scarcity of beds they could not be taken in. In that particular case he repeated the work of Jacquet, wrapping up one arm and one leg alternately several times; and there was no doubt that in the covered limbs the papules did not make their appearance. So on that point he considered Jacquet was right. He (Dr. Pernet) verified, to his own satisfaction at any rate, that the itching preceded the formation of the papule in that instance. The patient was a Jewish boy, and he agreed that most of these patients belonged to the Jewish race. But he did not quite agree with Dr. Leslie Roberts that these children

[1] "Dermatological Notebook," 1897-98, iv.

were backward and stupid. This particular boy was very bright and sharp. He considered that prurigo of Hebra was a good name for it, and that it was a definite morbid condition. As a result of scratching it became very polymorphous. His object in speaking was mainly to insist a little more than the President had done on the prurigo or the prurigo-like eruptions of adults. These he designated for his own orientation as *Prurigos diathésiques* of Besnier. The point about these prurigo-like eruptions which began in adult life was their great multiformity. There might be various-sized papules, usually large, or vesiculo-papular conditions; or, again, lichenified patches or impetiginous complications, chronic furunculosis, and so forth—that is, secondary infections. In one such case, though the furunculosis disappeared under vaccine treatment, the prurigo-like condition continued and made the patient's life a burden. In addition there might also be pigmentation, and that he regarded as an important appearance. The great point about these prurigo-like conditions was the intense pruritus, which, during the paroxysms, especially at night, drove the patient frantic; hence a vicious circle. In these patients there was not only a toxic condition, but also a distinct neuropathic soil. In women this *Prurigo diathésique* became more difficult to deal with after the menopause. He had never found in these cases any sugar or albumin in the urine, although he had made frequent examinations. Indican, &c., might be present. In one case, which he had under observation for some time, there was an alternation of asthma with the eruption of the skin.[1] Following upon beneficial treatment from the skin point of view the patient would have an attack of asthma. When the asthma subsided then the skin again became bad, and so on. He desired to call the attention of the meeting to a case which he had observed with the late Dr. Radcliffe-Crocker, that of a man, in whom lichenification of the back of the neck had been followed by generalized true lichen planus. Sabouraud appeared to think that streptococcal infection was the cause of lichenification. He (Dr. Pernet) had tried many kinds of treatment in prurigo and the prurigo-like conditions. Thyroid did good; so also did phosphoric acid. He, like others, had tried calcium lactate without much result. One could not always induce patients to submit to lumbar puncture, which might give relief for a time, but unfortunately did not cure.

[1] Casebook E, p. 190.

Dr. ADAMSON said that he held the view that lichen urticatus of Bateman, prurigo of Hebra, eczema and lichenification of Vidal and Brocq, were definite and distinct affections. They had been separated from the mass of pruritic eruptions after many years of patient observation, and he thought that to try to link them up by intermediate forms could only lead to confusion once more. He was not familiar with the eruption to which the name prurigo mitis had been applied, and he did not know what was meant by pruriginous eczema. Prurigo might be complicated by eczema and so might lichenification, but he did not agree with Sir Malcolm Morris that there were eruptions which could be regarded as intermediate types or which formed connecting links between these quite distinct affections.

Dr. SEQUEIRA said he thought the Society was to be congratulated on the masterly way in which the President had put the facts of this difficult subject before the Section. His own experience was that Hebra's prurigo was a very uncommon disease, and yet at his hospital, the London, there were many foreign patients. He always had cases of Hebra's prurigo about his clinic because he could not cure them, and they were practically all among other than British-born people. He could not add anything to the discussion as to whether there was urticaria preceding the formation of papules, or whether even the itching preceded the papules, because in these cases he had never had the opportunity of seeing the onset of the trouble. Moreover, the information he would be able to get from such a population would be quite valueless. With regard to lichenification, he looked up the notes of a dozen cases of lichenification which he had seen in private, in order that he might go very thoroughly into the causation. Six of the patients were males and six females. The ages of the former ranged between 38 and 61 years, and of the females from 45 to 68. The majority of the females were at about the period of the menopause. Of the cases of lichenification, typical oval patches on the neck, thighs, &c., one case in a man was definitely the result of friction. He was not an obvious neurotic, but he wore a waist-belt, and his patch of lichenification was in the middle of the back, the part constantly rubbed by the waist-belt. Two of the cases were apparently associated with prolonged chronic constipation. Another patient had had an attack following appendicitis, but removal of the appendix did not relieve the lichenification of localized prurigo, although such relief had been hoped for under the belief that the appendix was the cause of the chronic irritation, which

had been very severe, and caused the parts to be torn. With regard to neurasthenia, overwork and a generally worn-out, nervous condition, he had one case in which the woman was worn out by rheumatoid arthritis —a very painful and prolonged case. Another was a patient whom the President had seen and whose nerves were shattered in the Boer War. That man had had recurring attacks of lichenification on the nape of the neck. That followed a period of great nervous depression for which he was now constantly in the hands of nerve specialists. There were three cases of distinct overwork—two in men, one in a woman, a nurse. One of the men was employed in a large railway system. In two of his twelve cases he could not find any obvious cause; there was nothing pointing to grave neurasthenia or a diagnosable nervous condition. He agreed that for the treatment of these conditions there was nothing to equal X-rays or radium. As most patches were of considerable size he preferred to use X-rays. He had been much interested in one or two remarks, especially those about gland enlargements and Dr. Galloway's reference to the association of prurigo with lymphadenoma. He had an extraordinary case in which the glands were very large in many groups, groin, axillæ, &c., and in which there was an extraordinary papular eruption, which was so torn that he did not recognize it, and he thought it was a pruritic eruption associated with lymphadenoma and bossy glands. When he had the patient in hospital the case turned out to be one of severe lichen planus and nothing else.

The PRESIDENT (Sir Malcolm Morris, K.C.V.O.), in reply, said that his object had been fulfilled. The hour and a half had not been wasted, and it would prove to have been useful to hear the various points debated. With many of the things which had been said he agreed, from others he dissented. Among the latter was a remark by Dr. Adamson that the diseases discussed were separate and ought to be put into different boxes. He (Sir Malcolm), however, considered them as links in a chain, which it was necessary to learn and appreciate. The older one got in the work the more one realized that there was no absolute subdivision, as was set out in text-books. One came to appreciate the fact that the labels decided upon did not always cover what was seen. Dermatologists were constantly seeing cases to which they could not give a precise diagnosis; such cases had been brought to the Section time after time, and yet no one ventured to give an absolute opinion. The facts were there, but the cases could not be labelled, because there were so many links. The motive in classifying certain

diseases was to try to link them together. The term "pruriginous" had been criticized, but it was a matter of no moment to him what word was used. He did not agree with Dr. Galloway that these cases of lichenification were seborrhœic. He was aware that certain cases of seborrhœic dermatitis ultimately ended in lichenification, but a large group of cases in which there was lichenification did not begin as seborrhœa, and had nothing to do with it. They were nervous at the start, and began with itching, resulting in scratching. The reason was not understood. Even in old cases, where the skin had been badly torn, a cure was brought about by X-rays or radium, the latter for preference. Ordinary drugs did very little in true lichenification; something more penetrating was required to make the desired change.

Dermatological Section.

July 18, 1912.

Sir MALCOLM MORRIS, K.C.V.O., President of the Section, in the Chair.

Case of Ichthyosis.

By Sir MALCOLM MORRIS, K.C.V.O., F.R.C.S.Ed.

THE patient was a girl, aged 14, who was shown at the last meeting as a case of ichthyosis, when some members regarded it as nævus.[1] He had not seen her since that time, but the mother suggested that she should be seen again, owing to the great change which had taken place from the daily application of an ointment containing 15 gr. of salicylic acid and 15 gr. of precipitated sulphur to the ounce. The condition, it would be remembered, involved the whole of the abdomen, but she was now clear. It was possible that if the treatment were discontinued there would be a recurrence. It would be interesting to hear from those who regarded it as nævus how they would explain such complete disappearance under the treatment he had mentioned. He attributed great importance to the question of symmetry in differentiation.

Case of Nævus Unius Lateris of Skin and Mucous Membrane.

By J. H. SEQUEIRA, M.D.

THE patient, a little girl, aged 8, had suffered from the deformity since birth, but the colour had deepened and the excrescences upon it had gradually increased in size. The upper margin of the nævus began at the root of the nose and extended along the edge of the lower eyelid across to the margin of the hairy scalp above the ear. Its mesial border followed the middle line of the nose to the tip. From there the lower margin passed down the naso-labial sulcus to the left angle of the mouth, and thence obliquely to the ramus of the jaw, which it followed to the

[1] *Proceedings*, p. 154.

front of the auricle. A narrow band passed from the angle of the jaw to the anterior border of the sternomastoid. The surface of the area was irregular, and, in parts, warty and of a dirty brown colour.

At the tip of the nose there was a group of filiform excrescences resembling the digitate or filiform wart. Scattered over the surface there were numerous pinhead-sized to split-pea-sized dark brown warty growths. There were two isolated patches, one at the root of the nose and an elongated warty growth extending from the chin to the suprasternal fossa. On the surface of the latter there were many large filiform masses, some projecting from the surface for a quarter of an inch.

The great interest of the case lay in the fact that the left half of the soft palate was covered with a growth of wart-like appearance, soft, and of the colour of the normal mucous membrane. Some of the projections here were elevations a line to a line and a half in height. On the left upper and lower lips on their mucous surface there were a few isolated filiform projections and also three small groups on the buccal mucosa and on the gums of the left side.

A portion of the area on the face had been treated by radium in doses of 32 mgrm. hours to each square inch and considerable improvement had followed, a sharp reaction beginning on the eighteenth day after treatment.

The exhibitor brought the case as a reply to the suggestion made at a previous meeting that ichthyosis hystrix was a form of ichthyosis. He (the exhibitor) considered that ichthyosis hystrix was always a variety of nævus, and this case, in which both the skin and mucous membrane were simultaneously affected, he held to prove this contention. Vascular nævi of the face commonly included the mucous surfaces also, and in this case of ichthyosis hystrix, or linear nævus, the anomaly included both the skin and mucous membrane.

DISCUSSION.

Dr. COLCOTT FOX said his belief was that these so-called linear nævi might occur on both sides of the body, and so closely together that it became rather difficult to distinguish the condition from ichthyosis. But the skin was not universally involved in nævi as it was in ichthyosis. He considered that a radical distinction. He had always thought that ichthyosis hystrix was a nævus—at least some forms of it.

Dr. WHITFIELD reminded members that the case which he showed last time was bilateral and symmetrical.[1]

[1] *Proceedings*, p. 163.

Case for Diagnosis.

By A. WHITFIELD, M.D.

DR. WHITFIELD had seen the patient, a man, three times, and he had always been brought by a doctor who was one of his late clinical assistants, so that he had not made a complete study of him. He was a confectioner by occupation, and was aged 51. Eight months ago he had what he called a "water blister" on the right eye. Shortly afterwards a similar condition came out under the left eye, and about two months ago he had an eruption on his body. It itched very badly, but subsided somewhat under ointment. When Dr. Whitfield first saw him with Dr. Malcolm he was very much struck by the condition, as the face was covered over with large disks of an urticaria-like eruption. After consideration he made a tentative diagnosis of mycosis fungoides, and suggested the application of X-rays. Those rays had had an extraordinarily good effect where applied. At the area which was worst—namely, under the left malar region—the infiltration had now almost gone. He had not done a biopsy. No lesions had gone except as the result of treatment; they had lasted eight months. Their onset had been slow and insidious, and they spread centrifugally. He had had pastille doses of X-rays. The patient said his skin was very sensitive to touch. Dr. Whitfield had had a private case in which a biopsy was done, and the case carefully studied, so that the diagnosis was firm. All lesions had disappeared with X-rays three or four years ago, and there had been no symptoms since.

DISCUSSION.

Dr. PRINGLE agreed with the diagnosis, and said he had watched a similar case for more than twelve years, in which X-rays were applied frequently as soon as infiltrated or even erythematous patches appeared. The result was the dissipation of the lesions over and over again; but the patient had not been really cured. More permanent benefit appeared to result from the same treatment in a certain number of cases *à tumeurs d'emblée*.

Dr. DORE said he saw, three days ago, the patient referred to by Dr. Pringle. The tumours had disappeared, and he had now erythematous lesions corresponding to those of the pre-mycotic stage.

Case of Acne Scrofulosorum (Papulo-necrotic Tuberculide) with Episcleral Tubercle.

By H. G. Adamson, M.D.

The patient, D. W., a girl, aged 18, had been shown at a previous meeting of the Section on account of the association of acne scrofulosorum with congenital syphilis. The present interest of the case was that the patient had on the right eye what the exhibitor believed to be an episcleral tubercle. There was a leash of blood-vessels running from the internal canthus to the edge of the cornea, and upon this were two raised gelatinous-like masses, one at the margin of the cornea and one midway between the edge of the cornea and the inner canthus. These lesions recalled exactly the "tubercles" which are seen upon the meninges in tuberculous meningitis. Within the gelatinous mass were two or three pinhead-sized, whitish, opaque bodies. As previously recorded the patient had given a positive cuti-reaction to tuberculin. Mr. J. B. Lawford, in the *Transactions of the Ophthalmological Society*, (1909-10, xxx, p. 135), had described an exactly similar case of episcleral tubercle in a boy, aged 10. In this case the nodule was removed and inoculated into a guinea-pig with a positive result, while another portion of the nodule had shown the microscopic characters usual in tubercle. A very beautiful coloured drawing of Mr. Lawford's case was also published in the *Transactions*. The case now exhibited had been seen by Mr. Brooksbank James, who regarded it as tubercle, and Dr. Adamson understood that the patient was receiving injections of tuberculin for the eye condition. On casual inspection the lesion looked like phlyctenular conjunctivitis, but closer observation revealed the fact that it had opaque "tubercles" at its centre. Shallow ulcerations (scrofulous ulcers) upon the cornea were not uncommon in association with acne scrofulosorum in children, but episcleral tubercle appeared to be very rare.

Case for Diagnosis.

By A. M. H. GRAY, M.D.

THE patient, a male, aged 32, had suffered from an irritable eruption since he was 3 months old. The eruption was most marked on the lower limbs, mainly on the extensor aspect, the flexures of the knees and groins being quite free. Lesions were also present over the pubic region, over the whole extent of the back, and on the extensor surfaces of the upper limbs. The head and neck, and chest and abdomen, were free from eruption, as were the flexor aspects of the upper limbs. There were some lesions on the anterior and posterior borders of the axillæ. The lesions consisted of medium-sized pale papules, which showed a distinct tendency to grouping. When first seen, two years ago, marked scratch lesions were present, and numerous follicular pustules, mostly on the lower extremities, which almost masked the characteristic lesions. At that time the patient also showed a circinate syphilide in front of the upper part of the right thigh, and gave a history of syphilitic infection ten years previously; the Wassermann reaction at that time was positive. Considerable pigmentation was present on areas named, and especially on margins of axillæ and on outer side of hips. The eruption was intensely irritable. The glands in groins and axillæ were markedly enlarged.

The exhibitor considered it to be a case of prurigo on account of the history and distribution of the eruption. He pointed out that the patient was an Englishman, that he was well nourished, and that the condition had undoubtedly improved while under observation.

DISCUSSION.

Dr. WHITFIELD and Dr. PERNET considered the case as one of dermatitis herpetiformis.

The PRESIDENT (Sir Malcolm Morris, K.C.V.O.) also inclined to the view that it was dermatitis herpetiformis, and said he had seen that condition in a patient as young as 6 years of age. If the condition in this present patient began in infancy it was rather remarkable for dermatitis herpetiformis. He had never known the disease begin at such an early age.

Dr. MACLEOD considered that the grouping of the lesions and the tendency to vesiculation in some of them, combined with the absence of lichenification, pointed to the case being one of dermatitis herpetiformis. He had seen a case in a boy, aged 9, which presented similar clinical appearances.

Dr. ADAMSON thought that the fact that the flexures were involved was against the diagnosis of the prurigo of Hebra. He also pointed out that the pinhead-sized vesicles with a tendency to arrangement in groups could be seen on close inspection, and this suggested chronic eczema or dermatitis herpetiformis rather than prurigo.

Keloids of the Ears following Piercing for Ear-rings.

By J. M. H. MACLEOD, M.D.

DR. J. M. H. MACLEOD showed specimens of two keloids which developed in the ears of a woman as the result of being pierced for ear-rings eight months earlier. There was no unusual soreness, and the rings were soon removed (within two months) owing to the development of the swellings. These, however, continued to grow. The specimens were kindly lent to him for exhibition by his surgical colleague, Mr. Stanley Boyd, who had excised the lesions. The keloids were both round, one being about the size of the kernel of a filbert-nut and the other the size of a pea. They were well defined and easily dissected out. The patient had shown no tendency to keloidal formation previously. The keloids were removed on May 6, 1912, and there had been no recurrence (July 30).

Two Cases of Summer Eruption.

By HALDIN DAVIS, F.R.C.S.

THE patients were two girls, aged 7 and 11 respectively. They were brought to illustrate two somewhat different types of summer eruption—one very common, in which papules and tiny vesicles appear on the face and hands, but disappear during the winter without leaving any trace; the other more severe, of which the younger child was an

example, in which the lesions were larger vesicles which became confluent and purulent, and which left permanent scarring both on the face and hands. The blood had been examined from the more severe case, and it was found that of the white corpuscles present no less than 10 per cent. were eosinophiles.

The PRESIDENT said he always told such patients to rub in red ointment before going out in the morning. Sometimes those who went in for mountain-climbing in Switzerland got the condition.



Case of summer eruption. Lesions on the hands. The old scars can be seen.

Drawing and Photograph of a Case of Mycosis Fungoides d'emblée.

By G. PERNET, M.D.

THE patient being too ill to be brought before the Section, Dr. Pernet showed a water-colour drawing and a photograph of the case, for which he was indebted to his friend Dr. Martin Randall, of Wimbledon, who had sent the patient on to him for diagnosis. The patient was a perfectly healthy woman, aged 44, who six months previously developed a

small growth over the left tibia. This had been followed rapidly by others, one on the front of the left knee being characteristic, and the size of half a fair-sized tomato. There had been absolutely nothing the matter with the skin; nothing in the shape of a premycotic dermatitis. As the details of the case would probably be published later, Dr. Pernet would hold them over for the present.

The PRESIDENT reminded members of a classical case in which some of the lesions were mycosis fungoides d'emblée and others had a premycotic stage; it was a mixed case.

Announcement.

THE PRESIDENT announced that the Council of the Section had decided to hold an evening discussion on November 21, on the subject of "Erythema multiforme," which would be opened by Dr. Adamson.

PROCEEDINGS

OF THE

ROYAL SOCIETY OF MEDICINE

VOLUME THE FIFTH

COMPRISING THE REPORT OF THE PROCEEDINGS FOR THE SESSION 1911-12

ELECTRO-THERAPEUTICAL SECTION



LONDON
LONGMANS, GREEN & CO., PATERNOSTER ROW
1912

Electro-Therapeutical Section.

OFFICERS FOR THE SESSION 1911-12.

President—

ARCHIBALD D. REID.

Vice-Presidents—

W. IRONSIDE BRUCE, M.D.
C. THURSTAN HOLLAND.
REGINALD MORTON, M.D.

Hon. Secretaries—

G. HARRISON ORTON, M.D.
CECIL R. C. LYSTER.

Other Members of Council—

NORMAN E. ALDRIDGE, M.B.
C. F. BAILEY, M.D.
A. E. BARCLAY, B.C.
FRANK FOWLER, M.D.
ROBERT KNOX, M.D.
CHARLES J. MORTON, M.D.
S. GILBERT SCOTT.
G. ALLPRESS SIMMONS, M.D.
S. SLOAN, M.D.

Representative on Library Committee—

H. LEWIS JONES, M.D.

Representative on Editorial Committee—

W. DEANE BUTCHER.

ELECTRO-THERAPEUTICAL SECTION.

CONTENTS.

October 20, 1911.

PAGE

Presidential Address: Survey of the Year's Work in Electro-Therapeutics. By A. D. Reid 1

November 17, 1911.

Radiography in Intestinal Stasis. By Alfred C. Jordan, M.D. 9

December 15, 1911.

Demonstrations by :—

(I) N. E. Aldridge, M.B. 49
(II) W. Ironside Bruce, M.D. 50
(III) G. E. Bowker, M.D. 53
(IV) H. Lewis Jones, M.D. 55
(V) C. J. Morton, M.D. 55
(VI) A. D. Reid 56

January 19, 1912.

The X-ray Prognosis of Fractures. By Charles James Morton, M.D. ... 57

The Importance of Stereoradiography, especially of the Alimentary Tract, with Demonstration of Plates. By James T. Case, M.D. 73

February 16, 1912.

Secondary X-radiations: Their Uses and Possibilities in Medicine. By Francis Hernaman-Johnson, M.D. 87

A Method of Reducing Excessive Frequency of the Heart-beat by means of Rhythmical Muscle-contractions Electrically Provoked. By W. Hampson 119

March 15, 1912.

PAGE

The Radio-therapeutic Treatment of Uterine Fibroma. By Dr. H. BORDIER (Lyons) 125

April 19, 1912.

The Treatment of Rodent Ulcer. By REGINALD MORTON, M.D. 141

The Clinical Use of the Active Deposit of Radium. By C. R. C. LYSTER and S. RUSS 150

Demonstration of a New Compressor. By REGINALD MORTON, M.D. 155

Skiagram of a Case of Separation of the Lower Epiphysis of the Femur. Shown by G. HARRISON ORTON, M.D. 156

The Council think it right to state that the Society does not hold itself in any way responsible for the statements made or the views put forward in the various papers.

JOHN BALE, SONS & DANIELSSON, LTD., 83-91, Great Titchfield Street, London, W.

Electro-Therapeutical Section.

October 20, 1911.

Mr. A. D. Reid, President of the Section, in the Chair.

PRESIDENTIAL ADDRESS.

Survey of the Year's Work in Electro-Therapeutics.

If I were to attempt to give you a complete résumé of the advances made in medical electricity during the past year, I realize that I should be undertaking a task which would occupy a much longer period of time than the scope of an address of this kind should allow. I propose, therefore, to make a general survey of the year's work, as far as I have been able to follow it from the publications at my disposal, and very briefly to comment on what seems to be of special interest, and draw what seems to be logical conclusion.

Abroad there have been the International Congress of Radiology and Electricity at Brussels, September, 1910; the Fifth International Congress of Medical Electrology and Radiology, September, 1910, Barcelona; Twentieth American Electro-Therapeutic Association; German Röntgen Congress, April, 1911; Third French Congress of Physiotherapy, April, 1911; French Association for Advancement of Sciences; whilst at home there have been the two meetings of the Section of Radiology and Medical Electricity of the British Medical Association, and the meetings of our own Society.

There are times when we may think that the limit to the possibilities of any branch of medical science has been reached, and when we may think that we cannot hope for any further advance in our branch; but the most cursory glance at the titles alone of the articles and papers published during the last year make it obvious that although a vast quantity of work has been done, we are not much more than on the fringe of possibilities previously unthought of.

Looking through the publications and records of the last year, it is a task of some difficulty to make a selection for special mention, not on account of any paucity of good material, but rather on account of its quantity. Speaking broadly, there have been several general subjects which have been much written about and discussed.

In the field of physics cognate to our branch, there have been many admirable and helpful papers—notably, those of Sir J. J. Thomson at the Section of the British Medical Association last year on "Röntgen Rays in Therapeutics," with the modest sub-title "A Suggestion from a Physicist," in which he emphasized the difference of absorption by hard and soft rays, with reference to their use for treatment. His opinion was that the soft rays, owing to their greater facility for absorption, must have much greater physical and physiological effects than the hard rays. He referred to the work of Professor Barkla's discovery of the secondary rays produced by the impact of the ordinary X-rays on different metals, and the constant quality of these secondary rays according to the metal used, and threw out the suggestion that these secondary rays had possibilities in medical treatment. This suggestion has been followed up and embodied in an admirably worked-out paper by Dr. Hernaman Johnson, read at the Section of Electro-Therapeutics and Radiology of the British Medical Association, in 1910. Touching this matter, the production of the Lindemann tube promises to be of great value in the production of the softer rays, and this opens up a field for research which may give us better results in treatment of superficial disease.

Sir Oliver Lodge's paper at the Section of the British Medical Association this year was a fascinating account of the conveyance of electricity through solids, liquids, and gases, which will be read with the greatest interest by everyone who is interested in electrical phenomena, and more especially those who have the X-ray tube as their constant daily companion.

Professor William Bragg's paper at the Royal Institution, in January, 1911, on "Radio-activity as a Kinetic Theory of a Fourth State of Matter," gives an account of his most interesting experiments in support of his theory, which well repays reading and re-reading.

The theory that the action of the electric and radio-active treatment of disease is due to the production or liberation of vaccines is one of great interest, and Dr. Deane Butcher made out a very good case for this hypothesis in his paper during the discussion on vaccine therapy at a series of special meetings of the Society last year. The fact that so many diseases of such diverse characteristics have reacted satisfactorily to these agents is, in itself, the strongest evidence in favour of this idea.

The main points in Dr. Butcher's paper may be summarized thus: The action of radio-active treatment is biological, not merely destructive or congestive, and the cells of the body are stimulated to produce an antitoxin. He instances the cure of rodent ulcer by such diverse means

as X-ray, radium, ultra-violet light, high frequency effleuve, and zinc ions. In the first two, at any rate, the process is not that of destruction, but of repair. It states his belief that thermo-penetration and muscular exercise electrically produced act in the same way, and suggests the ultimate possibility of producing or controlling auto-vaccination by electrical means. While it must be admitted that the evidence is too meagre for a definite pronouncement as to the accuracy of the theory, the action of the X-rays in leucocythæmia and the profound change in the blood produced thereby is difficult to explain by any other theory. This is a subject that will well repay further research with the aid of the pathologist, and I have no doubt that it will be followed up further, and I hope that before the end of our year's work we may be able to get some communications to our Section on this very important development.

With regard to the branches of our own special work. These fall into four divisions—radio-diagnosis, radio-therapy, radium-therapy, electro-therapy—and I propose very briefly to mention what seem to me to be the most salient points in the advances in the first two only, because the two latter branches have been so fully discussed at the meeting of the Section of the British Medical Association at Birmingham.

Radio-diagnosis.

We find many aids to the above in the improved forms of apparatus which have been produced. The improvement in the manufacture of coils and the advent of the transformers have provided an output of satisfactory electrical energy up to and exceeding our requirements, and both instantaneous and even cinema-radiography are now no longer wished-for possibilities, but established facts. The latter method is unfortunately of necessity a procedure which is out of the reach of most of us, but the interest of its successful production, which has been sought after so long, is undoubted. The quality of the tubes manufactured is improving steadily, and of the accessory technical apparatus we have a great number of interesting appliances—notably, automatic plate-changing plate-holders, which enable us to take stereo-radiograms in the space of a second, or less. This latter process will undoubtedly be used much more universally in the future, more especially in the examination of the chest and the gastro-intestinal tract, and from what has already been accomplished in this line we may hope for early and more accurate diagnosis in the X-ray examinations of these regions.

I do not intend here to enter into any discussion as to the relative value of coil and transformer for radiographic work. This has been too

lately discussed in the Section to require any further comments. It may be, however, that in the not far distant future a further discussion in the light of increased practical knowledge will be of interest.

I might mention, in relation to the transformer, that there are great possibilities in the method of stereo-fluoroscopy by the utilization of the two phases acting intermittently, as published by Dr. Pirie. This method of examination is one that I worked on for a long time before the advent of the transformer, but which never proved satisfactory in my hands.

The examination of the alimentary canal by means of bismuth has been, I think, the field in which most progress has been made, and a large number of communications of great interest and value have been the result. Dr. Groedel, whose admirable paper and demonstration we had the pleasure of hearing at our Section before we left Hanover Square, gave a summary of the advances in the Röntgen diagnosis of diseases of the stomach and bowels, which was published in July, 1910.[1] Since then there have been several further communications, of which I may mention the following: "Diagnosis of Hour-glass Stomach," by Dr. Hertz, giving an account of hour-glass contraction which the X-rays failed to reveal, and the causes of the failure;[2] "Gastric Radioscopy," by Dr. Barclay, with his technique and conclusions; "Œsophageal Peristalsis," by Dr. A. C. Jordan; "Some Intestinal Cases," by Dr. A. C. Jordan; "X-ray Appearances of Hour-glass Stomach," by Mr. Thurstan Holland; "Röntgen Diagnosis of Chronic Gastric Ulcer," by Dr. Martin Haudeck, giving an account of seven cases where the diagnosis was made and confirmed by operation; "Mobility of Stomach after Gastro-enterostomy," by Ribas y Ribas. Œsophageal constrictions can be accurately located and the diagnosis between spasm and malignant disease generally made, and peristalsis in the œsophagus can be demonstrated. The size, shape, and mobility of the stomach and intestines can be accurately determined, and in many cases a definite diagnosis made in cases of carcinoma and gastric ulcer. The appendix can be demonstrated in some cases and the peristaltic movements of the intestine can be watched.

From the point of view of hospital work, the increase of this work demands a great deal of the time of those who undertake it, and the example of one of our great hospitals, which many years ago recognized the necessity of separating medical and surgical radiography, is one that will have to be extensively followed by others if the work is to be adequate and satisfactory. It is a serious problem how the vastly

[1] *Archives of Roentgen Ray*, 1910-11, xv, p. 51.
[2] *Ibid.*, p. 127.

increasing demand for screen work, both in these examinations and in the examination of the thorax, is to be adequately met.

The inducement at present offered to medical men to take up this work, which under the best conditions is one of danger to health, is at present totally inadequate, and we are conscious of the fact that at present very few new names are known to us as entering this branch. Several of the smaller hospitals find it impossible to get medical men to undertake the charge of their departments, and undoubtedly there will be not only a shortage but a dearth of men who will be willing to run the risk of devoting their lives to radiology.

To return from this digression. Another subject, namely, the diagnosis of early pulmonary tuberculosis, is one in which there has been some advance, and the details of the best methods of making the examination, are well set forth in a paper by Dr. Orton,[1] and in the discussion of Dr. Knobel's paper at the British Medical Association. The general tendency is to put the screen examination at a higher value than the plate. Though the limitation of the diaphragmatic movement is a sign of great importance, and at one time was considered the earliest sign, the deficiency of illumination of the apex, in the opinion of most observers, has taken a prior place.

It is hoped that we may be able to have a full discussion on the subject in the ensuing year, and that we shall all benefit thereby.

Pyelography.

The development of the X-ray examination of the pelvis of the kidney after injection of collargol, which was first introduced by Voelckler and Lichtenberg, is another valuable aid to diagnosis. Dr. Bruce, at the British Medical Association at Birmingham, gave a full account of the method employed by him and Mr. Thomson Walker. There are still many directions in which improvements in technique may develop. Cerebral tumours are still elusive unless they involve the bony structures of the skull. Gall-stones are still, in most cases, beyond the possibility of diagnosis. Uric acid calculi remain the bane of the radiographer. The line of discovery that we wish to see successfully followed up, therefore, is the differentiation of structures of great similarity in density. This is an object which forms a most attractive study. Dr. Rosenthal, of Munich, at the Berlin Röntgen Congress, in April, 1911, read a paper on this subject, and exhibited skiagrams of the hand showing normal blood-vessels, and many other radiographs of great interest.

[1] *Archives of Roentgen Ray*, 1910-11, xv, p. 323 ; *Brit. Med. Journ.*, 1910, ii, p. 529.

Radio-therapy.

Before reviewing this, we welcome the appearance of some new instruments for measurement of quantity and quality of X-rays.

The milliampere meter, as Dr. Wertheim Salomonson has proved in a recent paper, only enables us to reproduce a known dose of X-rays, and is not to be relied upon for accuracy.

Bauer's qualimeter, which was shown at our Section in February,[1] has now been used for some time in this country and gives us a visible record of the condition of the tube during the whole time that it is running, and allows us to keep the vacuum in the condition that we require.

With regard to quantimeters in this country, preference, I think, is still given to the Sabouraud-Noiré pastille dose, and in this connexion there are two improvements in technique, described by Dr. Pirie and Dr. Hampson, which were both primarily devised for the treatment of ringworm, but of course are both applicable for the treatment of any skin condition. Both methods have the following points in common: (1) The pastille is placed *on the skin;* (2) the pastille is examined by artificial light; (3) less distance between anode and skin; (4) reduction in time for producing full dose. In Dr. Pirie's method it is necessary by experience to learn the quarter B tint, and it is not possible to use already exposed pastilles. In Dr. Hampson's method he uses half the distance as a rule and has the standard tints, twenty-five in number, arranged round a small circle, which rotates beneath a black card, in which a window allows a comparison of the standards with the exposed pastille. The epilation dose corresponds to any six intervals on the scale, so the same pastille can be used for four successive doses. Three or four minutes have sufficed to complete the dose, according to distance—10·6 cm.

The new model of Bordier's radio-chronometer is an improvement on the old one, but the small number of tints and the daylight examination are ob ections.

General Technique of X-ray Therapy.

There is a definite divergence of opinion on the subject of the quantity and frequency of dosage. In the early stages of its existence the tendency of radio-therapy was to give small doses frequently repeated. This was, at first, purely empirical, and the dosage was, therefore, unequal and unscientific. When the different form of measurements appeared there was a gradual change to bigger doses less frequently repeated, and the use of filters enabled these doses to be repeated more frequently. At the present there is a movement in favour of returning to the smaller doses again.

[1] *Proceedings*, 1911, iv, p. 71.

X-ray Treatment of Malignant Disease.

This has been so recently discussed that I only wish to say one thing, namely, that the introduction of its treatment by large doses of radium has at present thrown this method into disrepute. It may be that the results of the application of radium may be better and more permanent than were obtained by X-rays; we sincerely hope that they may, but the fact remains that there have been reported very many cases of undoubted recurrence of malignant disease where the recurrence has absolutely disappeared and the patient remained apparently perfectly well for a number of years. Immediate irradiation after operation is the logical sequel to the established fact that superficial recurrences can be removed by X-rays.

Professor Leduc, whose communications discard the filter, advises irradiation in different directions to protect the skin and attack the deeper parts, leaves a fortnight's to a month's interval, uses no localizer, continues applications for months or years, and absolutely disapproves of post-operative radio-therapy; but if operation be thought necessary, advises treatment before operation. Although any communication of his is received with respect, I think that he will have many opponents on the latter score.

The fact that cancer is a purely local disease at its inception, and the large percentage of non-recurrences after operation, will certainly weigh heavily against the doubtful chance of *cure* by radio-therapy.

Treatment by means of Secondary Radiation.

This opens up an entirely new ground for research, and I think that it has possibilities that will make it worth our while to experiment further, and I have no doubt that it will be followed up with interest.

Treatment of Uterine Myomata.

Although it has been known for some years that uterine myomata have been influenced by the X-rays, it is only recently that the technique has been perfected, but enough has been done to prove its great value in the treatment of this condition. Foveau de Courmelles, Albers Schönberg, Dessauer, and Bordier are the names to whom the credit is due for this addition to the aids already given by radio-therapy to medicine.

Dr. Bordier's paper, published in August last,[1] gives the exact details which he uses, and the history of eighteen cases treated with good results.

[1] *Archives of Roentgen Ray*, 1911, xvi, p. 92.

A further communication by Dr. Curtis Webb, in conjunction with Dr. Bordier, appears in the *Clinical Journal.*[1] Professor Krönig and Dr. Gauss give the results of carrying out the treatment by Albers Schönberg's method in sixty-three cases, and analyse their results. Total amenorrhœa was produced in 60 per cent. and partial in 30 per cent. They recommend surgery in otherwise healthy individuals to whom time is an object, and radio-therapy in all other cases.

This method of treatment is now beyond the experimental stage, and may be said to be established as an alternative to extirpation in suitable cases.

I will not weary you with platitudes on the present price and shortage of radium and the subsequent difficulty of obtaining it, but I note with great interest the possibilities of mesothorium described in a lecture by Professor Rutherford. It emits beta and gamma rays and decays to half its value in five and a half years. It emits initially no alpha rays, grows radiothorium in a few years which in turn emits alpha rays, changes into thorium X, and then into thorium emanation. It increases in activity for three and a half years, arriving at one and a half times its value. Its beta rays have equal and the gamma slightly more penetrating rays than radium. It is obtained from the residues from the extraction of thorium, and there is unlimited scope for its production, and if it can be obtained at a reasonable price we shall have the opportunity of testing the possibilities of treatment with larger quantities of radiant material than we can hope to have with radium as the source of energy.

Before I close my remarks I have a few suggestions to make for the coming year. We hope to elaborate the clinical evenings and welcome the exhibition of skiagrams of cases of doubtful diagnosis, and we intend to give notice of some chosen subjects which will enable our members to add any cases of their own which may bear on the subject chosen. These will be duly announced.

Another proposal which we hope to be able to elaborate further is to commence to form a series of skiagrams to be kept in the Library of the Royal Society of Medicine for reference. These skiagrams, it is suggested, shall consist of prints or transparencies of normal bones and joints at different ages, and also of pathological conditions. We propose to form a sub-committee to work out details which will be submitted to you later, and it is hoped that members will assist in undertaking some part of this work which will in time form a collection of great value to all who are interested in X-ray diagnosis.

[1] *Clin. Journ.*, 1911, xxxviii, p. 187.

Electro-Therapeutical Section.

November 17, 1911.

Mr. A. D. Reid, President of the Section, in the Chair.

Radiography in Intestinal Stasis.

By Alfred C. Jordan, M.D.

Intestinal stasis is a chronic disease due to the retention for too long a time of the intestinal contents, and the toxic absorption resulting from this undue retention. The symptoms may be slight or severe, but they are very definite. They may be enumerated as follows: Abdominal pain; often severe. General depression and want of energy, which may be so extreme as to make life intolerable. Poor appetite, with attacks of nausea or vomiting; a bad taste in the mouth; breath of an unpleasant odour. Headache, backache, muscular pains and aching in the joints. Cold hands and feet and other signs of a poor circulation. Constipation is usually a marked symptom; often it persists in spite of all treatment, the patient being compelled to rely on purgative drugs and injections. Flatulence is always present, and leads to attacks of abdominal distension. There is tenderness to pressure over the abdomen, especially in certain regions, of which the right iliac fossa is the most usual. The skin is stained, sometimes to a deep brown colour; it feels unhealthy, as though wanting in elasticity; the sweat has a disagreeable odour. The breasts show changes such as are usually described as chronic mastitis; in more advanced cases cystic degeneration takes place, and the breasts are then in a condition in which a transition to cancer is very liable to occur. Similar changes

are produced in other glands, and at operations on these patients chronic pancreatitis is found to exist; in many cases cholecystitis is present also, and gall-stones are very often found.

There can be few diseases in which the symptoms and signs are more definite or more constant, and yet this condition of intestinal stasis was not recognized or understood until Mr. W. Arbuthnot Lane

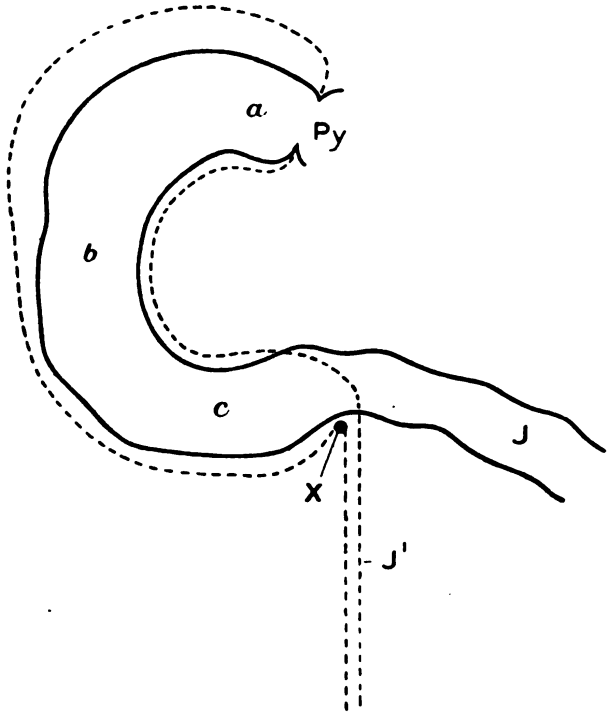


FIG. 1.

Diagram to illustrate the normal duodenum and jejunum (firm line) and the dilated duodenum and pendulous jejunum (dotted line) due to kinking, at the duodeno-jejunal junction, by the sharp lower margin of a peritoneal fold. indicated by the point *X*. The distension of the duodenum is most marked in its first part, where it is least supported, and has a complete peritoneal covering. It is in this part that inflammatory changes are set up in consequence of the abnormal distension. *Py.*, pylorus; *a*, *b*, *c*, first, second and third parts of the duodenum. J., J.', jejunum in its normal and in its pendulous position.

described it; and it is to his accurate and thoughtful observations that we owe our present knowledge of the causes, symptoms, and treatment of intestinal stasis. Mr. Lane has described the anatomical changes

that lead to the disease, and the secondary effects that follow.[1] It is in this part of the work that I have been able to bring the resources of radiography to his aid by obtaining upon the fluorescent screen, and upon photographic plates, actual irrefutable ocular evidence of the reality of the changes under consideration.

All mammalian animals retain fæcal matter in the large bowel, and are subject to some amount of intestinal absorption. In the view of Metchnikoff this absorption of toxins causes changes which shorten their lives.

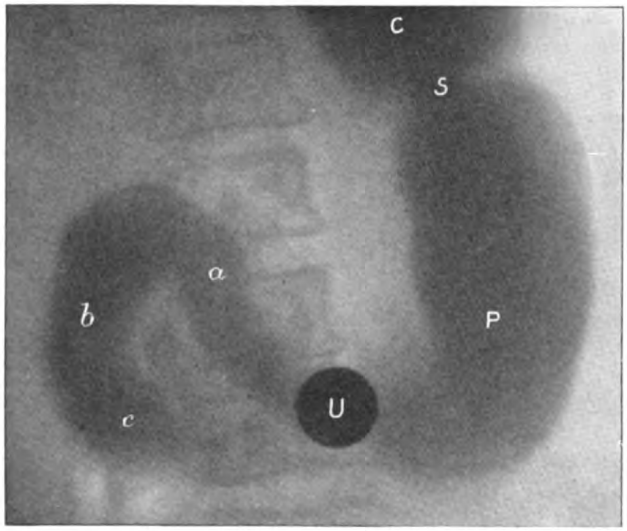


FIG. 1A.

Stomach and duodenum after a bismuth meal, showing a spasmodic hour-glass constriction of the stomach, and delayed passage through the duodenum. *C.*, *P.*, cardiac and pyloric portions of the stomach, separated by a spasmodic constriction at *S*. *a*, *b*, *c*, first, second and third parts of the duodenum. *U.*, umbilicus, marked by a penny.

EFFECTS OF THE UPRIGHT POSTURE.

In the human subject the position of the abdominal viscera is altered fundamentally by his adoption of the upright posture. Man gained enormous advantages thereby, but as usual it carried with it certain disabilities, and one of the greatest of these is due to the

[1] *Brit. Med. Journ.*, 1911, i, p. 913.

effect of gravity, which causes the heavier of the abdominal viscera to fall, and in so doing to pull upon their attachments. This tendency of the viscera to fall is accentuated by civilization, for the upright posture, whether in standing, sitting, or walking, is maintained all day—i.e., for seventeen out of the twenty-four hours—and is only relaxed when the subject is in bed.

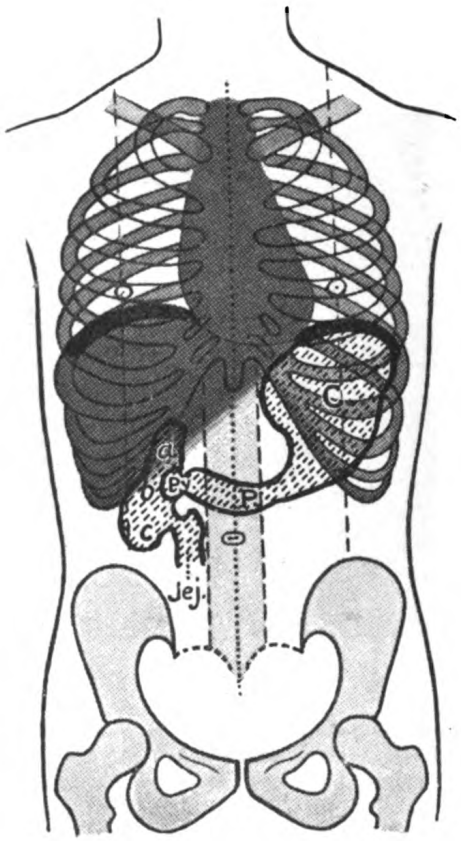


FIG. 2.

Stomach and duodenum, showing an early stage of kinking at the duodeno-jejunal junction. Note the jejunum passing vertically downward at its commencement. *a*, *b*, *c*, first, second and third parts of the duodenum. *Py.*, pylorus; *Jej.*, jejunum; *C.*, *P.*, cardiac and pyloric portions of stomach.

"VISCEROPTOSIS."

The heaviest parts of the alimentary canal are the stomach, when full of food, and the large intestine when loaded with fæces, and the

tendency of these parts to drop is well known. The name "viscero-ptosis" is often used to denote the fact that the viscera (or some of them) are dropped: in this sense there is no harm in using the word, though it is a clumsy word, and is not needed. Unfortunately it is often used to designate other conditions supposed to be associated with dropping of the viscera; more particularly weakness of the muscles of the abdominal wall, with bulging of the lower part of the abdomen.

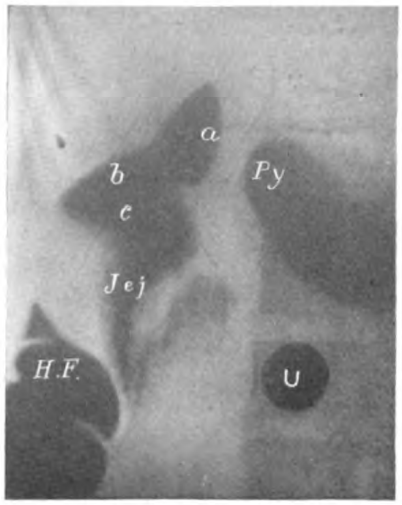


FIG. 3.

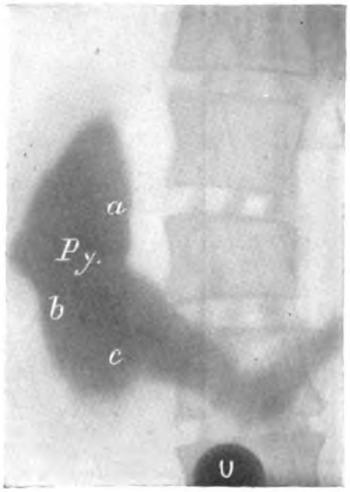


FIG. 4.

Fig. 3.—Stomach and duodenum seven hours after a bismuth meal, showing an early stage of kinking at the duodeno-jejunal junction. Note the jejunum passing vertically downward at its commencement. At the time of taking this skiagram a quantity of bismuth had passed through to the hepatic flexure. The delay in the stomach and duodenum is due solely to the duodeno-jejunal kinking; *a*, *b*, *c*, first, second and third parts of the duodenum; *Jej.*, jejunum; *Py.*, pylorus; *H.F.*, hepatic flexure; *U.*, umbilicus, marked by a penny.

Fig. 4.—Duodenum showing well-marked kinking at the duodeno-jejunal junction. The duodenum is distended. It was seen to be contracting actively against the obstruction produced by the kink. *a*, *b*, *c*, first, second and third parts of the duodenum; *Py.*, pylorus.

Now I find that in many of the most extreme cases of dropping of the viscera there is absolutely no tendency for the abdomen to be prominent in its lower part; the abdomen is perfectly flat when the patient stands. When flatulence is severe there may be some *general* prominence of

the abdomen. Hence it is better to refer to the condition as "dropping" of the particular viscera concerned.

Anatomical Changes accompanying Dropping of the Viscera.

To Mr. W. Arbuthnot Lane we are indebted for demonstrating and explaining the mechanism of the changes brought about by the dropping

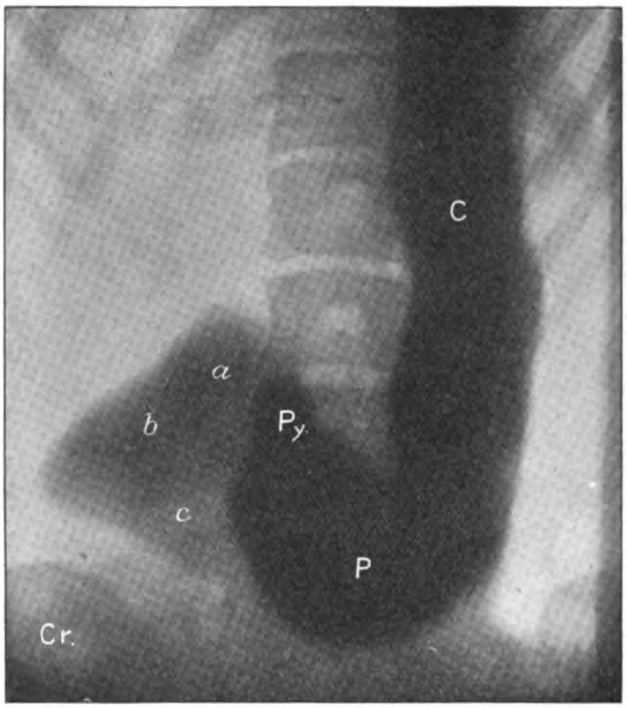


Fig. 5.

The long dropped stomach and the duodenum in a case of suspected duodenal ulcer, examined on the couch after a bismuth meal. The behaviour of the duodenum was similar to that in fig. 6, though less extreme. *C.*, *P.*, cardiac and pyloric portions of the stomach; *Py.*, pylorus; *a*, *b*, *c*, first, second and third parts of the duodenum; *Cr.*, crest of ilium.

of the viscera.[1] When the viscera begin to drop they exert a drag upon the mesentery; this results in the formation of thickened bands in the mesentery. These bands become so thick and strong in certain

[1] *Surg., Gynæcol., and Obstet.*, Chicago, 1910, xi, pp. 495-500.

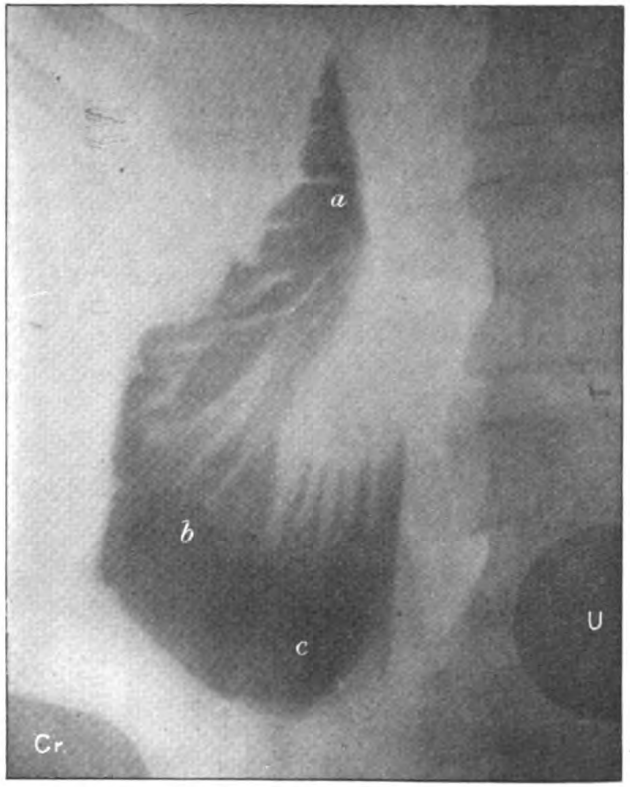


FIG. 6.

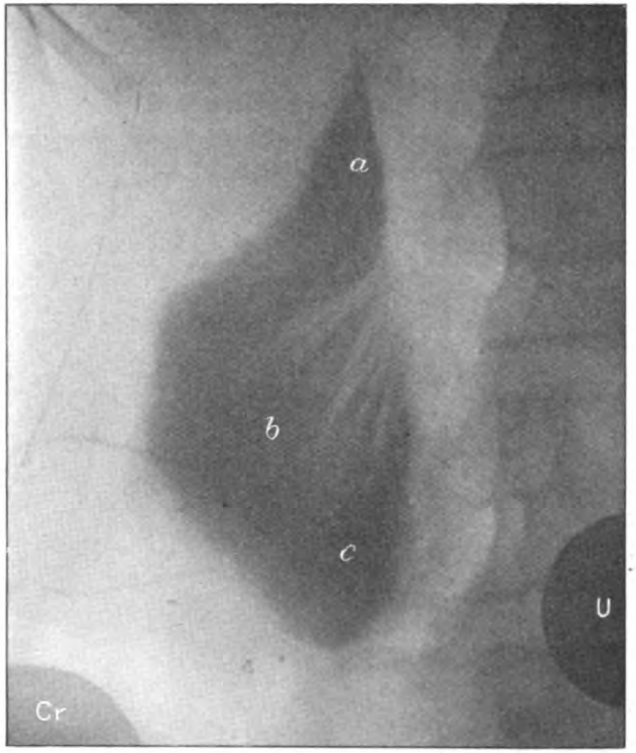


FIG. 6A.

Figs. 6 and 6A.—Duodenum in a girl, aged 19, showing two phases of the powerful contractions by which it sought (in vain) to overcome the effect of kinking at the duodeno-jejunal junction (confirmed by operation). The duodenum appeared to be writhing. *a*, *b*, *c*, first, second and third parts of the duodenum; *Cr.*, crest of ilium; *U.*, umbilicus, marked by a penny.

cases and in certain parts as to form supplementary mesenteries, which support the bowel and tend to prevent the dropping. These "adhesions" are entirely non-inflammatory in origin; they are brought into being as the direct result of preternatural strain, just as in certain

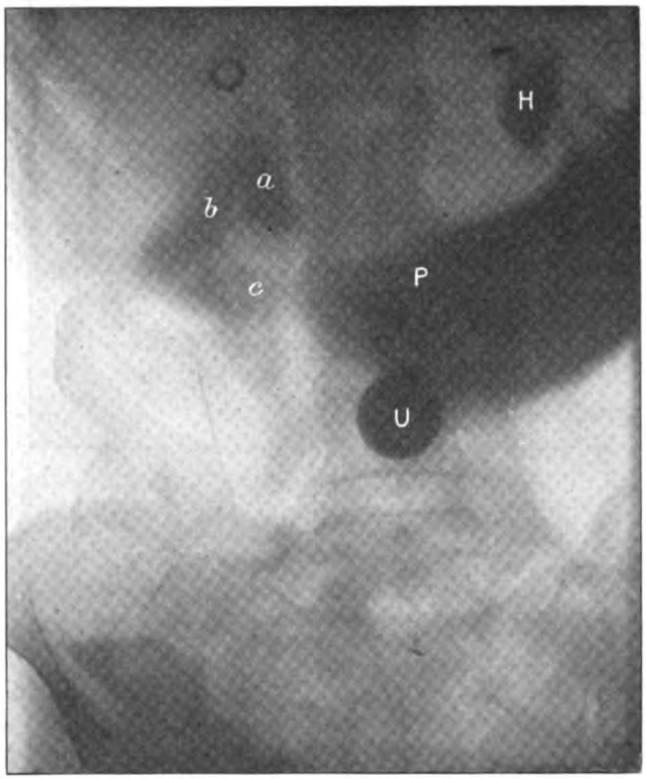


FIG. 7.

Stomach and duodenum in a case of cicatricial hour-glass constriction of the stomach. An isolated pouch (*H*) and the pyloric portion (*P*) still contained bismuth, and the duodenum was seen to be contracting actively against an obstruction due to kinking at the duodeno-jejunal junction (confirmed by operation). *a*, *b*, *c*, first, second and third parts of the duodenum; *U.*, umbilicus, marked by a penny.

handicrafts abnormal thickenings and spurs of bone arise, and give additional strength to parts which are too weak to stand the unnatural strain put upon them.

Kinks.

If these adhesions were equally strong in all parts, the viscera would be held up uniformly, and all would be well. Unfortunately they are often strong in some places, weak or absent in others; con-



FIG. 8.

Dilated duodenum with partial obstruction due to kinking at the duodeno-jejunal junction. *a*, *b*, *c*, first, second and third parts of the duodenum. *U.*, umbilicus, marked by a penny; *Cr.*, crest of ilium (confirmed by operation).

sequently the bowel is held up by them at certain points, and kinks are produced which lead to some degree of obstruction. In severe cases the obstruction is extreme.

The Duodenum.

There are certain points at which these kinks are specially liable to occur when the bowel drops. Following along the digestive canal from the stomach, the first of these is met with in the third part of the duodenum, at the commencement of the jejunum. There is normally at this point a peritoneal band fixing the end of the duodenum; this band is apt to become thickened (fig. 1). In normal cases the jejunum continues in the line of the duodenum; in the subjects of intestinal stasis, however, the jejunum is frequently dragged down vertically, an abrupt kink

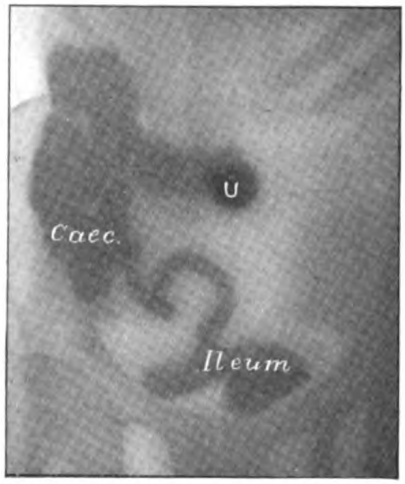


FIG. 9.

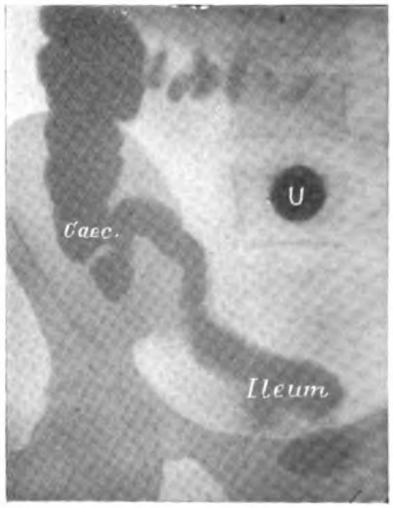


FIG. 10.

Fig. 9.—Horseshoe loops of ileum. Taken ten hours after a bismuth meal. The loops were slightly fixed at two points. (Loose adhesions were found at the subsequent operation.) *U.*, umbilicus.

Fig. 10.—Taken seven hours after a bismuth meal, showing a long terminal coil of the ileum, free from tenderness or obstruction. *U.*, umbilicus.

being produced. I have described this condition fully elsewhere,[1] but further experience has extended our knowledge of this duodenal kinking, and has shown us that it is scarcely ever the primary abnormality. It is secondary to dropping of other viscera, more particularly of the lower end of the ileum. In healthy persons a bismuth meal passes through the upper parts of the small intestine at a rapid rate; it can

[1] *Brit. Med. Journ.*, 1911, i, p. 1172.

be watched running round the coils. At the lower end, however, it encounters the ileo-cæcal valve, and progress here is very much slower; hence the bismuth collects at the lower end of the ileum in a group of coils normally placed above the pelvis. These coils are very liable to drop into the pelvis (*see* figs. 10, 11, 12, 15, 18, 19 and 21). The heavy end-coils of the ileum then drag upon their mesentery, and exert a downward pull which involves the whole of the small intestine, and causes a downward drag upon the commencement of the jejunum, resulting in duodenal obstruction. This is no mere theory or fanciful explanation; I have repeatedly diagnosed the condition beforehand

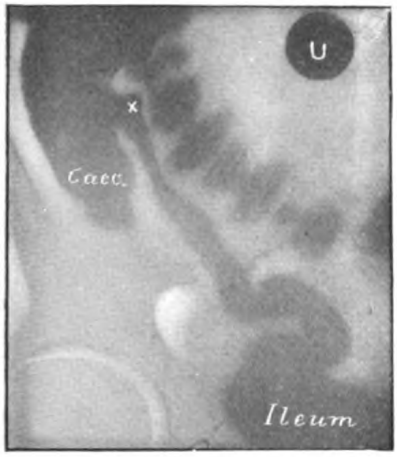


FIG. 11.

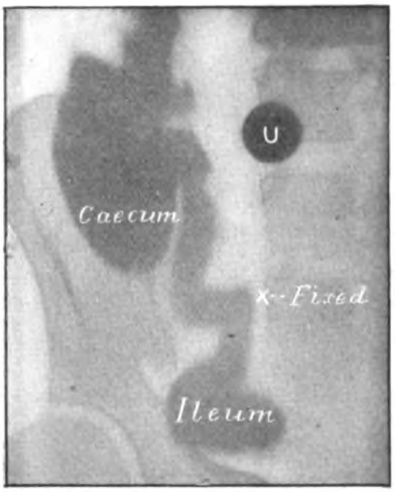


FIG. 12.

Fig. 11.—Ileal kink. Taken twenty-five hours after a bismuth meal, showing a long terminal coil of the ileum rising out of the pelvis; fixed by adhesions at the point *X*, close to the ileo-cæcal entrance. There was tenderness to pressure at this point. The dropped transverse colon is also shown. (Confirmed by operation.)

Fig. 12.—Ileal kink. Taken ten and a half hours after a bismuth meal, showing the long terminal coil of the ileum rising out of the pelvis. Fixed by adhesions at the point *X*. In the same subject as fig. 20 (confirmed by operation). *U.*, umbilicus.

(*see* figs. 2 and 3), and subsequently seen it confirmed at operation. I have seen the distended, congested duodenum held in a grip at its termination, and the jejunum hanging down vertically at its commencement; and in some cases rotated upon its axis, the obstruction being thus increased by torsion of the jejunum.

This duodenal obstruction is not a permanent condition; in slight cases (fig. 2) it is relieved at once when the patient lies down. It is worst when the patient is compelled to remain upright for long periods, and when he is, for any reason, in an exhausted condition. The mechanical consequences of the kinking are very obvious when they are looked for. The first result is distension (figs. 4, 5, 6, and 8). This is greatest in the first part of the duodenum—the part which is unsupported, and has a complete peritoneal covering. The next result is congestion; later, ulceration occurs in the congested mucous membrane. At operations for intestinal stasis, if the duodenum be examined, it is nearly always found to show well-defined evidence of congestion; it is full, even distended, thick, red, and prominent. These appearances

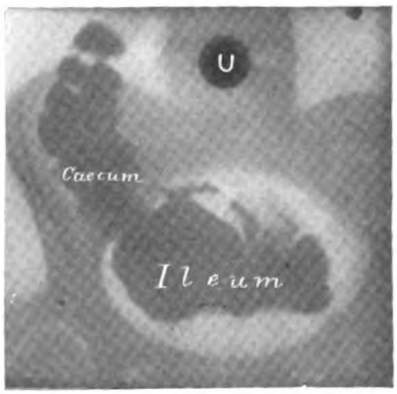


FIG. 13.

Ileal stasis, in a girl, aged 13. Taken ten hours after a bismuth meal. (In the same subject as figs. 23 and 26.) *U.*, umbilicus.

are seen even in patients who have never had symptoms referable to the duodenum (fig. 7). Very often, however, the symptoms have been severe; there has been great pain which has often been most persistent, and there has been vomiting. The pain is often severe long before the congestion of the duodenum has gone on to the formation of an ulcer. Pronounced radiographic evidence of duodenal obstruction due to kinking may be obtained in most cases before operation. It is necessary to bear in mind, however, that the kinking is intermittent; it is most severe when the patient is at his worst, as when he has been at prolonged hard work in the upright posture. The obstruction passes off after the patient has been kept in bed for some days, except

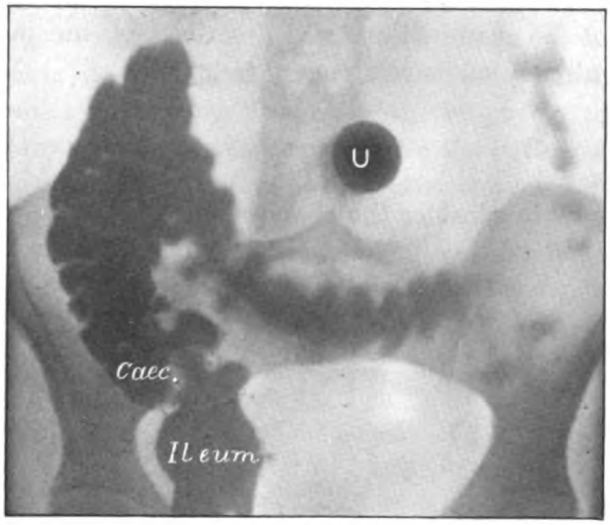


FIG. 14.

Ileal stasis, in a woman, aged 40, taken thirty-seven hours after a bismuth meal. After eighty-five hours there was still some bismuth in the end of the ileum. Very severe symptoms (as described on p. 36) cured by the operation of "short-circuiting." *U.*, umbilicus.

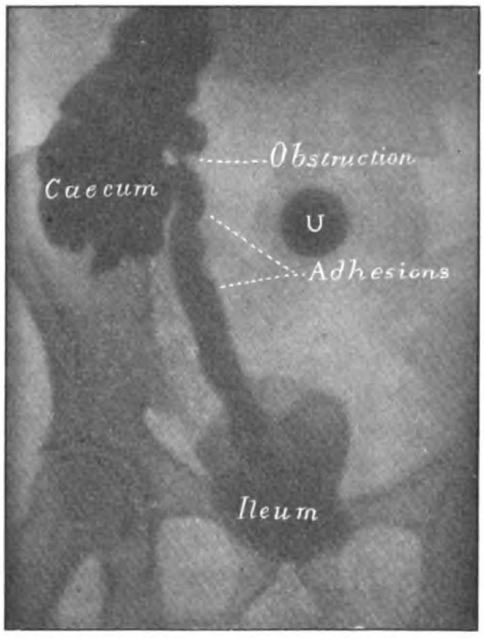


FIG. 15.

Ileal kink, in a man, aged 71. Taken twenty-four hours after a bismuth meal. Showing a long, terminal coil of the ileum, hitched up for the upper three inches and obstructed in the last inch (confirmed by operation, and the patient cured). A duodenal ulcer was found at the operation. *U.*, umbilicus.

in severe cases, when it may persist for a week or more. The method of carrying out the examinations was described by me in the paper to which I have already made reference (loc. cit.).

The Ileum.

We have now to consider the changes that occur in intestinal stasis at the lower end of the ileum. I have already given an account of these[1]; further experience has fully borne out the results there described,

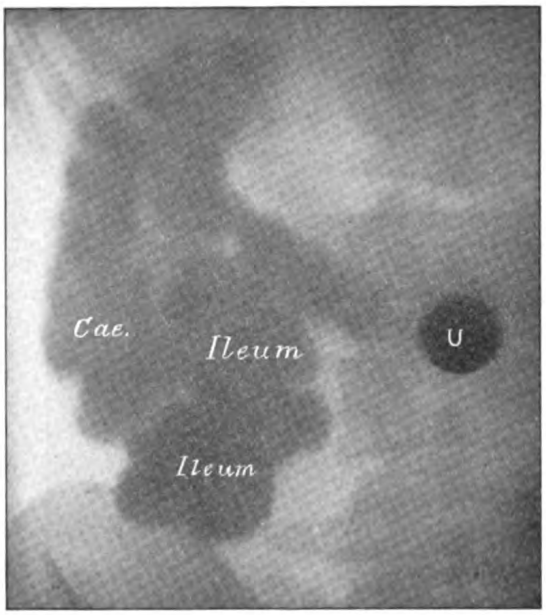


FIG. 16.

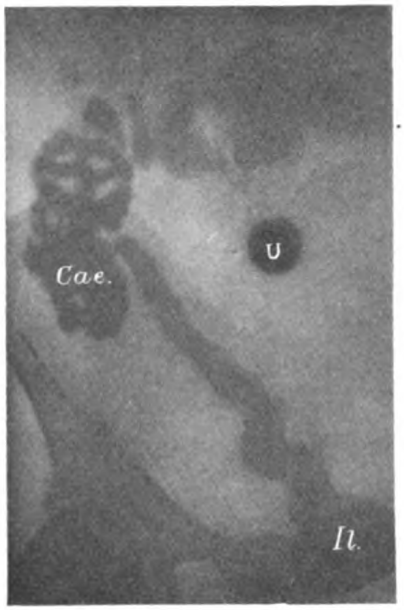
Taken twenty-four hours after a bismuth meal, showing extreme obstruction by adhesions at the end of the ileum, with enormous dilatation of the ileum, in a man, aged 26. Cured by division of the adhesions. *U.*, umbilicus.

and has enabled me to advance greatly in accuracy of diagnosis. I have now examined a large number of patients, and have subsequently been present at the operation and been able to see the actual condition of the parts, so that, as with the duodenum, there is now no room for doubt as to the reality of the anatomical changes first described by

[1] "Lane's Ileal Kink," *Practitioner*, 1911, lxxxvi, pp. 567-76.

Mr. Lane, and now found repeatedly by all surgeons who look for them.[1]

The dropping of the lower coils of the ileum has been described already; it leads to considerable delay in the emptying of the contents of the ileum into the cæcum, especially while the patient's body is erect. A long terminal coil of the ileum rises almost vertically to the ileo-



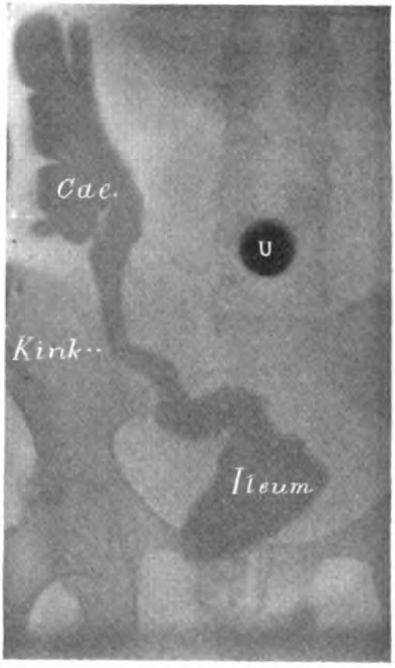


FIG. 17. FIG. 18.

Fig. 17.—Taken five hours after a bismuth meal, in a man, aged 32, showing a long terminal coil of the ileum hitched up near the ileo-cæcal valve, and a corkscrew formation in its pelvic portion, showing that the coil was pulling upon its mesentery. Three hours later this long coil still contained bismuth. *U.*, umbilicus.

Fig. 18.—Ileal kink in a man, aged 36. A sharp kink, causing obstruction, is shown at a point 4 in. from the ileo-cæcal valve. At this point the ileum was firmly fixed to the iliac fossa. Above and below this point the gut was freely movable (confirmed by operation). *U.*, umbilicus.

cæcal entrance (figs. 10, 11, 12, 15, 17, 18, 19 and 21). This long coil is present in many cases, and leads to a varying amount of delay; in the slight cases it is freely movable (figs. 10, 19 and 21), and pressure upon

[1] *Surg., Gyn., and Obstet.*, Chicago, 1911, xii, pp. 34-40; xiii, pp. 485-91.

it with the fingers (while the patient is recumbent) may cause no pain. In other cases there is definite tenderness to pressure at one or more points, the most usual being at the point where it crosses the pelvic brim or at the ileo-cæcal entrance. If there is a fixed point along the terminal coil of the ileum, this point is usually tender also (figs. 11, 12, 15 and 18).

The discovery of a fixed point can be made with great accuracy by means of the fluorescent screen while there is bismuth in the long terminal coil of the ileum; a fixed point here is of the greatest im-

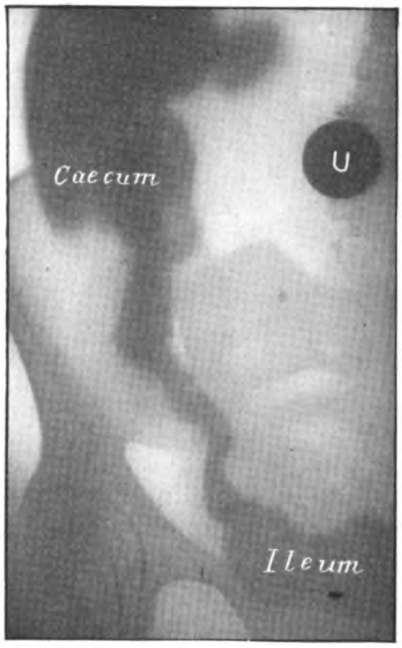


FIG. 19.

Taken ten and a half hours after a bismuth meal in a lady, aged 51. The long terminal coil of the ileum is pulling upon its mesentery, but is not fixed or kinked.

portance, for it indicates the presence, in the particular patient, of a "Lane's Ileal Kink." The fixity is demonstrable in three ways: Firstly, by observing the movements accompanying deep respiration, when the cæcum is seen to move freely up and down, taking with it the end of the ileum as far as this is free, while the fixed point retains its position unchanged. Secondly, by pressure with the fingers (wearing protective gloves), when the part above and below the fixed point may

be moved, but the deepest pressure fails to move the fixed point. Thirdly, by an examination in the upright posture, when the cæcum will fall into (or at any rate toward) the pelvis, leaving the fixed point of the ileum tied up. It is in the upright posture that the effect of an ileal kink is most severe, the two portions of the terminal coil, above and below the kink, hanging down vertically, and producing extreme obstruction in severe cases (fig. 18). Individual cases show great variety in the form of the terminal coil, and the position of the kink (*see* figs. 9 to 21). The ileum may be held up at more than one point along the terminal coil (fig. 15); again, it may be grasped so securely

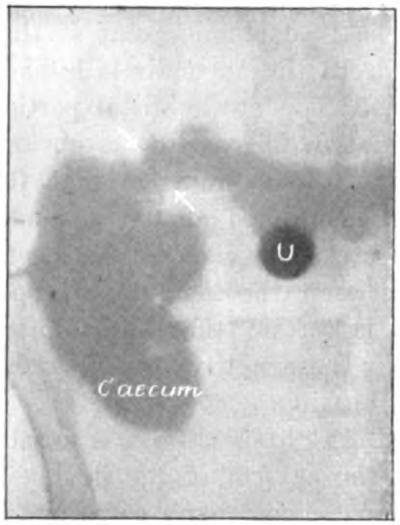


FIG. 20.



FIG. 21.

Fig. 20.—Taken forty-seven hours after a bismuth meal in the same subject as fig. 12 (aged 57). Fibrous bands run from the under-surface of the liver down to the transverse colon, obstructing it just beyond the hepatic flexure (confirmed by operation). *U.*, umbilicus.

Fig. 21.—Taken five hours after a bismuth meal in a man, aged 49. The terminal coil of the ileum is unobstructed. The appendix is well shown by the bismuth it contains. No tenderness of the ileum or the appendix.

and tightly as to produce permanent obstruction, even in the recumbent posture (fig. 15). The obstruction may be so extreme as to lead to great dilatation of the end of the ileum, which may actually exceed the cæcum and ascending colon in diameter (fig. 16).

The corkscrew formation sometimes shown by the lower portion of

the long terminal coil of the ileum is evidence of the manner in which this coil is pulling upon its mesentery (fig. 17).

As a result of the stasis in the ileum, bacteria, normally confined to the large intestine, ascend the ileum, giving rise to decomposition of its contents. This is evidenced in the upper parts of the small bowel as pancreatitis and cholecystitis, and the unpleasant breath of these patients bears further testimony to the bacterial invasion of the upper regions of the intestinal tract.

The Appendix.

The appendix is often involved in the newly formed bands that arise in the ileo-cæcal region. It may be held at one point so that when the patient stands and the cæcum drops the appendix is hitched up at the fixed point and obstructed. Distension of the distal portion of the appendix then occurs, and inflammation follows. It is obvious to anyone who approaches the subject without prejudice that the "adhesions" frequently found in the neighbourhood of the appendix cannot be inflammatory in origin; they are often disposed in sheets, and the fingers can then be inserted between the mesentery proper and these adventitious mesenteries. It is certain that the primary disorder is the kinking of the appendix by these newly formed bands; the appendicitis is a consequence of the kinking.

As already stated, the tender spot in the right iliac fossa is usually localized accurately to some point along the end-coil of the ileum; it has nothing to do with the appendix. In a good many cases one is able to go further, for a little bismuth is found to have entered the appendix. In such cases direct pressure can be brought to bear (by means of the fingers) upon the appendix, and the absence of tenderness of this organ can thus be confirmed (figs. 21 and 23). Conversely, the discovery of a tender appendix is important, while fixity of the appendix, disclosed in this manner, may also be of consequence.

Ileal Stasis.

I have stated already that considerable delay in the passage of the intestinal contents from the ileum to the cæcum is produced by the falling into the pelvis of the lower coils of the ileum, leaving a long terminal coil rising to the cæcum. The delay thus brought about is independent of kinking, and may be great, especially in the upright

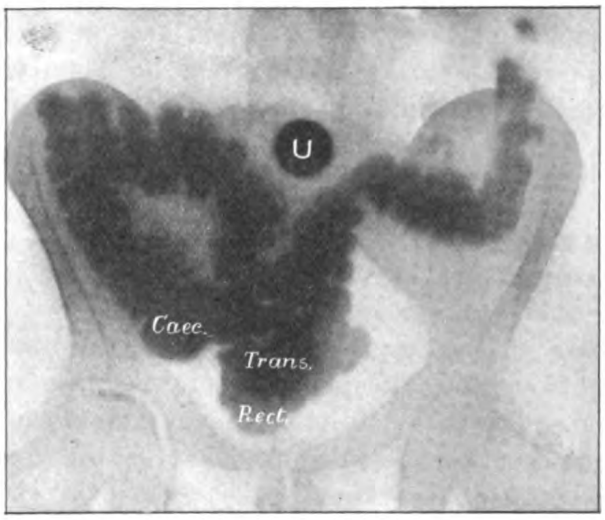


FIG. 22.

Taken one hundred and two hours after a bismuth meal, in a man, aged 36, showing extreme stasis in the transverse colon, which is greatly elongated and dropped. The hepatic flexure does not rise as high as the iliac crest. *Trans.*, transverse colon ; *Rect.*, rectum ; *U.*, umbilicus.

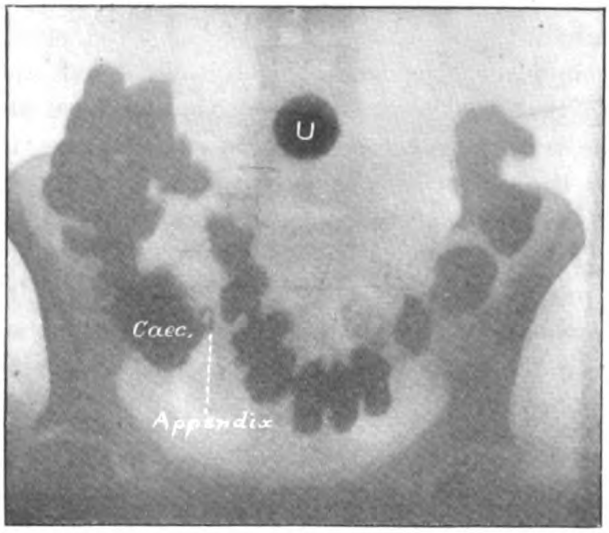


FIG. 23.

Taken thirty-four hours after a bismuth meal in a girl, aged 13 (the same subject as figs. 13 and 26), showing stasis with marked dropping of the transverse colon. The appendix is also shown.

posture. The delay is greatest in feeble subjects, and in them, even in the supine posture, there is often the greatest difficulty in the way of the passage of the contents from the ileum to the cæcum; they are unable to surmount the obstacle formed by the promontory of the sacrum and the pelvic brim (figs. 13 and 14).

The Hepatic Flexure.

The next part of the intestine at which obstruction by bands is liable to occur is in the region of the hepatic flexure and just beyond, in the first part of the transverse colon, where bands sometimes arise, running from the under-surface of the liver down to the transverse colon. This is much less common than in the cæcal region, but when bands occur at the commencement of the transverse colon they may produce great obstruction. The contents of this part of the bowel are solid; hence the effect of a slight diminution of the calibre of the gut is far greater than in the case of the small intestine (fig. 20). If torsion of the bowel behind the fixed part occur, the condition known as "volvulus" will be brought about.

The Splenic Flexure.

The splenic flexure is normally fixed below the diaphragm by the costo-colic ligament; this is always a strong band, and it keeps the splenic flexure in place even when the ascending and transverse colon are dropped so that the hepatic flexure does not rise above the iliac crest, while the transverse colon hangs in a great loop, the middle of which occupies the pelvis in company with the dropped cæcum, the dropped great curvature of the stomach, the dropped lower coils of the ileum, the sigmoid flexure and the rectum, all pressing upon the pelvic organs proper (fig. 22). In such a case there is a very long rise from the lowest part of the transverse colon to the splenic flexure and, especially in the upright posture, fæces may be retained for days in the transverse colon.

The Sigmoid Loop.

The sigmoid loop is very frequently the seat of abnormal bands or adhesions in the peritoneum; they pass down to the outer surface of the meso-sigmoid. In some cases they bind down the whole of the sigmoid, which then takes a short, straight course from the iliac crest obliquely downward and inward to the rectum. This state

of affairs is favourable to the patient unless the tube be actually narrowed by pressure of the peritoneal bands upon it; narrowing in this region causes obstruction, and diverticula may be produced, communicating with the narrowed central lumen of the sigmoid (fig. 24). In other cases only the ends of the sigmoid loop are fixed by bands close together, the loop itself being free, and projecting into the false pelvis. Thus two kinks are produced, the lower of which is a very sharp one and may cause great obstruction. This obstruction leads to lengthening of the loop and renders it very liable to the occurrence of volvulus. The most severe case of volvulus I ever saw was found, at operation, to be produced in this way.

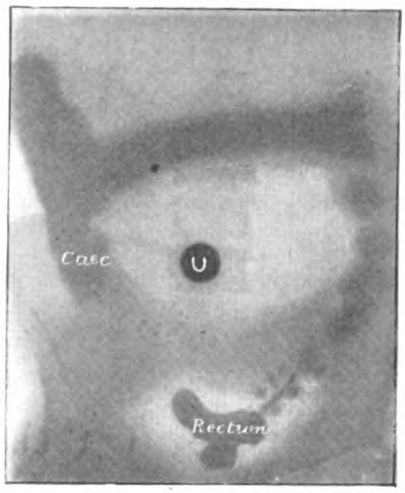


FIG. 24.

Taken twenty hours after a bismuth meal in a man, aged 54, showing the short, straight sigmoid tied down, and its lumen narrowed by adhesions, and numerous diverticula communicating with the central lumen (confirmed by operation, and the patient cured). *U.*, umbilicus.

In other cases the upper part only of the sigmoid loop is fixed down; the lower part then runs without separation into the rectum, forming a long mobile tube in the pelvis (fig. 25).

The Rectum.

An elongated S-looped rectum is not uncommon in the subjects of intestinal stasis, more especially in children (fig. 26), and it is clear that in efforts to defæcate the pressure brought to bear upon the

abdomen by the abdominal muscles and diaphragm is in the long axis of the body, and is hence at right angles to the length of the loops of the rectum. The effect of such efforts is to press the loops closer together, and increase the lateral elongation. Thus the condition becomes steadily worse; an enormously capacious rectum is produced, which is continuously packed with fæces. There may be a full-sized motion daily, but only a small portion of the rectum at the lower end is evacuated thereby, leaving by far the greater part of the fæces to stagnate and to poison the system (*see* fig. 26). The results of this poisoning are very serious, especially in children, whose vitality and resistance to micro-organisms are thus lowered so that they are rendered very liable to tuberculous disease. It is a suggestive fact that children suffering from tuberculous disease of the bones or joints are always found to have an elongated rectum. This is yet another fact which I have often been able to ascertain by the X-rays in the first case, and subsequently seen confirmed at operation.

Treatment.

We have seen that the anatomical changes found in intestinal stasis affect every part of the intestinal tract, and that in most cases several parts are affected, though to a varying extent. Obviously the treatment will depend on the degree of affection of the various parts in the individual case.

To take an example: A patient may have symptoms pointing to the presence of a duodenal ulcer. The X-ray examination may provide indisputable evidence of the presence of duodenal kinking (and I have found such evidence very often in patients with symptoms pointing to duodenal ulcer (e.g., figs. 3, 4, 5, 6 and 8). So far the clinical diagnosis has been confirmed, for duodenal kinking, as already explained, is very likely to lead to a duodenal ulcer. We must not rest here, however; we must follow the bismuth meal further, and we shall find in every case some degree of kinking or static obstruction at the lower end of the ileum. We may find very definite obstruction in this region; it becomes clear at once, therefore, that the mere treatment (whether medical or surgical) of the duodenal affection would not be a rational procedure. In following the bismuth meal still further, through the large intestine, we are likely to find further evidence of stasis, possibly of very severe stasis. Hence it is clear that we are never wise in applying treatment for the cure of duodenal ulcer until

we have investigated the entire alimentary canal. As a matter of fact, the duodenum is the only portion of the alimentary canal that is never affected singly.

Gastro-jejunostomy.

Hence the operation of gastro-jejunostomy for the cure of duodenal ulcer is not rational; this operation should be reserved for cases of

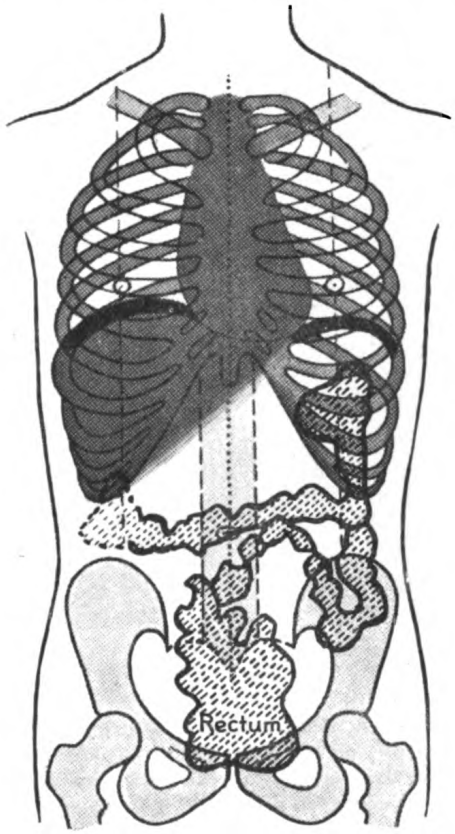


FIG. 25.

Taken eighty hours after a bismuth meal in a woman, aged 46, showing marked stasis in the large bowel, and a greatly elongated sigmoid and rectum. There was also stasis, exceeding forty hours in duration, in the end of the ileum, much as in fig. 14. A remarkable cure was achieved by the operation of "short-circuiting."

actual organic stenosis of the pylorus or duodenum. I have now seen a large number of patients at various periods after having gastro-jejunostomy performed for the cure of duodenal ulcer. No doubt

many patients have obtained great relief from the operation, but it must be remembered that duodenal ulcer is a very readily curable condition, three weeks' rest in bed sufficing, as a rule, to effect a cure. Thus the recumbency necessitated by the operation would be expected to permit of the cure of the duodenal ulcer even though no new opening had been made from the stomach into the jejunum. It is true that duodenal ulceration is very apt to recur when the patient returns to his ordinary occupations, and this is frequently the reason for which patients consent to an operation. In the cases, few in number, in which the duodenum is the most serious point of obstruction, the result of gastro-jejunostomy may be permanently satisfactory, although the patients are for ever deprived of a great part of their gastric digestion. The fact should be

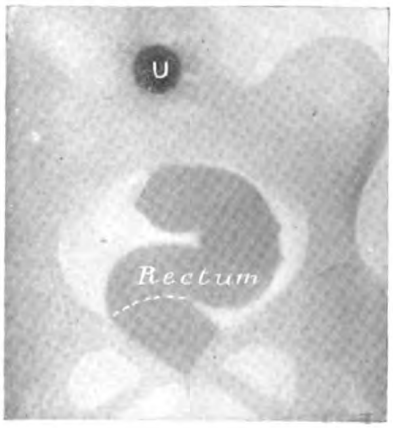


FIG. 26.

Elongated and dilated S-looped rectum in a girl, aged 13 (same subject as figs. 13 and 23), taken fifty-eight hours after a bismuth meal. There was an evacuation every day or two, but only a small portion of the rectal contents was eliminated at a time. The portion marked off by a dotted line represents a good-sized motion.

borne in mind that the operation of gastro-jejunostomy would be expected to do good by holding up the jejunum in such a position as to prevent the occurrence of kinking at the duodeno-jejunal junction for ever after. This good effect is procured quite independently of the making of a new opening between the stomach and the jejunum; it is a benefit bestowed by accident.

In a great many cases, however, the relief obtained by gastro-jejunostomy is merely temporary, not much greater, in fact, than would

be obtained by simple rest in bed on a milk diet. I have now examined a considerable number of patients in whom the operation of gastro-jejunostomy had been skilfully performed by eminent surgeons, but intestinal trouble still existed, or had returned after apparent cure. The symptoms complained of are varied; all have pain, many have vomiting, and the vomit usually contains bile and is very distressing; most exhibit all the signs and symptoms of intestinal stasis. An X-ray examination in such cases shows various conditions. In most the bismuth meal finds its way out through the new opening at a rapid rate,

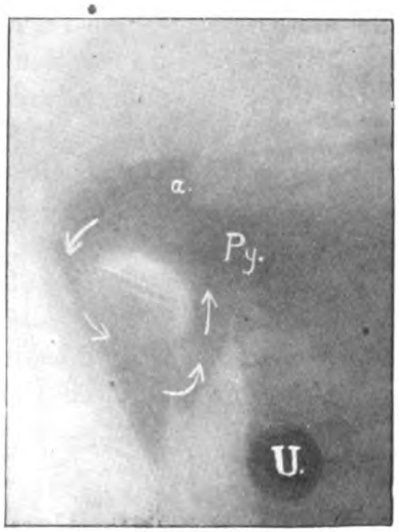


FIG. 27.

Skiagram of a "vicious circle" following gastro-jejunostomy, showing the course taken by a bismuth meal from the pylorus through the duodenum, and back again into the stomach by way of the gastro-jejunostomy opening. The operation had been performed eight months previously for the cure of a duodenal ulcer, but the patient, after a brief period of relief, had so much pain and vomiting of bile that the surgeon (after the X-ray diagnosis) reopened the abdomen and closed up the gastro-jejunostomy opening (*see* p. 33). *a*, first part of the duodenum; *U.*, umbilicus; *Py.*, pylorus.

so that nearly the whole of it has left the stomach within half an hour, provided the patient remain upright.

"*Vicious Circle.*"

In a certain number of cases a "vicious circle" is produced, the bismuth leaving the stomach by the pylorus and returning once more by the new opening, carrying with it a large quantity of bile. This is

vomited with great pain and distress to the patient (fig. 27). With a view to avoiding the possible creation of a "vicious circle" some surgeons now actually close the pyloric aperture by a process of invagination at the time of performing the gastro-jejunostomy. In other words, they first create a pyloric stenosis and then proceed to cure it by a gastro-jejunostomy. Observing these cases subsequently by the fluorescent screen after a bismuth meal I have often found that while some of the meal passes out by the new opening, a portion is carried on by the powerful peristaltic contractions of the stomach beyond the new opening, and is seen to beat against the pylorus, which has become bulbous in form from the pressure of the gastric contents. The reason these patients are sent for an X-ray examination is pain; and there is no possible doubt but that the pain is due to pressure upon the closed pylorus.

The conclusion—the only rational one that can be formed—is that the operation of gastro-jejunostomy should only be performed for the treatment of actual gastric or duodenal stenosis, but not for duodenal ulcer, unless in the rare cases in which a series of X-ray examinations, properly carried out, has demonstrated the absence of any marked kinking or stasis in the lower parts of the intestinal tract. Now these are the very cases in which duodenal ulcer yields readily to simple medical means with rest in bed, and ordinary precautions suffice to prevent a relapse.

Radical Cure of Intestinal Stasis.

In the more usual case in which the presence of an ileal kink can be demonstrated, the operation of gastro-jejunostomy can do no lasting good; and the same is true of the cases in which there is well-marked stasis in some or all parts of the large intestine. In these cases surgical treatment, to be rational, must be directed to the cure of the intestinal stasis. This is accomplished by the operation of "short-circuiting" which consists in dividing the ileum near its lower end, and making an anastomosis between this divided end and the upper part of the rectum. By this means the whole of the stasis due to the ileal kink and the large bowel is abolished. The operation, if skilfully performed, is no more risky than other abdominal operations (such as posterior gastro-jejunostomy). The enforced recumbency is sufficiently prolonged to enable the duodenal ulceration to heal, and when the patient resumes his ordinary life there is no longer any drag upon the jejunum leading to kinking at the duodeno-jejunal junction. I have

seen patients in whom the symptoms of duodenal trouble were very well marked make a complete and lasting recovery as the result of the operation of making an anastomosis between the ileum and the rectum. The duodenum was not touched at the operation, but it was carefully examined, and found to be distended and congested. In one case there was a well-marked duodenal ulcer.

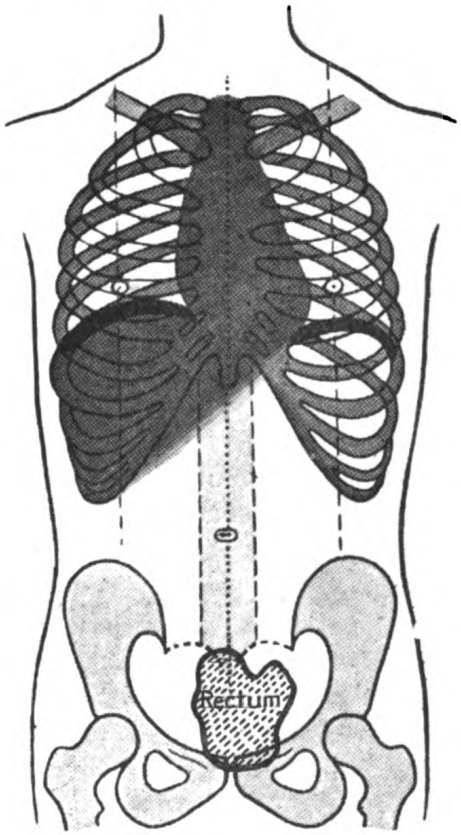


FIG. 28.

Taken nine hours after a bismuth meal in a case of severe intestinal stasis, cured by the operation of "short-circuiting." The whole of the bismuth is in the rectum, proving that all intestinal stasis has been abolished.

The operation of making an anastomosis between the ileum (near its lower end) and the rectum is the best procedure in all severe cases of intestinal stasis which fail to yield to less radical treatment. The operation has been carefully described,[1] and I need only draw attention

[1] Harold Chapple, *Brit. Med. Journ.*, 1911, i, pp. 915-22.

to a few points: Where definite kinks are found, due to fibrous bands, the surgeon is often tempted to content himself with their mere division (this was done in figs. 15 and 16). Experience shows, however, that this is rarely sufficient; the adhesions are apt to re-form, and the symptoms return. In making the anastomosis the end of the divided ileum is sutured into the side of the rectum. It is important to make the anastomosis as near the end of the rectum as practicable; at any rate below the last of the sigmoid kinks described above. If the new opening were made above the sigmoid kink there would be obstruction, and a cure would not be effected. Frequently the temptation is great to clamp the prominent loop of the sigmoid which presents itself in the left iliac fossa (fig. 25), and to suture the ileum into it; this should not be done on any account. With the anastomosis properly made into the rectum, the sigmoid kink, when present, is of great use in preventing the possibility of the ileal contents flowing back into the descending colon, and giving rise to flatulent distension.

After-results.

The results of the operation on the intestinal stasis are very striking. All abdominal pain is abolished; the constipation is relieved; the headache and other aches leave off; a good appetite replaces the nausea; from a state of depression the patients become bright and cheerful; the staining of the skin clears up; the breasts return to their healthy state. A woman, aged 46, was recently admitted to Guy's Hospital to have both breasts removed for supposed cancer; there was extreme cystic disease of the sort which commonly becomes cancerous. The X-ray examinations revealed the existence of severe intestinal stasis (figs. 14 and 25). Within a week after the intestinal operation the breasts began to improve, and by the end of three weeks they were practically normal; the woman was greatly improved in all other respects; she had begun to gain in weight. This gain in weight occurs in all cases; the circulation improves, so that the extremities are found to be warm, and the patients begin to enjoy life again. During the X-ray examinations that precede the operation patients frequently tell me they have lost all desire to live, so extreme is the depression produced by intestinal stasis. A careful study of a number of these patients before and after operation has been published by Mr. Harold Chapple (loc. cit.). After recovery from the operation a bismuth meal passes through, and is all collected in the rectum by the

end of seven or eight hours, proving that all intestinal stasis has been abolished (fig. 26).

I trust I have succeeded in showing that intestinal stasis is a most important disease, in the diagnosis of which radiography is of the greatest assistance by affording evidence, which is positive and beyond dispute, of the existence and the position of the obstructions in the intestines which form the subject of this paper. The condition can now be accurately diagnosed beforehand by radiography, and the accuracy of the diagnosis afterwards tested by operation. Obviously this means of diagnosis is of the greatest value.

DISCUSSION.

Dr. HERTZ said he had been much interested in Dr. Jordan's paper and the excellent pictures he had shown. But the chief impression he had gained from hearing it and seeing the pictures was that a great deal more work was yet required on the subject, because he had been working at the subject for a long time, and his conclusions were, in almost every detail, opposed to those of Dr. Jordan. Dr. Jordan said that constipation was merely one, and apparently not an essential, symptom of chronic intestinal stasis. But he (the speaker) did not know what chronic intestinal stasis was unless there was constipation, for surely intestinal stasis meant constipation, in which case there was no need of a new term to describe the same condition; one might just as well say that chronic intestinal stasis was a symptom of constipation. Many of the other symptoms of intestinal stasis which Dr. Jordan had mentioned Dr. Hertz regarded not as symptoms but as causes of constipation. For instance, a poor appetite was a common cause of constipation; people who ate too little naturally formed too little fæces, and without the stimulus of bulky fæces there was a sluggish action of the intestine. Chronic abdominal pain, whatever its cause, tended to produce a reflex inhibition of the intestinal movements, and the result was constipation. He did not say that constipation might not produce pain, but it was comparatively rare for it to do so. Dr. Jordan had also spoken at length about the influence of the erect posture. But practically everybody lived nearly all their life in the erect posture; therefore, if that posture was a factor in the production of chronic intestinal stasis, why had not everybody got chronic intestinal stasis? As a matter of fact, constipation was almost always a symptom when an individual had for any reason to remain in the horizontal position for a prolonged period. With regard to the position of the colon, many of the severest cases of constipation which he had seen were in people whose colon was in the normal position, and many of the severest cases of ptosis were in people

who had no intestinal symptoms or stasis. Some time ago Dr. C. J. Morton examined some twenty normal students at Guy's with him, and it was found that a considerable number of them had their colon quite as low as those illustrated in the pictures shown by Dr. Jordan, yet they were absolutely healthy men, and none of them had ever had constipation or symptoms of "chronic intestinal stasis." He thought that, as a rule, when severe ptosis of the colon was associated with constipation both were due to the same thing—i.e., weak abdominal muscles, which resulted in ptosis, and also made defæcation difficult. He agreed, however, with Dr. Jordan that there might be ptosis of individual organs without weak abdominal muscles. With regard to the "duodenal kink," ever since he had first heard of it he had looked out for it, but he had not yet seen it. He hoped, however, that Dr. Jordan would have an opportunity of showing it to him: he was open to conviction on the matter. But what he felt he would not readily be convinced about was that the duodenal kink could have the remotest connexion with duodenal ulcer. He had examined some twenty-five cases of duodenal ulcer by means of the X-rays, and the characteristic feature in every case was the absence of ptosis. If one saw a stomach which was high, in which the greater curvature was an inch or more above the umbilicus in the vertical position, very likely the patient had a duodenal ulcer. One never found, in uncomplicated ulcer, the smallest degree of kinking of the duodenum; the rapid way in which the bismuth passed through the duodenum into the jejunum in these cases being most striking. In cases of gall-stones one sometimes found a similar but less marked condition of hypertonus. In these cases the shadow of the duodenum was exceptionally obvious because of the rapid evacuation of the stomach; perhaps this unusually dark duodenal shadow had led to the idea of obstruction, as the shadow of the distal part of the duodenum as well as the jejunum was much less obvious than the proximal part of the duodenum, owing to the dilution of the bismuth-containing chyme with the mixture of succus entericus, bile and pancreatic juice. He was much struck by the fact that when Dr. Jordan wished to demonstrate the duodenal kink, which was said to be the result of ptosis, he placed the patient on a couch, although in that position the obstruction was supposed to disappear. In regard to the ileal kink, he had seen it in a number of patients, but the only kind of case in which it had produced symptoms and in which it was a serious matter, was that in which the patient had had acute or chronic appendicitis. He thought most people would agree with the commonly accepted view that adhesions round an appendix that had been inflamed were due to appendicitis, and that the presence of adhesions round an acutely inflamed appendix was evidence of a previous attack and not, as Dr. Jordan had suggested, that the appendicitis was due to adhesions of a non-inflammatory region. The shadow of the end of the ileum was always more obvious than the rest of the small intestine, because of the ileo-cæcal sphincter. In some cases of constipation inhibition of the intestinal movements was associated with insufficient relaxation of the sphincter. That was one cause of the

slight delay sometimes seen in the end of the ileum, but in simple constipation the delay was never well marked. In Dr. Jordan's skiagrams it was remarkable how the shadow was as wide and as dark in the part immediately distal to the so-called kink as it was in the part proximal to it; this showed that the kink could not have led to much stasis. Moreover, the contents of the ileum were always fluid, so that any slight kinking there could be only of very small importance. With regard to operation in these conditions, he thought everyone would admit that a gastro-enterostomy performed for duodenal ulcer was one of the most successful operations conceivable. Mr. Moynihan, who had perhaps done that operation more often than anyone else, found that the recurrence of symptoms after operations was extraordinarily rare, and that, moreover, the constipation previously present disappeared. If he were to look for ileal kinks and divide them when present Dr. Hertz thought that the mortality would be distinctly increased without improving the results. He had made a post-mortem examination on a case of duodenal ulcer, in which death occurred from peritonitis, the result of dividing adhesions in the right iliac fossa in addition to doing the gastro-enterostomy required for the ulcer. The operation for gall-stones also was a most successful one if all the stones were removed and the gall-bladder drained, and it was not necessary to divide ileal kinks even if they were found to be present. At the end of a series Dr. Jordan showed some pictures in which the bismuth had collected in the rectum. That was the condition which he (Dr. Hertz) described a long time ago as dyschezia. All the delay which caused the constipation was in the rectum, and the primary condition was difficulty in defæcation; there was no intestinal stasis at all. If one increased the power of the abdominal muscles and kept the rectum empty by means of enemata, these patients got well, as the dilatation and lengthening of the rectum were simply due to the inability to empty it. He did not wish to depreciate Dr. Jordan's valuable work on this subject, and doubtless was wrong in some of his points, but he was convinced that Dr. Jordan would require to reconsider many of his statements.

Dr. A. E. BARCLAY said that he had listened with great interest to Dr. Jordan's paper and wished to thank him for the many suggestive points he had demonstrated. He had come to the meeting prepared to be convinced, but after hearing Dr. Jordan's explanations of his radiographs he had come to the conclusion that differences in technique were accountable for the discrepancy which occurred between his own results and those of the author. In his own observations, which extended over about 1,800 examinations of between 700 and 800 cases, he had only once met with obstruction at the duodeno-jejunal flexure and was driven to the conclusion that Dr. Jordan's methods tended to produce the results he described and that they would not be observed unless special methods were used. In this one case the crowding of the food into the distended duodenum was exactly as Dr. Jordan had described, but as the patient did not submit to operation the cause of it was

not ascertained. He had come to the conclusion that it was not normal to see food passing through the duodenum by means of the fluorescent screen, and when he did observe a definite bolus either lodged in or passing through the duodenum he found that an operation in such cases showed the presence of some abnormality in the region of the duodenum, such as duodenal ulceration, gall-bladder trouble, adhesions, carcinoma, or an inflamed appendix. Any of these conditions seemed to bring about an abnormal intermittent relaxation of pyloric spasm, and he could well imagine that if special methods were used for getting the shadow of the stomach out of the way, an appearance of obstruction might be produced, although by the straightforward bismuth method no suggestion of obstruction was observed. Some of Dr. Jordan's cases of obstruction at the lower end of the intestine were convincing, but in others he could not reconcile the appearances with the presence of actual obstruction, as the shadow of the bismuth was just as dense on the distal as on the proximal side of the alleged obstruction. The splenic flexure always looked as if it must be kinked unless the patient was turned sideways. In one case—a case of spasmodic œsophageal obstruction—he had observed "Holzknecht's phenomenon" of movement of the contents of the large intestine *en masse*. The bismuth shadow outlined the colon from the cæcum to a point about 6 in. from the splenic flexure, and suddenly, as quickly as the eye could follow it, the shadow moved on round the splenic flexure and about 6 in. down the descending colon. On examining the flexure carefully it looked as if there must be a kink, but the fact that the solid fæces rushed round in this rapid manner was definite proof that there was no obstruction, although the appearance of kinking was seen upon the fluorescent screen. He quite agreed with Dr. Hertz as to the condition depicted in a radiograph showing a distended sigmoid. It seemed to him probable that the case was simply one of inefficient defæcation that had led to dilatation. He again desired to thank Dr. Jordan for his paper, and although he had expressed disagreement with certain points, he realized the importance of the work and hoped that further experience would disprove his own scepticism in the matter.

Dr. E. S. WORRALL said there was an anatomical point which struck him and which he hoped Dr. Jordan would explain. In the pictures he had shown, the part which was pointed out as the commencement of the jejunum was always well to the right of the spine. Was not that a departure from the normal?

Mr. HAROLD CHAPPLE said he did not propose to deal with the matter from the radiographic point of view—that he would leave to Dr. Jordan. But he would speak about the actual practice, and wished to point out that the operative findings in the abdomen were just as definite as Dr. Jordan's skiagrams had shown. He had had the privilege of being Mr. Lane's house surgeon, and had seen at least 100 of these cases; he could assure them that the condition under discussion was a very definite one and capable of the fullest demonstration. It could be diagnosed before the abdomen was opened.

If one knew the condition, one could recognize such cases at once. Dr. Jordan could show it by X-rays and bismuth, operation confirmed the diagnosis, and the condition was relieved by the operative procedures adopted. He had followed the stories of many of these cases, and in a recent number of the *British Medical Journal*[1] he had published the results of fifty of them. Those who knew the difficulty of tracing hospital patients would appreciate what that meant. As a result of this inquiry he had ample proof that the symptoms of this condition described as intestinal stasis were most definitely and certainly relieved by the treatment adopted. The adhesions to which this condition was due were found, it was stated, in practically everyone; and he believed Mr. Mollison and Dr. Cameron found at Guy's that in 100 consecutive autopsies they were present in practically all. But the point for consideration was the effect of those adhesions on the intestinal tract. For instance, doubt had been raised by some of the previous speakers as to genuineness of the presence of the ileal kink. They had seen in Dr. Jordan's series excellent skiagrams of the condition photographed after a bismuth meal, and there seemed to be no real difficulty in finding it. He could assure them that the condition could now be diagnosed from the outside; one could divide the adhesion causing the kink without interfering with the mesentery of the small intestine, and the symptoms were thus relieved. This adhesion was not part of the mesentery; one could place one's finger under it and divide it without in any way interfering with the blood supply of the bowel. A subsequent skiagram would show bismuth going through at the normal rate. It was difficult to know what further proof was necessary. The presence of a duodenal kink was the most recent development of this work, and its relation to the ileal kink was established. He had seen a case in which gastro-enterostomy was performed in dealing with it, with no marked benefit to the patient at all; but the mere division of the ileal kink freed him from his symptoms. In a paper written by C. H. Mayo, of Rochester, U.S.A., would be found a very beautiful picture of a Lane's ileal kink, which he dealt with by dividing it; but if one did only that, one was liable to get secondary obstruction. It was very difficult just to divide the adhesion and sew up the peritoneum without the risk of causing secondary obstruction. This disability had happened to two or three people, and the condition had to be dealt with afterwards. So whatever theories might be put forward on this subject, the obstinate fact remained that in these people a definite condition had been described which could be diagnosed beforehand, could be confirmed by X-rays as they had seen that evening, and the symptoms could be relieved by operative procedures. Any theory put forward in explanation that did not reckon with these facts was obviously out of the question. The condition was accompanied by the gross naked-eye changes to which Dr. Jordan had referred, such as marked discoloration of the skin, marked cystic changes in the breast, and so on. There was now a female patient in Guy's Hospital who had been sent in to have both breasts removed for chronic mastitis and cystic degeneration, probably going on to malignant

[1] *Brit. Med. Journ.*, 1911, i, pp. 915-22.

disease. She was treated by short-circuiting four weeks ago, and the change for the better in the breasts was already extraordinary, the large lumps having almost disappeared. Another case of interest was that of a woman who had a very large thyroid in which there was a marked cystic change, so large that the anæsthetist raised the question of the advisability of doing anything. The woman had marked symptoms due to intestinal stasis. In about four weeks after a short-circuiting operation the thyroid was reduced to about half its former size. These changes were not myths; they were clearly evident to the eye, and any theory that dealt with this subject had to explain them. He had seen staining of the skin so marked that it was almost black; the darkness had been increasing during the last few years, and affected various parts of the patient's skin. The patient was submitted to colectomy and now was a pink-cheeked woman, quite healthy in every way. She was sent in by her doctor as a case of acute gastric ulcer, with hæmatemesis, which had been going on with frequent vomiting and marked constipation. The stomach was examined at the operation, but there was nothing in the form of gastric ulcer. It was all very well to say that such a case was neurotic; this was an elastic term with limits becoming more defined as knowledge advanced; and when the patient complained of abdominal pain and there seemed no obvious reason for it, he would suggest examination by X-rays after a bismuth meal to see if there was any obstruction of the nature described that evening. It was always fair to look on both sides of a question, and the opponents of the operative treatment naturally urged the mortality which resulted. This had been brought down to the figure of other abdominal operations which were performed with skill; ileo-colostomy offered no more difficulties than did gastro-enterostomy. The other point which was raised was as to what happened to the patient afterwards. He had seen these patients up to as long as nine years afterwards, and in every case of which he knew there had been a rapid improvement in symptoms; the patient had gained in weight, and the skin changes and the breast condition, as well as the abdominal pain, had improved. He was quite aware there were a few cases in which the abdominal pain was still markedly present, and this was of interest because the original treatment was one of lateral junction. The ileum was divided and put laterally into the rectum; pouching of the end of the ileum resulted which caused marked abdominal pain. The treatment was to lace it up again. End-to-side anastomosis was now performed, and the most that could now be fairly urged against the operative procedure was that sometimes if there was not marked obstruction of the sigmoid there would be regurgitation of fæces and flatulence in the colon. This was a matter which would be dealt with, and the operation would be perfected. So that whether they believed the bands described to be the cause of the condition or not, he hoped they would look for them in the future. He had seen them diagnosed beforehand, and confirmed at operation, by bismuth meals and X-rays. He had seen them divided and the condition relieved. What further proof could any reasonable person require?

Dr. IRONSIDE BRUCE said he had been much interested in Dr. Jordan's paper, and he had been glad to hear the discussion on it. He had seen appearances described by Dr. Jordan, in observing bismuth food passing through the intestine. He had also seen the so-called kinks, but he thought the subject should be thoroughly discussed as to the meaning of such observations before arriving at any conclusion. With regard to Dr. Jordan's remarks concerning the delay in the large intestine, when he (the speaker) began to make observations of the kind a patient was brought to him in whom a delay took place in the ascending colon for seventy-two hours. He stated that this case was certainly one of obstruction in the region of hepatic flexure. The abdomen was opened, but nothing abnormal was found. This case opened his eyes to the situation, and he now believed it was difficult for any person to make positive statements about such appearances, at any rate so far as the colon was concerned. If one had a powerful imagination one could deduce wonderful diagnoses, but he did not think practical conclusions could always with safety be drawn from the X-ray appearances of bismuth food in the intestine. If one remembered that there were 60 ft. of intestine confined in a very small space it was evident there must be large kinks in several places, it being a very difficult problem to decide what was pathological and what was not. Even with the aid of direct observation at operation it would be a long time before definite conclusions could be arrived at. For example, in such a viscus as the stomach, Dr. Jordan would probably say that any delay in its dealing with food was pathological—pyloric, or duodenal obstruction. He had seen delay occur in the stomach when there was no such condition present, and the case impressed him very much. The patient was a boy whom he was asked to examine, and having given him bismuth food, immediate examination showed the stomach to be normal, but he was astonished to find, at the end of the day, that the stomach was still full of food. In such a case one would be inclined to think that there must be pyloric obstruction, but there was not. The boy was very nervous at the time of the examination; he did not relish the food which was given to him, and he felt sick, though he did not say so. The result was—whether on account of the mental impression or the condition of the organ itself—that the stomach did not empty itself. There were so many such factors to be considered that one could not very well state that a diagnosis could be arrived at by radiographic examination only. A parallel case to the one just mentioned was one of Dr. Jordan's cases, a feeble, ill-nourished woman, in whom thirty-seven hours' delay occurred in the ileum. Perhaps she did not relish the food, and so the intestine or stomach refused to deal with it, the delay being due, not to a mechanical cause, such as a kink, but occurring as a symptom of the general condition. He counselled further study of the subject before coming to definite conclusions, and thanked Dr. Jordan for a most interesting paper.

Mr. THURSTAN HOLLAND said he had not intended to speak on the subject, but since Dr. Jordan interested him in his work about eighteen months ago, and showed him his radiographs, he could not help feeling that there was

something in his contention. He had himself made many X-ray examinations, and for the last year he had been on the look-out for those conditions, but he was obliged to say that from his own point of view he had failed to find those kinks at the end of the duodenum or in the ileum. But because he had not found them he did not feel justified in saying that they did not exist. He thought it very likely that Mr. Lane's reputation attracted to him cases of a certain type, at least in greater number than they turned up in at other hospitals. As a result, Dr. Jordan had opportunities of seeing cases of the kind in large numbers. Perhaps he also saw cases which were not usually sent to an X-ray department. Conclusive pictures had been shown that evening proving that the conditions described did exist. And, what was more conclusive to his mind, the cases had been diagnosed by the surgeon in the first place, had subsequently been confirmed by skiagrams, which, in turn, had been verified by operation. He did not think anyone could get away from the conclusions shown by this series of events. Work such as this was very valuable, and he did not think those who did not happen to have seen similar appearances should try to undervalue the conclusions formed by those who had seen them. All would agree to give Dr. Jordan great credit for his work.

Dr. C. J. MORTON asked if Dr. Jordan showed the pictures of the duodenum with the intention of illustrating delay in the duodenum? He had often seen delay in that organ, and had not been able to make up his mind whether in those cases the cause was duodenal ulcer or kinking. It was certainly not always due to stricture or obstruction. In one case which he had—the only one in which he recommended operation—there was no stricture present, yet the duodenum remained filled with bismuth for some time. He would be glad to hear Dr. Jordan's opinion on this point, and what experience he had had of operation in such cases. With regard to kinking of the ileum, he had seen that frequently. In examining normal people, such as students, there always seemed to be apparent kinking between the ileum and the cæcum: there was always more or less of a gap, sometimes partly filled, sometimes a complete gap; and this seemed to be quite normal. His attitude on the matter was that even if there was a kink there it did not matter so long as the ascending and the transverse portions of the colon filled in the normal time. The only cases of real obstruction which he had seen presented a markedly different appearance. The ileum, in the lower coils, was enormously distended in cases of actual obstruction, even when only partial. In one case he examined the patient by X-rays twenty-four hours after the test meal was given, and the coils of the ileum were still as much dilated as they were at the first examination, six hours after the meal. He had seen several cases of real obstruction in the large intestine, and in those cases also there was a great increase in peristalsis, and a huge dilatation. He had also occasionally seen the writhing in the duodenum and about the pylorus and the pyloric portion of the stomach, as had been described. He agreed with Dr. Jordan that it was not very common, but when present it meant definite obstruction.

Dr. HARRISON ORTON thought Dr. Jordan deserved congratulation on his excellent paper. He had been extremely fortunate in being able to confirm so many of his observations by subsequent operation. Dr. Orton had examined a fairly large number of cases after giving bismuth meals, and he found that one of the chief difficulties consisted in interpreting the various shadows seen. There was no difficulty in finding the situation of the bismuth, but in the small intestine especially there was often difficulty in making out the exact spot it occupied. With regard to kinks, Dr. Jordan pointed out that in the splenic flexure a kink might be thought to be present, but if viewed sideways it was seen to form a U. If other kinks, or supposed kinks, were viewed sideways, it is possible they also might form U's, showing they were not kinks at all. He asked if Dr. Jordan as a rule diagnosed his cases after one examination only, or if, after an interval when the bismuth was cleared out, he gave another meal and examined the case again. In several cases in which he thought he had found kinks after a week or so, when all the bismuth had disappeared, he had given another meal, and no such kink could be found; in fact, the shadows varied very much compared with those at the first examination. With regard to intestinal stasis, one should remember that the habit of the bowel varied greatly in different people. That was especially noticeable to him when he was at Addenbrooke's Hospital, in the Fen country, where many people came to hospital whose bowels were opened only once a month. One lady came complaining because her bowels had been opened as often as once a fortnight. These were perfectly healthy women, but all elderly. Although intestinal toxæmia was very important, it was well to remember such cases as these. Referring to the remarks of Dr. Barclay, he thought much depended on how much bismuth was given with the meal. If the movement of a meal was to be obtained in the small intestine, larger quantities than were usual in bismuth meals must be given, namely, 2 oz. If 4 oz. or even 6 oz. were given—he had given the latter quantity without bad results—the condition could be better observed, as there was enough to spread out and cause a definite shadow. Our chief difficulties at present were due to the fact that the normal had been but imperfectly studied.

Dr. G. B. BATTEN desired to refer to the subject from the standpoint of the general practitioner, who was also a radiologist. He did not see many cases of the kind described, but after listening to the discussion and seeing the beautiful pictures which Dr. Jordan showed, two points had impressed him. One was that he had seen some cases with very decided kinks and obstruction due to malignant disease in which there was no constipation, stoppage, or stasis. One case he had recently was that of an old gentleman who was perfectly sane, and who felt he was quite full up inside. He had malignant disease behind the peritoneum, and very distinct kinking in his transverse colon and at the duodeno-jejunal junction. He got on extremely well as far as action of the bowels was concerned, although eventually he died from the carcinoma. With regard to the practical results of operation in these cases, he had seen several

of these cases which had been operated upon by Mr. Lane and others, and there could be no doubt about the great improvement. But he agreed with Dr. Hertz and other speakers that the patients still remained nervous and retained some of their other symptoms. In particular he referred to two ladies who had had the short-circuiting operation done by Mr. Lane; they both improved in complexion, but their constipation had not been cured, though they lived much happier lives.

Dr. G. E. BOWKER desired, as a provincial member, to add his word of thanks to Dr. Jordan for his paper, which he had found very instructive. The method pursued was so well known to members of the Section that the author did not consider it necessary to speak of the technique followed. He wished to know the amount of bismuth which Dr. Jordan usually gave for a test meal, and what particular salt of bismuth he preferred. He asked if he had had experience of the preparation known as "Kontrastin," sold by Messrs. Siemens Brothers, and which was shown at the Birmingham meeting. If he had tried it, did he consider it an advantage over the ordinary bismuth salt generally used?

Dr. HORACE MANDERS desired to call attention to a possible error due to the amount of bismuth given. Bismuth was a very heavy substance, and therefore it probably did not pass down the intestine at the same rate as did the other food. He had experimented with several foods which evidenced their presence more or less unpleasantly when they had passed through the intestines, and found that the natural food passed through severa hours before the bismuth. Did not the weight of the bismuth prevent it mixing with the other substances in the meal intimately, causing the occurrence of pockets in the food? This would depress the intestine, and cause the appearance of a stricture below the depression. He thought this was the case just before the food arrived at the ileo-cæcal valve, and there was no doubt that bismuth had a difficulty in getting there. Some of it passed through the ileo-cæcal valve and then there was a drop. When the peristaltic movement had sufficiently recovered itself some more went through, and there was a gap between, which gave the appearance of an obstruction. Many years ago, when he first commenced practice, it was the custom in the country to give large doses of mercury in cases of intussusception. He had seen acute intussusception recover after giving 2 or 3 lb. of mercury. He wished some of the radiographers, after they had tried the bismuth meal, would supplement their observations by giving the patient 2 or 3 lb. of mercury, to see whether this would pass through the intestines more quickly than the bismuth meal.

Dr. HENRY DUTCH desired, as another general practitioner, to add his testimony to that of others who had spoken. He had seen large doses of mercury given in Dublin for constipation. With regard to bismuth, it occurred to him that the bismuth exerted a certain therapeutic action on the peristalsis of the intestine. It was commonly given in all forms of diarrhœa, and in such

large doses it possibly had a paralysing effect on the peristalsis of the intestine, and so might actually cause kinking in different places. That might account for some cases showing kinking, and others not.

Dr. A. C. JORDAN, in reply, thanked members very cordially for the interest they had shown in the discussion of the paper. As the hour was late he could not reply to all questions in detail, especially as most of them had been answered in advance in the paper itself. He had nothing to say to a man who would not face facts. The conditions he had described were all illustrated by actual photographs taken by himself in cases he had himself observed. He had made mistakes at first, but with increasing experience his diagnosis had become more and more accurate. After his examinations, if there were an operation he made a point of attending, and observing the actual condition of the organs; hence there was no possibility that his interpretations were erroneous: they had been confirmed beyond dispute. With regard to the terminal coil of the ileum, he had shown them examples of all stages from the normal ileum to the most marked kinking with severe obstruction. He had explained the manner of ascertaining, by the fluorescent screen, the presence, the situation, and the degree of ileal kinking, and they would see that the accidental occurrence of a gap in the terminal coil had no part in the diagnosis, while the fact of the shadow being darker above or below a kink was of no diagnostic value. In the case of the duodenum, he had shown them examples of all stages from the normal duodenum to the duodenum exhibiting the most extreme obstruction due to kinking, in which the duodenum had been seen to be writhing in its endeavour to propel its contents into the jejunum. He had actually seen all stages confirmed at subsequent operation; he had seen the duodenum distended, congested and prominent, in some cases ulcerated. He was convinced that this intestinal work was of the greatest importance; there was much work to be done, and the more workers there were engaged upon it the better. He would be most happy to give any assistance in his power to those engaged upon X-ray diagnosis. Correct method was essential to success. Doubtless Mr. Thurstan Holland was right in his suggestion that he (Dr. Jordan) had unusual opportunities of examining cases of intestinal stasis. Mr. Lane saw many cases of the kind, and Mr. Lane recognized cases when he saw them, which was more than could be said of many surgeons. Dr. Ironside Bruce had suggested that food might remain in the stomach for many hours if the meal were unpalatable, and had added the suggestion that delay at the end of the ileum might be occasioned similarly. He (Dr. Jordan) had shown them instances of delay in the ileum amounting to thirty-seven, fifty-seven, and eighty-five hours in different cases. If this delay were due to subjective causes, how was it that the patients, from being thin, wretched women, to whom life was a misery, had been transformed into healthy, happy, plump women, thoroughly enjoying life? This had been the result of the operation of "short-circuiting." There was no room for the exercise of the imagination. In the case of the rectum it had been suggested that the accumulation of fæces in the rectum might cause great

dilatation. Quite so, but it could not give rise to a rectum that was fully four times the normal length, and greatly dilated as well. In the particular case he had illustrated the rectal elongation and dilatation represented merely a part of the case; there was marked stasis in the ileum and also in the transverse colon, which was greatly dropped. This was the case of a girl, aged 11, with an old tuberculous hip that refused to heal, her general condition being very bad. Her health began to improve immediately after the operation of "short-circuiting," and the tuberculous disease began to heal. One speaker mentioned the condition in which the bismuth meal reached the rectum in the normal time, and was retained there for lack of the rectal reflex; this was of no clinical importance; it was easily dealt with by rectal injections. He was extremely gratified by the interest his paper had aroused, and he would end by repeating his invitation to his colleagues to join him in following up this most important and valuable means of diagnosis.

Electro-Therapeutical Section.

December 15, 1911.

Mr. A. D. REID, President of the Section, in the Chair.

Demonstrations.

(I) By N. E. ALDRIDGE, M.B.

PAPERS had been read before the Society and skiagrams exhibited showing abnormalities of ossification of the tubercle of the tibia. The abnormalities shown were mostly separation of the centre of ossification in the tubercle of the tibia, and the skiagrams showed a tongue-like process separate from the shaft, with a clear unossified space between. Some were described as Schlatter's disease. Dr. Aldridge had not seen this appearance reproduced in any book as belonging to a normal knee. He had some skiagrams to show the condition, and hoped members would express their opinion as to whether it was normal or not. Last November a boy was brought to him with the history that he had suffered pain in the right knee, but not enough to disable him. During the last few months his doctor had not allowed him to play football. The skiagram showed the separation referred to; but the other knee showed an even more exaggerated condition, yet he had not complained of pain in it. The boy's sister, aged 16½, had not the condition. There was a history of injury to the knee caused by a fall. A month later he was asked to radiograph the knees of a girl, aged 14½, who had complained of pain; there was no history of traumatism, and she was not troubled much, as she was following the hounds on foot. Dr. Aldridge showed a number of other skiagrams revealing the same condition. In only one out of ten cases of this kind was there any history of traumatism, and in only two was there a history of pain. Since taking these skiagrams he had met with other similar cases. He showed also skiagrams of a large kidney stone, of peculiar shape. The stone was removed from the pelvis of the kidney, but the latter was not removed.

The next picture was a stereoscopic radiograph taken in 1911, of the left chest of a man who was wounded at Spion Kop—nearly twelve years ago. His left arm was so shattered that it was amputated. He developed empyema. The pieces of lead or shell could plainly be seen well within the cavity of the chest. The way in which the rib had ossified was also interesting. The man was now in good health, and was employed by the Government as a messenger.

DISCUSSION.

The PRESIDENT (Mr. A. D. Reid) said the condition shown in the first series seemed to be fairly common, and appeared to have no pathological significance. He had seen several cases where the process had been broken into two, or had developed in two halves. In one case the boy had a fall on the knee, and it looked like a fracture. The other knee had never touched the ground, but it was developed separately in two pieces.

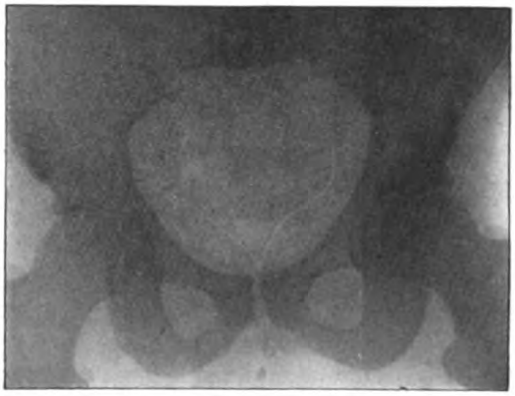
Dr. ORTON said that in cases sent to him for X-ray examination as Schlatter's disease, he made a point of examining the other knee, and in nearly all the two knees were practically identical. The appearance was so common that it must be regarded as normal; it occurred perhaps in 70 per cent. of all young persons examined.

(II) By W. IRONSIDE BRUCE, M.D.

THE first slide was one of a concretion in the appendix. Dr. Bruce would not have felt certain that it was a concretion in the appendix if he had not seen the case shown by the President at a previous meeting. The symptoms pointed to either right renal colic or colic from the appendix; but he was able to give a definite opinion from the skiagram. Operation was performed and the concretion removed. The next slide (fig. 1) showed a male pelvis, in which both ureters had been catheterized. The cystoscope was removed, and the bladder was emptied, so that the position of the ureters was as near normal as one could expect. The outline of the ureters had been marked by pencil, so that it could be seen they were placed away from the position in which the appendix concretion was placed in the previous slide, this point being of importance, since one was often asked to exclude the possibility of stone in the ureter in suspected appendix cases. The next was of the diverticulum of the bladder, in which the whole bladder and the diverticulum were filled with bismuth emulsion. The emulsion has passed up the ureter to some extent, the valve at the lower end of the ureter being destroyed, so that the bismuth emulsion not only filled the bladder, but passed some distance up the ureter.

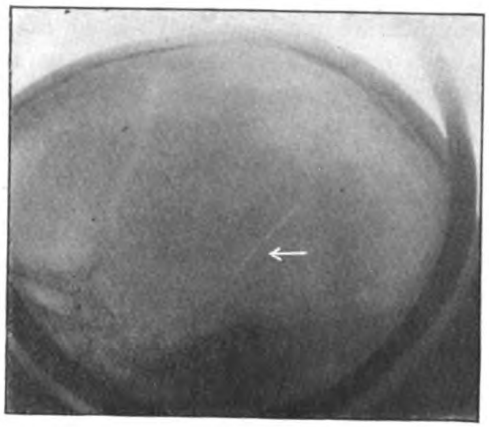
In the next case collargol had been injected into the pelvis of the

kidney. The kidney had been fixed surgically some time previously, and as a result the position of the ureter was abnormal, lying almost across the middle line. The object of that examination was to exclude the possibility of developing hydronephrosis, due to a kink in the ureter. The line of the ureter, however, can be seen to be straight, and the pelvis not dilated.



R. FIG. 1. L.

"Fixed" view of the pelvis with the normal line of the uterus indicated.



Anterior. FIG. 2. Posterior.

Fracture of the skull involving the base.

The next slide (fig. 2) was from a case of fracture of the skull, and the line of fracture appeared as a straight line, extending towards the base, and involving the sella turcica. In this skiagram irradiation was directed laterally through the skull, the head being held with the Reid's basal line placed horizontally.

The next was from a child, aged 5, in which there was obvious obstruction of the œsophagus, as the food stayed there for a long time. After the bougie had been passed food passed well for some time; but two or three weeks afterwards the condition recurred.

The last was of an example of Charcot's disease of the knee-joint of the atrophic variety, and the slide showing the absorption of the ends of the bones and the osteophytic formation.

DISCUSSION.

Dr. HARRISON ORTON said he was much interested in the skiagrams of the concretion in the appendix, and he hoped to be able to show, at the next clinical evening, one which he had had, in which there were ordinary shot. The patient was very fond of game, and every time she swallowed shot the appendix had its mouth open, so to speak, waiting for the shot to drop in. She was sent to him for an X-ray examination, the question being whether there was something in the appendix or stone in the ureter. On examination he found two shadows, rather wide apart, of peculiar shape, and very dense, so he thought they must be metal. Mr. Berry operated, and removed nine ordinary medium game shot from a practically normal appendix. One shadow was quite an inch from the other, each shadow consisting of a group of shot.

The PRESIDENT inquired as to the composition of the concretion, as it showed so well. In the case he himself showed on a former occasion the patient had had a bismuth meal just previously. Such cases must be rare; he had only once before seen one. It was common to be asked to exclude the presence of a stone before an appendix operation was to be done. If in this case the concretion was only a small quantity of fæcal material, he was surprised it showed as well as it did.

Dr. STANLEY MELVILLE said the skiagram showing the direction taken by the ureters was rather a novelty, and the investigation was a valuable contribution to our knowledge. He would like to know what relation the line of the ureters taken in the picture bore to the sacro-iliac synchondroses; also was it not a fact that recent investigation showed the line of the ureters to be a little nearer the middle line than one had been led to believe it lay with regard to the acetabulum? The position with regard to the sacro-iliac synchondrosis was very important with respect to these concretions in the appendix.

Dr. DAVID ARTHUR said he appreciated Dr. Bruce's pictures, but there were many factors which had not been taken into account. It was not reasonable to draw a line where the ureters passed from the kidney to the bladder, as that line varied according to the distance of the lamp from the patient. If one put the lamp very far away, the ureter line would be seen to be tolerably near the vertebræ, whereas if it were put close to the body the ureter line was shown to the outside. The line varied according to where the tube was placed, even though it was focused over the symphysis pubis.

With regard to the colon, a good many foreign bodies were in the colon. Recently he had had a case of what he thought at first was stone in the kidney, but after a further dose of castor-oil the concretion disappeared: it seemed to be exactly where a shadow of a stone would appear.

Mr. JAMES TAYLOR (Bristol) asked what was the strength of the solution of collargol that was used. He had radiographed a case in which a 10 per cent. solution was used, and it gave no shadow. To test it, he soaked some cotton-wool in the solution, and with this also he got no shadow on the plate.

Dr. IRONSIDE BRUCE, in reply, said he was sorry he could not say definitely what the composition of the concretion in the appendix was, although it was removed in King's College Hospital. No bismuth meal had been given so far as he knew, the patient having lived abroad, and it was therefore unlikely he had bismuth given for diagnostic purposes. He believed that it was a calcareous concretion. In answer to Dr. Melville, the first time he had the opportunity of ascertaining the position of the ureters was on a post-mortem specimen, the tissues of which had been hardened in formalin. The position of the ureters indicated in his atlas was not quite correct, but in the view just shown the patient was alive, the ureters being catheterized with the bladder empty, and the cystoscope removed. The ureters passed further inside the sacro-iliac synchondrosis than he thought they did, and further away from the brim of the pelvis, which might be accounted for by the fact that in one case the ureters were plotted out from the post-mortem subject and hardened in formalin, and in the other there was no hardening of the tissues. He believed Mr. Thomson Walker used 20 per cent. strength of collargol, which was sufficiently opaque, there being no reason why such strength should not be used, since collargol was inert. In answer to Dr. Arthur, Dr. Bruce said that his system of radiography could not have been studied assiduously by him, because the minimum distance which the tube must be placed from the part was definitely laid down. If this were reduced, naturally there would be distortion of the image; for if the tube were placed nearer than 3 ft., rays which were not parallel were used. Mr. Hurry Fenwick had published a book in which he analysed a large number of cases of stone in the kidney, and he placed great reliance on the relative positions of the opacities, moreover, Mr. Fenwick was very successful in recognizing and removing renal calculi. It was not unreasonable, therefore, to rely on the relative position of the opacities; on the contrary, it was essential that a knowledge of the relative position of abnormalities of the urinary tract in X-ray shadows should be acquired.

(III) By G. E. BOWKER, M.D.

DR. BOWKER showed a picture of a fractured pelvis, taken as near the plate as possible, so as to avoid exaggeration. He took the skiagram in the first place with the plate under the sacrum; there was so large a gap that he attributed part of it to the exaggeration due

to the position. These were taken twelve months after the accident happened. It was generally said that fractures of the pelvis were due to very great weights coming upon it, but this was not the case here. The patient was a solicitor, and his age 30. He went to Ceylon, where he had much horse-riding to do. His horse started bucking as soon as he got into the saddle, so that he sat far forward, and every time he came down the animal's plunge met him with the pommel and each time gave him a blow in the pubes. A fracture of the pubes was produced, causing him to faint and he fell to the ground. There was also extensive damage to the soft parts. He showed the skiagram so as to ascertain what members thought of the liberties which might be taken twelve months after such an injury had occurred to the pelvic bones and when the skiagram showed a considerable gap to be existing between the old bony structures. Would it be safe, for instance, for the patient to jump down from a high dog-cart now, seeing that this would throw the whole weight of the body suddenly on one half of the weakened pelvis?

DISCUSSION.

The PRESIDENT said the point was whether there was mobility between the fragments; and whether pressure from the two sides resulted in any degree of approximation of the two portions.

Dr. F. BAILEY (Brighton) said he had a similar case a few months ago, in which there had been an accident of much the same kind, the horse having rolled over the man; there was considerable separation, but not so great. The man had been unable to resume his duties, which involved a good deal of riding.

Dr. HARRISON ORTON said he had met with two instances of similar accidents. One was four years ago, when a man, being thrown in a steeple-chase, had a very wide separation of his symphysis. The patient was put up on a double long Liston support, with metal fittings, and the services of the local blacksmith were secured to wind it up. Four nurses, two at night and two in the day, were employed to move him about. When Dr. Orton saw him last there was an apparent gap of ½ in., but the skiagram was taken with a portable apparatus, with the plate on the back. Owing to the exaggeration, the true gap was probably very much less. The man was now riding again. The other case he saw this year; there was separation, but he could not find anything else. The same treatment was adopted. When he examined it again two months after, the gap was very much smaller, and there seemed to be a fracture through the ramus of the pubes also. It happened five months ago, and the man was now walking about, but he had not been able to ride. In symphysiotomy, which used to be done for gynæcological purposes, there

was no wiring of the pelvis, and therefore there seemed no reason why, after these accidents, there should not be a useful pelvis afterwards. In such cases a fracture was found frequently somewhere else, not always a springing of the sacro-iliac joint. But as they most often had to be examined with portable apparatus near the scene of the accident, it was possible that such fractures were sometimes overlooked.

(IV) By H. Lewis Jones, M.D.

Dr. H. Lewis Jones showed photographs of a severe case of X-ray burn followed by carcinoma, the result of excessive X-ray treatment of lupus. The patient had had from 70 to 100 applications of the rays to both sides of the neck; the history was meagre, as the patient was only seen on one occasion. The X-rays had been given by an unqualified person.



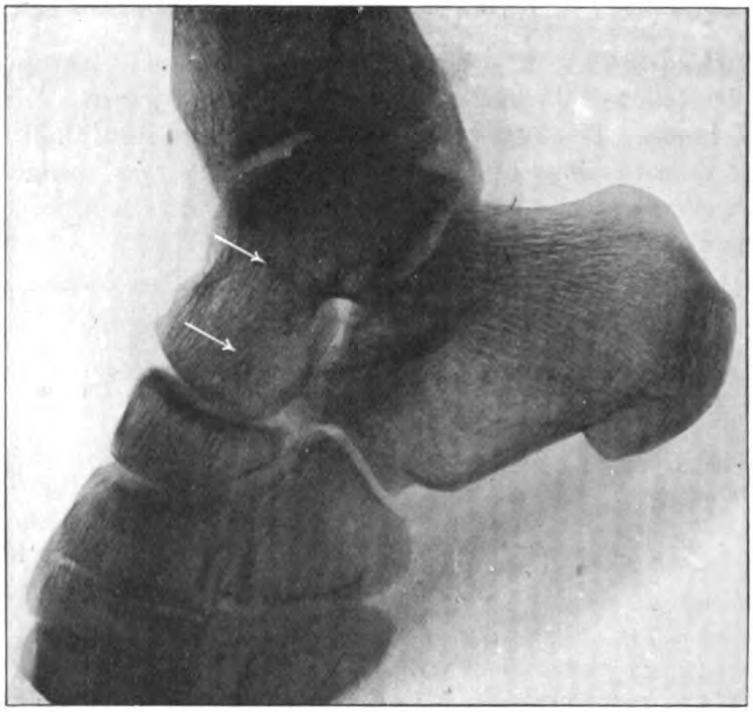
Atrophy of the left pectoralis major.

Dr. Lewis Jones also showed a photograph of a case of atrophy of the left pectoralis major (*see* figure). The case was probably congenital. The patient, a girl, considered that the muscle had always been thin on that side. She attended at the hospital because she had felt increasing weakness at her work, which was that of a book-folder, and entailed a good deal of use of the left upper limb. The breast on that side was smaller than its fellow.

(V) By C. J. Morton, M.D.

Dr. Morton exhibited a skiagram showing a dense opaque spot in the head of the astragalus in a case of tarsalgia (*see* figure). The case had been treated as tuberculous, but there was no sign of tuberculous disease present. The patient said that every time he knocked the foot

the pain was intense. He said the ankle was sprained in August, 1906, adhesions were broken down in August, 1907, and there was some disability until 1909, when he consulted a surgeon and had an operation for "tubercle." For a year after that there was no improvement. At the operation one dense opacity was removed; originally there were two. He had never seen a similar case.



Skiagram of case of tarsalgia. Note opaque spots in astragalus mistaken for tuberculous foci.

Dr. LEWIS JONES said he had seen similar patches of opacity in the foot-bones of a boy, and that the question of tubercle had been raised in that case, but had been ruled out.

(VI) By A. D. REID.

MR. A. D. REID showed a series of slides of skiagrams, taken at intervals during two years, of the spine and wrist of a child suffering from tuberculous disease, demonstrating the progress of the formation of new bone in the affected parts.

Electro-Therapeutical Section.

January 19, 1912.

Mr. A. D. REID, President of the Section, in the Chair.

The X-ray Prognosis of Fractures.

By CHARLES JAMES MORTON, M.D.

THE subject of prognosis of fractures is, I think, of sufficient interest to bring before you. I understand that it has not yet been considered by this Section, and a discussion of a subject which forms a large part of the daily work of most of us should prove not only interesting, but helpful in clearing up many doubtful points in the present methods of diagnosis, prognosis and treatment.

I do not propose to refer to the question of diagnosis except to say that, in my opinion, the large number of cases of disability in private practice is directly due to the neglect of X-rays in diagnosis and in the guidance of treatment. Indeed, the results obtained in general practice, taken as a whole, are now little if at all better than they were before X-rays were discovered, and one is constantly meeting with cases of "neglected fracture," and with more or less disastrous results to the patient. This is the more surprising because the recognition of the value of X-rays in the Law Courts has greatly increased the legal liability of general practitioners to actions for malpraxis.

The subject of treatment is so intimately connected with that of prognosis that it must to some extent be considered, but as far as possible I shall confine myself to the question of prognosis, treating it solely from the X-ray point of view, and bearing in mind that the ultimate functional result is the all-important one.

In drawing up a scheme of the factors necessary to form an opinion on the prognosis, it is first of all essential that the more important causes of disability should be recognized. At first sight disablement would appear to be the direct result of mal-union, but the X-rays have

proved that in many cases, where the after functional result is quite satisfactory, apparent malunion is present. As it is obvious that the functional result is more important than the X-ray picture, I undertook an investigation of a considerable number of old cases of fracture with disability, in order to ascertain the chief causes of loss of function. (Many of these were under treatment at the massage department of Guy's Hospital and for the opportunity of examining them I am indebted to the various resident surgical officers, and especially to Messrs. E. C. Hughes and W. H. Trethowan.) The results I found were briefly as follows:—

(1) The chief and much the most frequent cause of permanent disablement is angular displacement.

(2) A much rarer, but when present an equally important cause, is severe joint implication.

(3) Some form of bone or joint inflammation is comparatively common and I found that not infrequently a trophic rarefying osteitis was the sole and unsuspected cause of the disability.

Overlapping and transverse displacement do not of themselves imply a loss of usefulness and their presence is unimportant unless accompanied by one of the real causes of disability.

With these facts to form the basis of the scheme, I think that the following are the principal factors that require consideration in forming a prognosis in a case of recent fracture:—

(1) The presence and amount of angular displacement.

(2) Implication of joints.

(3) The proposed method of treatment.

Other points that must be considered are the age of the patient, the presence of pain, comminution, rotation in the long axis of the bone, and lastly and of least importance, shortening and lateral displacement. The possibility of non-union in simple fracture is, in my experience, so rare that for the purpose of prognosis it may safely be neglected.

I propose to consider these points very briefly, and perhaps it will save time if I illustrate my meaning by showing a few slides.

To take the least important factors first, I would say that the effect of transverse displacement and of overlapping is generally negligible, provided there is no angular displacement. In transverse displacement accurate apposition is not necessary, and as a rule is not possible to attain without operation. But if the fragments are separated and cannot be brought into actual contact by careful manipulation, there is probably some intervention of muscle or other soft tissue, and this necessitates

operative interference. If, then, after an attempt at reduction any parts of the fractured surfaces are actually or nearly in contact, it is best to leave well alone. The only exception to this is when the fracture is close to a joint and the displaced fragment interferes with joint movement or with the function of the muscles or tendons in its neighbourhood. In the same way shortening, if excessive, should be corrected. I think it is a safe rule to regard overlapping in the lower limb, up to 1½ in., as unimportant. This amount will be compensated by tilting of the pelvis. For example, athletes with moderate shortening find as a rule no inconvenience, and in one case, that of a Rugby football player, the patient assured me that his inch of shortening actually improved his game.

The question of angular displacement or loss of alignment is by far the most important factor. A comparatively small degree of angularity seriously affects the prognosis, and in the lower limb especially may lead to a disastrous loss of function. Although the ill-effects of pronounced angular deformity in the upper and lower thirds of the femur are well known, the effect of a slight deviation in the axis of the limb has not been fully appreciated. When there is a loss of alignment the joints above and below the fracture are thrown out of gear and disablement inevitably follows. These arthritic changes, which are usually permanent and progressive, have hitherto usually been ascribed to rheumatism or rheumatoid arthritis, but are almost invariably the direct result of angular displacement and the rarefaction of bone or pressure changes that follow it. Angular displacement, when it occurs near a joint, is shown by a tilting or rotation in its horizontal axis of the smaller, usually the lower, fragment. This is seen in the two fractures, Colles's and Pott's, in which undoubtedly mal-union is most common. The names Colles's and Pott's are here used as convenient but not scientific terms to denote fractures with definite backward or lateral displacement. In Colles's fracture the lower fragment is frequently rotated backwards. It may be displaced backwards only, but usually there is some degree of both conditions. If either is present it is absolutely necessary that the displaced fragment be properly reduced, otherwise the stretched extensor tendons inflame and mat together and the joint is either temporarily, or more often permanently, impaired. As a rule, and especially in impacted cases, reposition can only be effected by levering in the fragment with a screwdriver after cutting down on the parts. Once the fragment is in its place it stays there, and therefore splints are not required. These fractures about the

lower end of the radius are, in my experience, more often overlooked than any others, probably because the rapid swelling obscures the position and an accurate diagnosis by the ordinary methods becomes impossible. Consequently they are often treated as sprains and the actual condition is only found out when disablement appears and induces the patient to seek further advice and an X-ray examination on his own account. If splints are applied while the fragment is unreduced the prognosis is infinitely worse than when the case is regarded and treated as a sprain. Whatever the method of treatment early movement is essential.

Fracture of the external malleolus of the fibula without displacement is one of the commonest of bone injuries. In these there is no tendency to after-displacement provided the patient is not allowed to use the limb and so possibly convert it into a true Pott's fracture. They are often undetected and these cases do extremely well. Indeed, so long as the ankle is not allowed to become stiff any method of treatment will give a satisfactory result. As in Colles's fracture, treatment without splints and with early massage and movement offers the best prognosis. Unfortunately, these cases are usually termed "Pott's," or more recently "abduction" fractures, and are included in the statistics of treatment of displaced fractures, such as the true Pott's and Dupuytren's varieties. As a consequence, these statistics are quite fallacious unless the character and amount of the displacement, as well as the nature and position of the fracture, are indicated. In this connexion the relative frequency of displacement in simple fractures is interesting. On looking over the records of a series of some hundreds of cases I found that in a considerable majority there was no appreciable displacement. Of the displaced fractures, most were more or less easily reduced. In the remainder, comprising, I think, about one-sixth, and certainly not more than one-quarter, of the whole, reduction could not be effected without operation. It will be seen, therefore, that practically all the useless limbs come from a comparatively small group of cases and also that it is useless to compare the results of the various methods of treatment unless the larger groups of non-displaced and of easily reduced fractures, in which the method of treatment is not of much importance, are excluded from the statistics.

In fractures about the ankle-joint with any degree of lateral or backward displacement the prognosis must be extremely guarded. Here it is not the fracture that matters but solely the amount of displacement of the astragalus and consequently of the whole foot. As a rule these

displacements are very difficult to reduce and often require operation. If they are not accurately reduced the best that can be expected is that the patient will be able to hobble about until arthritic changes entirely disable him. Even when they are reduced and the immediate result appears satisfactory re-displacement may occur as soon as the patient begins to walk. The astragalus is often rotated outwards as well as displaced, and if left in this position the patient has to walk on what is practically a knife-edge—the outer edge of its upper articular surface. Re-displacement may result from rupture of the inferior tibio-fibular ligaments and the astragalus gradually becomes wedged in between the two bones. The occurrence of rupture cannot always be shown by the rays, and it is therefore specially necessary in any doubtful case to ascertain the proposed method of treatment. The usual methods of operation do not always prevent secondary displacement, but I think a good prognosis may be given when the method adopted by Mr. R. P. Rowlands is employed. In this method a screw is passed through the lower fragment of the fibula into the tibia. It lies well above the joint, so that this is not interfered with and early movement can be employed. Angular displacement and tilting can only be recognized with any degree of certainty by the use of the X-rays. A slight amount of angular displacement, quite sufficient to cause permanent disablement, cannot be determined by the ordinary surgical methods of examination and an X-ray examination is absolutely essential. The best method of examining is by the use of the screen, because by rotating the limb any degree of angularity in any position can be determined; whereas, if skiagrams are taken in two fixed positions, antero-posterior and lateral, the presence of angular displacement is very likely to be overlooked. Rotation in the long axis of the bone is, with perhaps the exception of fracture of the shaft of the radius, not so important as tilting. Outward rotation due to the weight of the foot no doubt frequently occurs in the lower limb, but this is easily recognized without X-ray examination if the relative position of the malleoli be taken as the guide instead of the frequently deceptive position of the foot.

The next important factor, the implication of joints, seriously affects the prognosis when one part of the articular surface is displaced to a different level from the rest. This is frequently the case in T-shaped fractures about the knee- and elbow-joints, and is often a deciding factor in favour of operation. Fissures running from the main fracture into the neighbouring joint are, I think, more common than is usually

suspected, partly because they are often difficult to demonstrate. For example, they often complicate Colles's fractures, and if not treated lead to synovitis and subsequent stiffness from adhesions. Comminution, although a very frequent complication, does not call for special consideration unless it interferes with replacement or joint movement, when the fragment may require removal. Excessive comminution may contra-indicate operation. It may prevent union, and in compound fracture lead to extensive necrosis.

The age of the patient has an important bearing on the prognosis. For example, in young children, Nature, if left to itself, will join up,



FIG. 1.
Non-union after plating in fracture of ulna.

and in time straighten out almost any fracture in a long bone. Interference is therefore not so often necessary as in fractures in the adult. I show a slide demonstrating the process of cure in a child, aged 3, nine months after a separation of the lower epiphysis of the humerus. It is really a fracture above the epiphyseal line, like the majority of cases that are clinically separated epiphyses. You will see that the periosteum has been stripped off the lower end of the shaft, and that a new lower end has formed from it. The alignment is practically perfect. At present the old lower end projects forwards and limits the movements of the joint, but erosion of the useless bone has already commenced, as

is shown by the translucent areas, and very soon all the diseased portion will be eaten away, and a perfect joint result. I have not found that simple separation interferes to any extent with the subsequent growth of the bone, but if there is joint implication and displacement of a part of the epiphysis, a deformed and impaired joint results, and the usefulness of the limb is imperilled.

Pain I look upon as a very important symptom. When a dislocation is reduced the pain disappears. In the same way, when a fracture is properly reduced there is no pain. If it is persistent, it means that there is injurious pressure on the surrounding tissues, and if not

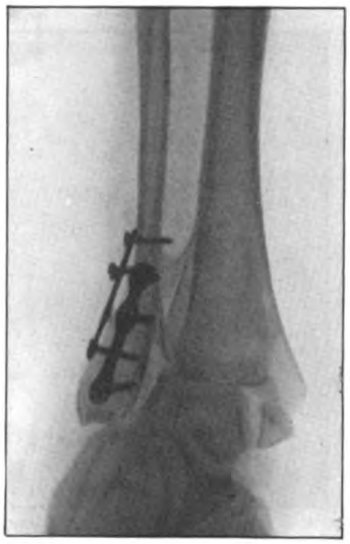


FIG. 2.

Re-displacement following operation for abduction fracture of ankle-joint. Note rarefaction of bone at internal malleolus.

relieved there will be permanent ill-effects. Therefore, even if the fragments appear to be in fairly good position, pain calls for immediate relief by resetting, or, if necessary, by operation.

In connexion with the proposed method of treatment, there are one or two points that must be considered. The danger of the indiscriminate use of splints has already been referred to. The use of pressure pads to obtain reduction of displaced fragments has no effect on the position, and is frequently injurious. Pressure by splinting or pads, as it is ordinarily employed, is only of value in maintaining the fragments in position. Recently, however, attempts have been made

to employ pressure in fractures with angular displacement—e.g., in the femur under the guidance of frequent X-ray examinations—and this method appears likely to be of considerable value in cases in which at present operation is the only available means of effecting reduction. Another defect in the usual methods of treatment is the routine practice of fixing the joints immediately above and below the fracture. If the fragments are reduced, it is not necessary, and in any case it leads to arthritic trouble and loss of function. Then there is the question of the use of extension to overcome shortening. The idea, I believe, is to tire out the muscles by pulleys and weights up to twenty or more pounds, or by more or less complicated extension apparatus



FIG. 3.
Broken wire in fracture of patella.

such as Hodgen's or Bryant's splints. In my experience these methods are absolutely useless. There may be apparent lengthening, but in no case that I have examined before extension was applied, and again some weeks later, has there been any appreciable diminution of the actual overlapping. Even in the case of fracture of the femur in young children treated by Bryant's method of slinging them up by the heels for five or six weeks, the original shortening remains the same. The only value of extension is in relieving pain from spasm of the muscles, and in a lesser degree in maintaining the alignment of the fragments when once this has been secured. Another point in the treatment refers to a very interesting phenomenon which I call "apparent reduction." It is a common experience to find that after the fracture has been set, and

the deformity to all appearances properly reduced, the X-rays show that the fragments are in exactly the same position as they were before. Occasionally the position is worse, especially if an anæsthetic has been given, and the patient has struggled. That apparent reduction is a very real and frequent illusion is easily proved by attempting to reduce a badly displaced fracture on the X-ray table. When the radiologist or the surgeon has succeeded in obtaining what he considers the correct position, and while he is still holding the limb, the rays are turned on, and the position is found to be unaltered. Even in an impacted fracture, such as a Colles's, an apparent reduction can be obtained by manipulation, while the impaction remains unaltered. "Apparent reduction" is a fertile source of mal-union and permanent loss of function. The

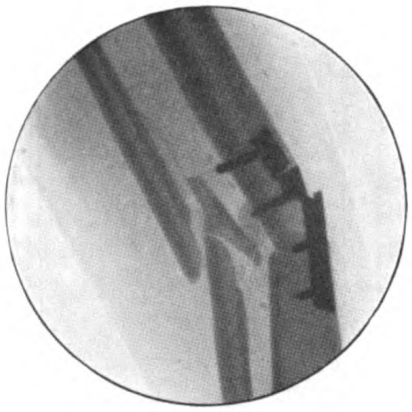


FIG. 4.

Angular displacement following breakage of plate in fracture of tibia and fibula.

moral is, insist on an X-ray examination after the ritual of setting before giving a prognosis in any case of fracture with displacement.

In my investigation of old fractures I found that, in addition to the ordinary forms of bone inflammation, such as osteitis, osteo-periostitis, necrosis, &c., which follow fracture, there is not infrequently a rarefaction of the bone. This is quite distinct from the gross lesions of bone described in the text-books as atrophy or rarefying osteitis. It resembles more the changes known to follow disuse of a limb. In some cases its appearances resemble those of the atrophic form of osteo-arthritis, but without the marked pencilling of the bone outlines. In others it shows as an irregular, blotchy mottling, often at some distance from the seat of fracture. As a rule the changes in the translucency are so slight that careful inspection of the negative is required to detect them, and it is not possible to reproduce them satisfactorily in prints or lantern

slides. It may follow any bone injury, and sometimes may be seen within a few weeks of the time of the accident. I think it is probably due to trophic changes, and, for want of a better name, I have referred to it as "trophic rarefaction." When it is present it considerably modifies the chance of union and contra-indicates operation. Possibly its chief effect is in producing a degenerating arthritis in the neighbouring joints. More probably similar trophic changes also take place in the muscles, joints, and other tissues, leading to a gradual disablement of the limb. Similar changes have been ascribed to the effects of alcohol. Chronic alcoholism, however, is by no means a rare condition in hospital patients, and were it a cause of bone rarefaction its effects would be seen much more frequently, and in other conditions besides fracture.



FIG. 5.

Angular displacement and broken screw in fracture of humerus.

Lastly, in considering the probable effect of operative treatment, it is necessary to bear in mind that the various operations of plating, wiring, &c., are not easy and not always successful. Wires and screws produce erosion of the bone surrounding them, and this very often necessitates a second operation for their removal. Erosion of bone may result in non-union, and, in my experience, non-union is much more frequent after operation than it is after treatment by the ordinary methods.

The slides (figs. 1 to 6) show some of the defects that follow operation. They demonstrate the need of giving a guarded prognosis in all cases requiring operative measures, and especially of a carefully considered

answer to a question which is usually put to us—is an operation necessary? I think that the proper answer depends entirely on the X-ray evidence. If there is irreducible angular displacement, or any other condition that is likely to cause an unsatisfactory ultimate functional result, an operation is imperative. In doubtful cases the patient, of course, should have the benefit of the doubt. But in the great majority of the cases that result in permanent disablement there is no question of doubt as to the proper treatment; the only possible treatment to prevent disability is immediate operation, and the operation must not be deferred, because, once the callus becomes organized, the difficulties and dangers are much greater.



FIG. 6.

Rarefaction of bone following plating, with re-displacement in fracture of tibia and fibula.

It is, I think, largely owing to the difficulties and uncertainties of the present methods that operation is not more frequently employed. In compound fractures, for example, the result of introducing foreign bodies has been so disastrous that this method is now employed in exceptional cases only. The majority are treated by simple replacement —that is, without the use of retaining wires, &c. The same method is also employed in simple fractures in the neighbourhood of joints, and by some surgeons in treating mal-united fractures of the shaft. In these, as a rule, there is no serious secondary displacement, and I have seen many excellent results in cases treated in this way.

This comparatively simple method of operation might well be extended to many other varieties of fracture. When once it is recognized that moderate shortening and transverse displacement are quite unimportant, and that accurate apposition is not essential, the present methods of wiring and plating will, I think, be reserved for exceptional cases, and simple replacement by operation will become the routine method of treating the very numerous displaced simple fractures that, at the present time, are a frequent cause of vicious union and disablement.

DISCUSSION.

Dr. C. FRED. BAILEY (Brighton) agreed with Dr. C. J. Morton in laying stress on the need for recognizing exactly the position of the fractured bones before any attempt was made at reduction; this could only be decided by X-ray examination. He did not think enough had been made of the advantage of stereoscopic radiography in these cases. It was a simple matter to take two stereoscopic photographs of most fractures on a 12 in. by 10 in. plate, and if this were done in two opposing diameters, most perfect information could be obtained. The other lesson from the paper was the necessity of an X-ray examination, after "reduction," to decide whether the bones had *really* been replaced in their proper position. The extensive bone changes of an atrophic nature, which seemed more common after the use of screws, plates, or wiring, would come as a surprise to many. He gathered, Dr. Morton thought that, in many fractures, satisfactory reduction was impossible except by operation, but, when once the parts were replaced correctly, there was but little need to screw or wire them there. He agreed as to the great value of early passive movement and massage.

Dr. REGINALD MORTON congratulated the author on the very courageous way he had attacked the subjects of the prognosis and treatment of fractures. Personally he would not have been so bold himself, as experience had taught him that, as a rule, surgeons were somewhat sensitive to any wandering from his own domain on the part of the radiographer. The author said he considered that the mistakes made in general practice were now just as bad as ever. This might well be the case, and it was just possible they were worse, and for very good reasons. The tendency at the present time, both in our hospitals and elsewhere, was to shirk the clinical examination of fractures. It was so simple in a suspected case for the X-ray department to do this, that it was rapidly becoming a regular practice. This had now been going on for several years, and it was bound to have a prejudicial effect upon the general degree of skill in the clinical examination of fractures. At the same time he considered the change was the better for the patient, inasmuch as an accurate method of diagnosis was substituted for one that was frequently erroneous; also it was better for the doctor, especially at the present time when an error in diagnosis, even in an admittedly difficult case, might be followed by very serious consequences to

himself. There was a very large casualty list at the hospital where he worked, and he could endorse most of the observations with regard to fractures, and also the frequency with which any attempts at resetting a fracture (by other than operative means) were unsuccessful. After the lapse of but two or three days the difficulties of resetting by ordinary means were very great indeed, and this was particularly the case where the line of fracture was oblique, with wide separation at the time of the injury. This was probably due to some of the soft tissues getting between the fragments. While he admired the enterprise of the author of the paper for tackling the subject in the way he did, he (the speaker) had not given much attention to the question of prognosis from the X-ray findings; this was intimately bound up with the question of treatment, which was not within the province or control of the radiographer.

Dr. IRONSIDE BRUCE did not think that anybody at the present day would say that the X-ray was the only method by which certain injuries of bone could be recognized, but certainly it was the best. Any person might go wrong as a result of clinical investigation, but by X-ray examination, properly carried out, he did not think it was possible to miss an injury to bone. Certain points in Dr. Morton's paper were so surprising that he wished to comment upon them. It was a new proposition that only angular displacement—he presumed the author referred to injuries to the shafts of bones—had an influence upon the function of the limb. Surely some lateral displacements must make a difference to the functions, even if they did not involve the joint. With regard to treatment, he was rather astonished at what the author said, because he was under the impression that the function of the X-ray examination was only to discover the exact extent of the injury. He did not consider that from the X-ray examination one was entitled to make statements with regard to the prognosis or treatment, because these conclusions were so involved with the clinical aspect. If a patient came with a definite injury of bone it would be impossible, from the X-ray examination alone, to make a definite prognosis or a statement as to treatment. The function of the X-ray investigator should surely be to make the diagnosis and leave the remainder to a competent surgeon. He had always thought that X-ray examination tended to produce mechanical surgical methods of dealing with fractures. It had been felt by every radiographer on looking at a fracture, how easy it seemed to be to put in a screw, or fix a plate, to correct deformity. It was perhaps this fact which accounted for many attempts being made to treat fractures mechanically. Later, surgeons would be able to state definitely the kind of case suitable or unsuitable for screws, &c., accurate diagnosis by X-rays being essential. Attention had been directed specially to the mechanical means of replacing displacements, but manipulative means aided by X-rays had been largely forgotten. It was astonishing how much could be done by manipulation. For example, in fractures of the lower end of the humerus, in which the injury was as shown in Dr. Morton's case, manipulation and fixation in the position of extreme flexion resulted in wonderful reduction of the malposition, and a most satisfactory result, as he (the speaker) had frequently seen on examination of such injuries afterwards. He thought that by X-ray examination surgeons

might in the future be able to devise satisfactory methods of manipulation and position, at least as far as the treatment of particular fractures was concerned.

Dr. FINZI said he had exceptional opportunities in his work of comparing the results of treatment of fractures by the methods of two different nations, as he was attached to the German Hospital, where the residents were trained in Germany, Austria, or Switzerland, and did not possess English degrees. His comparison of the German methods of treatment with the English was much to the advantage of the latter. The German methods inclined very much more to fixation of the limb by external methods, such as plaster and splints, and used little massage; whereas the English inclined more and more to the employment of massage, sometimes with fixation, sometimes without. He was sure that the prognosis depended far more on the treatment than on any radiographic appearance which could be discovered. He would much rather have the very worst angular displaced fracture treated properly than a fracture with very little displacement treated badly. With regard to the clinical diagnosis, he agreed with Dr. Reginald Morton that the tendency nowadays was to shirk the clinical diagnosis and depend more on the radiographic diagnosis. Radiographers could do much to prevent this by insisting on a clinical diagnosis being stated on the paper bearing the request for a X-ray examination. He contended that one was unable to give a prognosis from the X-ray appearances without considering the treatment which was to be subsequently adopted.

Dr. JAMES C. CASE desired to emphasize what had been said as to the value of stereoscopic plates. Scarcely anyone would make a radiographic diagnosis of a joint injury without taking at least two plates; and those two, made stereoscopically, were, in his view, more valuable than when made singly. The question of time and trouble involved in making stereoscopic plates was very much simplified by the automatic devices now available, and therefore it seemed to him much preferable to make the plates in that way. He congratulated Dr. C. J. Morton on the length to which he had carried his criticism in the paper; it was astonishing to one who usually stood in awe of the surgeon.

Mr. C. R. C. LYSTER desired to emphasize Dr. Case's remark that stereoscopic work was advisable for fractured bones. When a case was sent to him in which it was intended to use a wire, screw, or plate, he made a stereoscopic picture. He contended that no surgeon had a right to attempt to operate on a fracture, especially if near a joint, unless he had seen it stereoscopically. One could not otherwise get a true idea of a fracture, impaction, or displacement. He agreed with Dr. Finzi in insisting on the house surgeon making his clinical examination before the case was screened. He could scarcely agree with what the author said as to separation of the epiphysis not having an after-effect on the growth of the bone. There were some cases in which the epiphysis had been separated, with no injury to the joint itself, in which he knew there had been retarded growth in the bone and marked deformity in after years.

Dr. ARTHUR also desired to emphasize that in all cases of injury of bone, especially in the vicinity of joints, a stereoscopic view should be taken. If

two separate views were taken at right angles to one another, the exact nature of the case was a conundrum. It was particularly difficult in the case of a needle in the hand or in the breast. But if a stereoscopic view was taken the surgeon would be able to locate it at once. He agreed with Dr. Finzi as to the subsequent treatment having an important bearing on prognosis. He had seen very simple fractures in which the treatment had been erroneous, with very bad results; and he had repeatedly seen remarkably good results from bad fractures. His experience of the results of wiring and screwing had been tolerably good; he had seen but few in which the results had been bad. This might be due to his having seen the fractures immediately afterwards; he did not know what would be the condition in a few years' time. In the cases in children in whom the epiphysis was injured, he found the bone did not grow.

The PRESIDENT (Mr. A. D. Reid) said members would not be surprised to hear him endorse the idea of stereoscopic examination; in the majority of cases he considered it absolutely necessary. There were some cases in which an antero-posterior and lateral view gave nearly all the required information; but there were many cases where the facts could not be truly gauged from such pictures. An instance was fracture of the surgical neck of the humerus. One could get the bones in such alignment that the two fragments appeared to be in apposition, and one might say how beautiful the result was. But it would be found that although the fragments were apparently in position they were on totally different levels. He knew of no other way of detecting this than by taking stereoscopic radiograms. He had seen separated epiphyses in which there appeared to be good apposition, and yet in some of these cases the head was at a different antero-posterior plane to the shaft. Another type of case which could present great difficulty in arriving at a correct opinion was a bad fracture with displacement of the tibia and fibula. Without a stereoscopic picture it was impossible to show whether cross-union of the fragments had taken place or not. At hospitals there might be a financial objection to making stereoscopic pictures the rule, but the benefit gained was so great that it had become almost a *sine qua non*. With regard to Pott's fracture, the author said that, if the displacement was corrected, the results were fairly good even in bad cases. He (the President) heard that a leading surgeon, who had much to do with the Police force, had said that he had never seen a policeman who had had a Pott's fracture able to go back and resume his full duty. He agreed with what had been said concerning the use of the screen, but the stereoscope did away with the screen, and the tendency was to reduce the screen examination to a minimum. He had been much puzzled by the occurrence of rarefaction of bone after fracture. A few weeks after correct union of a fractured wrist one might see a patient with a practically useless hand, and on taking a radiogram find every bone in the neighbourhood of the hand intensely rarefied. He thought the explanation must be that it was trophic, as the degree of rarefaction was out of proportion to the injury received. The rarefaction was like that found in bone after infective disease. For several weeks he had had the opportunity of observing a septic thumb. During the

septic stage a very small sequestrum came away, and during the period over which the pictures extended almost every particle of bone disappeared from that phalanx, and it could be moved in any way. It gradually got better and he was able to see the lime salts gradually being replaced. Three months later the bone was nearly normal. He did not know how long it took for the bone to recover its structure completely, or whether it ever did so.

Dr. C. J. MORTON, in reply, said that several members had referred to the stereoscopic method. At Guy's Hospital all cases were examined first by the screen, and then, if it were thought necessary, a plate was taken. A stereoscopic screen was impracticable, and as many surgeons did not appreciate a stereoscopic picture, it was seldom asked for. In fractures near a joint, such as the shoulder, he agreed that in many cases a stereoscopic view was essential to ascertain the exact position. Still, a little transverse displacement had no practical effect on the functional result. He had seen many cases with displacement in which the after-result had ultimately been as good as before, although at first there was stiffness. With regard to muscular contraction causing secondary displacement, this had been described as the chief cause of re-displacement after setting. He did not think that was so, but he had no positive evidence. Dr. Ironside Bruce seemed to think he meant that angular displacement was the only real cause of bad after-results; but he did not go so far as that. He mentioned the causes of loss of function, amongst which he considered angular displacement as by far the most important. Of course in many cases there was transverse displacement associated with angularity. A remark by Dr. Ironside Bruce which surprised him was that he considered the only function of the radiologist was to diagnose fractures—that he was not to give a prognosis, nor to interfere with treatment. With that he did not agree. He thought that because of the number of cases he saw, the radiographer was in a better position than were most surgeons to say what the proper treatment was. In private work he found that when a practitioner brought a case for him to examine, one of the first questions he asked was as to the treatment that should be adopted? The radiographer should be able to answer this question. With regard to the growth of bone being interfered with by injury to an epiphysis, it was difficult to follow up every case, but he could not recall many cases of definite shortening of the bone following a simple separated epiphysis. With regard to Pott's fractures, he believed the reference the President made was to the paper by Mr. Clinton Dent, on prognosis. It was partly that paper which led him (Dr. Morton) to take up the subject, especially from the point of view of prognosis. Mr. Dent said that policemen after fracture of the leg were never able to do their proper work, and attributed this partly to the moral effect. He was glad his idea that rarefaction was a trophic change had been supported. He thought it was different from the change which resulted from sepsis, even from that which followed gonococcal infection. The question of recovery from rarefaction depended on the removal of its cause. An angular displacement rarefaction would not be recovered from until the angular displacement itself had been properly corrected.

The Importance of Stereoradiography, especially of the Alimentary Tract, with Demonstration of Plates.

By JAMES T. CASE, M.D.[1]

IN presenting for your consideration the subject of stereoradiography, I wish especially to call your attention to its application to the field of internal medicine, and particularly to its value in the diagnosis of pathological conditions of the alimentary tract. Within a few months after Röntgen's first publications on the X-ray, Mach [13] had succeeded in producing stereoradiograms, and in 1897 Levy-Dorn [12] published an extended consideration of the value of the stereoscopic method in surgical conditions. Marie and Ribaut [14], Mackenzie Davidson [3], Walsham [20], Hildebrand [8], Eijkman [6], and Walter [21] did extensive experimental and practical work in stereoradiography, and the writings of these men constitute almost the entire literature of this subject for the next decade.

During the early years the apparatus provided for Röntgen workers did not permit of sufficiently rapid exposures to allow stereoradiographic plates of parts subject to the changes of position and relation incident to the movements of respiration. The principal value of stereoradiographic work was in studying the extremities, in anatomical researches, and especially in the localization of foreign bodies.

With improvements in the generating apparatus and the manufacture of better tubes came the widening of the sphere of usefulness of the new method to include examination of the organs of the trunk. Substantial aids to accurate work were (*a*) the device by Hildebrand of the tunnel-plate changer, enabling exact placing of the second plate in the position of the first; and (*b*) the addition to tube stands of mechanical contrivances for accurately adjusting the tube.

Still more recent years have brought us powerful induction coils, larger static machines, and interrupterless apparatus, faultless intensifying screens, and automatic devices for shifting tube and plate, so that it is now possible to secure a pair of stereoscopic plates, even of the abdomen, within a single swing of a metronome beating seconds.

[1] Director of the Röntgen Laboratory, Battle Creek Sanitarium Hospital, Battle Creek, Michigan, U.S., America.

As the improved apparatus became available, the use of the stereoradiographic method was extended to radiography of the thorax, probably first by Walsham, who published a pamphlet on this subject as far back as 1899; then to the bladder and urinary tract, notably by Caldwell [2] in America and Morgan [16] in England. At first the disturbances due to the movements of respiration interfered with the perfection of the stereoscopic effect, but the advent of instantaneous radiography soon made possible such beautiful stereoradiograms of the chest as have been exhibited by Wenckebach [22], Hickey [7], Charles Lester Leonard [10], Beck [1] and others [9] [5] [19] within the last three or four years.

There still remained the field of stereoradiography of the stomach and intestines, where success in perfection of stereoscopic effect seemed less likely of attainment. These are organs, the form and position of which are modified, not only by respiratory movement, but by involuntary movements of their own, which, in the stomach at least, completely change the picture within two or three seconds. Here speed is essential; shorter exposures and still more expeditious change of plate and shifting of tube must be attained in order that the dual shadow may be registered before the rapidly moving peristaltic contractions produce fatal distortions. But by means of the new appliances it became possible to make the two plates, even of deep-lying organs, within a period of time so short that the positions represented are essentially identical.

To Dr. Charles Lester Leonard [11], of Philadelphia, belongs the credit of first successfully producing stereoradiograms of the stomach and intestine. The pair of stereoradiograms of the stomach and of the case of obstruction of the small intestine which appeared in the *Archives of the Röntgen Ray*, these two cases constituting thus far the only published plates of this kind, so far as I am aware, were included in Dr. Leonard's wonderful collection of stereoradiograms of the chest which he exhibited at Brussels in 1910. The wonderful results obtained by Leonard in stereoscopic work on the chest, which he kindly demonstrated to me in his private laboratory in the fall of 1910, led me to devote myself to the development of the possibilities of this method, as applied to the stomach, small intestines, and especially the colon, as a routine method of examination. By December, 1910, I had succeeded in perfecting the stereoradiographic technique which has since been employed as a routine practice in the examination of the alimentary tract in the Röntgen Laboratory

of the Battle Creek Sanitarium. With the improvements of the last year it is possible to complete the two plates in considerably less than a second, though such great haste is only required in cases where the stomach or small bowel is in rapid peristalsis. Slightly longer exposures may be permitted where the stomach walls are atonic and comparatively motionless, or fixed, as by indurations or adhesions, or in the study of the colon. Nevertheless, the greatest speed obtainable is desirable for all this work, the advantage being a more perfect stereoscopic effect.

The twenty-two reductions of stereoradiograms, which I have the honour of demonstrating to you, are representative of the work done in the Röntgen Laboratory of the Battle Creek Sanitarium, a number of the cases being verified by operation by Dr. J. H. Kellogg. I regret that I am unable to present the original negatives, which are, of course, superior to the reproductions.

Technique.

Gehler intensifying screens were used in every case, one being used in each of the two plate-holders. My purpose in employing the reinforcing screen is twofold: First, by cutting down the exposure to from $\frac{1}{20}$ to $\frac{4}{20}$ of a second, I am able to get perfect stereoscopic effect even in the presence of vigorous peristalsis; second, by reducing by ten times or more the strain upon the tubes incident to heavy currents I effect a considerable saving in tubes. This seems to me a very important point in the economics of the Röntgen laboratory. Even in lung plates I use the reinforcing screen, there being no perceptible graining, thanks to the excellent screens now obtainable. The increased speed permits absolutely perfect stereoscopic effect, and if one makes the exposures synchronous with the ventricular contractions, even the heart outlines are shown up clean-cut and distinctly. However, the chief value of this extra rapid stereoradiography of the chest is not that the heart may be shown up stereoscopically, but that the structures about the lung root, the aorta, mediastinal shadows, and the œsophagus are recorded with the least possible distortion or blurring caused by transmitted impulses from the heart or great vessels.

I now employ the interrupterless apparatus of Mr. Snook, operated by 110-volt direct current. A number of the plates exhibited were made with the open-core transformer of the Scheidel-Western Company. The tubes are of American manufacture, chiefly of the MacAllister-Wiggin

Company. The exposures vary from $\frac{1}{20}$ to $\frac{4}{20}$ of a second, according to contingent conditions.

Before making the second plate, I shift the tube a distance of 6 cm. or a trifle less. I choose this distance rather than 6·5 or 7 cm. for the reason that observers with an interpupillary measurement of less than the distance the tube is moved have difficulty in successfully viewing the plates, whereas if the tube is moved slightly less than the interpupillary distance, the stereoscopic effect is secured without difficulty. I do not follow the table of Marie and Ribaut [15], but, as advised by Snook, in a paper before the American Röntgen Ray Society, 1910, I use 6 cm. as a constant factor, and the tube is always moved this distance.

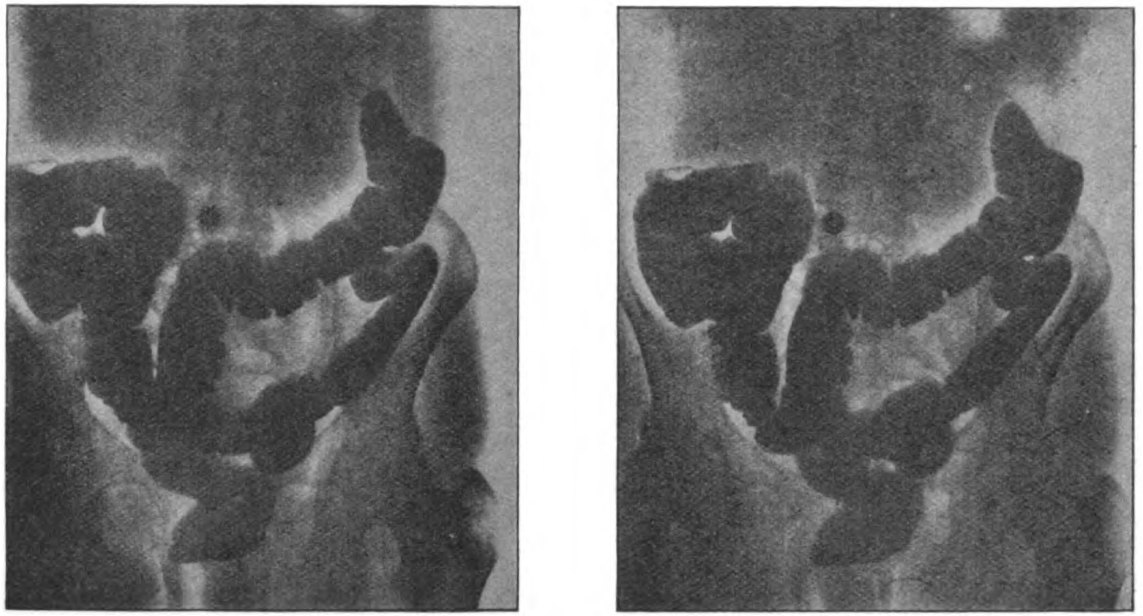
The anode-plate distance, for chest and abdominal work, is usually 65 cm.; that is, 35 cm. (the ordinary reading distance), plus the thickness of the body. The rule for determining the anode-plate distance in all our stereoradiography is to place the anode 35 cm. distant from the nearest point of the part radiographed. This anode-plate distance is carefully recorded. One must also carefully designate the right and left plates. I employ the automatic tube-shifting device of Snook, governed by a spring released by jerking a cord attached to the trigger. I prefer this arrangement rather than the use of a bi-anodal tube.

For changing the plate expeditiously I employ an automatic tunnel-plate changer, also controlled by a spring and trigger device, attached to the same cord which shifts the tube. In many cases I employ a rotary plate changer devised by Snook. Both give excellent results. By means of an automatic device, which at my suggestion the Polyphos Company of Munich have built into our Röntgen cinematographic installation, it is possible to still further reduce the time required for plate-changing. By this modification the apparatus of Rosenthal can be used either for cinematographic or stereoradiographic work. Lange, of Cincinnati, has also devised a special plate-changer for stereoradiography of the chest.

The stereoscopic effect may be enhanced in several ways, especially by the use of a small lead marker at the umbilicus and by the employment of sufficiently large plates to include points of comparison, to which the stereoscopic effect is due. For instance, in stomach work one is tempted, for the purpose of economizing, to use 10 by 12 plates instead of 11 by 14. It will be found that the improved stereoscopic effect of the larger-sized plates more than makes up for the extra expense. The presence of gas collections in the abdomen is really an advantage by still further improving the impression of perspective. The injection of air

FIG. 1.

Colon after bismuth clysma. Plate anterior, patient standing. The patient gave a history of chronic pain in the right inguinal region, with obstipation. There was fixation of the cæcum and the first part of the transverse colon. This case illustrates the value of the stereoradiogram in studying the relations and morphology of the individual segments of the bowel. For this purpose the clysma is better than bismuth administered by mouth.



into the bowel is a simple means of improving the plasticity of the effect. In the same way, the introduction of oxygen into the bladder, or even into the renal pelvis, aids in giving the conception of depth to the picture.

In viewing the stereoradiographs I prefer the mirror stereoscope of Wheatstone. The picture formed by the blending of the two images is more nearly life-size and life-like than with any available form of prism or lens stereoscope. In several of the London laboratories I have seen a type of the Wheatstone stereoscope which appears to be ideal. It permits one to view several pairs of plates in rapid succession, and each plate is adjustable separately in every possible direction. One must insist upon maintaining the eye-plate distance the same as the anode-plate distance—that is, the eyes must see what the tubes "saw," so to speak. Hence the necessity for recording the anode-plate distance, especially if any mathematical calculations are required.

The administration of bismuth oxychloride meals is carried out in the usual manner. For the clysma, I employ a gummy suspension of 90 grm. of bismuth in a litre and a half of warm water.[1] I always insist that the bowels should be emptied by a cleansing enema an hour or two before the examination. The clysma is used only when studying the morphology and relations of the colon; for the study of intestinal motility the bismuth meal is employed. In the majority of cases, with the patient lying flat on the back (hips not elevated), a litre of the mixture suffices to reach the cæcum within two to four minutes; another half-litre makes the bowel shadow complete. I use an ordinary enema outfit, the receptacle being held a metre above the patient, and the tube being inserted into the rectum not more than 4 to 6 cm. The injection is always done under radioscopic control, as advised by Haenisch and Pfahler, and much valuable information is gained in this way.

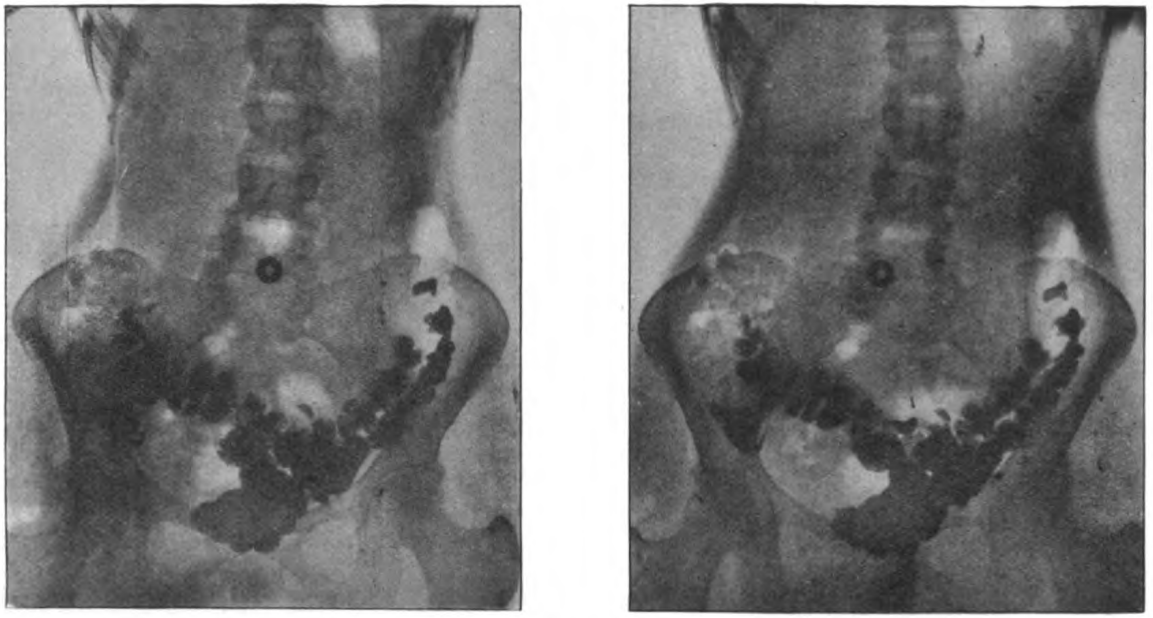
Objections.

The only objections which have been advanced against stereoscopic work are the alleged trouble and expense. The trouble is minimal with the shortened exposures and improved automatic apparatus of to-day. The expense is not so great as might be inferred from first glance. In

[1] To 2½ dr. of gum tragacanth add about 1 oz. of alcohol. Shake well. Add 20 oz. of warm water, and shake. Add 3 oz. of bismuth subcarbonate or barium sulphate, then 20 oz. of water, shaking well each time. This mixture should be made up fresh shortly before using.

FIG. 2.

Colon, on third day after a bismuth meal. Note that although the bulk of bismuth has passed into the transverse colon or further, there is still a "rest" in the cæcum and appendix. Two days later this "rest" still showed in the appendix and cæcum, though the remainder of the bowel was entirely emptied of bismuth. The cæcum was fixed and tender to pressure, and the cæcal region the seat of chronic pain. During the preceding year the patient had several attacks diagnosed "recurrent appendicitis." Operation one week after the last Röntgen examination showed the appendix buried in adhesions, still containing bismuth, and the cæcum fixed.



our laboratory the chief item of expense is not the plate account. Furthermore, there are several ways in which expenses may be diminished. (*a*) Previous screen examination should be practised to make sure that the stomach or bowel is properly filled and that there is a likelihood that the stereo-plates will be helpful. (*b*) Barium sulphate may be substituted for the bismuth preparations, reducing this item of expense about thirty times. The barium sulphate may be used without fear, both for test meals and clysmata. (*c*) The use of the intensifying screen so greatly reduces the time of exposures that one may use the slower, less expensive grades of plates, and still do sufficiently rapid work. In our laboratory the expense of the two plates is now only about 30 per cent. more than the single plate had previously cost us. (*d*) In many instances two plates are made in any case: why not stereoscopically?

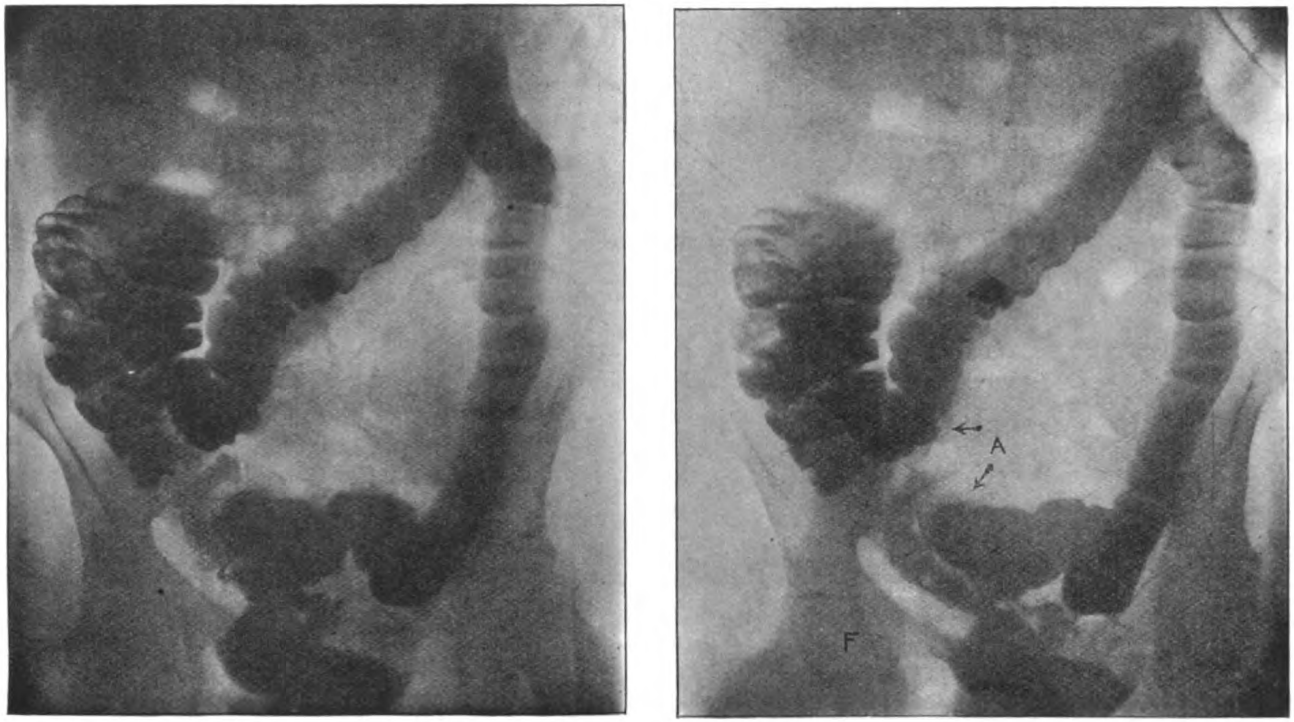
Another objection advanced against stereoscopic work is that many people cannot see stereoscopically. In our experience this difficulty is not encountered nearly so often as the objectors would have us believe; and in those cases where difficulty is experienced in fusing the images upon the retina, a little help in the way of suggestions, starting with some simple plates, as of the hand or foot, in the great majority of instances soon ends in success. With the mirror stereoscope these troubles are less frequently encountered.

Advantages.

In considering the advantages of this method we must recall that the ordinary Röntgen picture consists of a mere record of differences in density and distance, resulting in distorting and overlapping shadows. In stereoradiograms, on the contrary, made with proper technique and properly viewed, this distortion of images or shadows is entirely corrected. In truth the ordinary Röntgen plate cannot be called a picture; it is a mere flat record of shadows. But in the stereoradiograph an altogether new element appears. Instead of a mere flat record of shadows of varying degrees of density, the meaning and significance of which must often be more or less a matter of inference, subject to a degree of obscurity and uncertainty, one sees solidity and perspective —the semblance of *a real picture*. The body becomes, instead of an opaque object, a mass of crystal in which are embedded the several densities in the range of vision, each seen as a semi-transparent object in its proper plane, its relative size and position, without distortion or displacement.

FIG. 3.

Colon filled by bismuth clysma; fistulous tract between right inguinal region and sigmoid filled with Beck's paste, a considerable quantity of which had passed into the sigmoid (note denser shadow). The cæcum, part of the transverse colon, and the sigmoid extensively adherent. Patient had an appendicectomy two years before, followed by prolonged suppuration and fistula (*see* fig. 4).



One seems not merely to be looking *at* the object, but *into* it. The heart, the aorta, the bifurcation of the bronchi and the lace-like ramifications of the hilus shadow (seen in very rapid plates to the very periphery of the lung), the arch of the diaphragm, and in the abdomen the bismuth-filled stomach, colon and numerous other details, stand out as masses forming a marvellously striking and fascinating picture. In the study of the colon, particularly at the flexures and in the sigmoid region, and in the terminal ileum, the stereoscopic conception is most helpful. Diagnoses from ordinary plates, which might prove to be serious errors, are often corrected, apparent kinks often being shown to be well-rounded loops. Of course, the perfection of the picture varies with the different conditions which necessarily modify the definition of Röntgen plates.

One of the most striking and surprising features of the stereoradiograph, and one of its most essential advantages, is the fact that it gives a picture from both sides of the object examined. By simple reversal of the plates in the stereoscope, the point of view is transferred to the opposite side of the object. Thus, if the exposures are made with the tube anterior, one may obtain not only a true stereoscopic antero-posterior view but also an almost equally valuable and informing pseudo-stereoscopic postero-anterior view. It is thus possible to determine with exactness the location and position of any object visible in the plate in relation to any other object.

The limits of this paper will not permit a discussion of the advantages of the stereoradiographic method in the study of the pathology of the chest. This subject has been fully and most ably presented in the published papers of Walsham, Leonard, Emil Beck, Wenckebach, Kennon, Dunham, Hulst, and others. The experience of our laboratory confirms the position strongly maintained by these writers—namely, that stereoradiographic plates afford more reliable data upon which to base a negative diagnosis than the ordinary plate, even when supplemented by a fluoroscopic examination. This is also equally true in relation to the early diagnosis of pulmonary tuberculosis. Leonard's studies on the displacements of the intrathoracic viscera in pulmonary tuberculosis would have been impossible without the aid of stereoradiograms. Dr. J. H. Selby, in charge of the Röntgen Laboratory at the Mayo Clinic, told me last September that they had made over 1,900 stereoradiograms of the chest in the preceding twelve months.

The examination of a few stereoradiograms of the alimentary tract will at once suggest numerous conditions in which the method must



FIG. 4.

Same case as fig. 3. Note the ease with which the fistulous tract can be followed from skin to bowel. Observe the subcutaneous blind pouch branching off toward the suprapubic region.

render invaluable service. Among these may be especially mentioned: The course of the œsophagus and its relation to the aorta and the heart; the topography of the lower portion of the œsophagus and the cardiac orifice of the stomach; the contour of the diaphragm; the relation of the spleen and the gas- or bismuth-filled left flexure of the colon to the stomach; diaphragmatic hernia; subphrenic abscess, especially in its early stages; the form, position, and relations of the pyloric end of the stomach and the bulbus duodeni; the situation and relations of penetrating ulcers, especially when on the posterior wall of the stomach; the course and contour of the duodenum; the seat of obstruction in the small or large intestines, with special reference to kinks and adhesions in the terminal portions of the ileum; the course of fistulous tracts, either between contiguous portions of the stomach or intestine, or between the skin-surface and the bowel. In the study of the urinary tract, especially after collargol injection, stereoradiograms give the greatest possible amount of information.

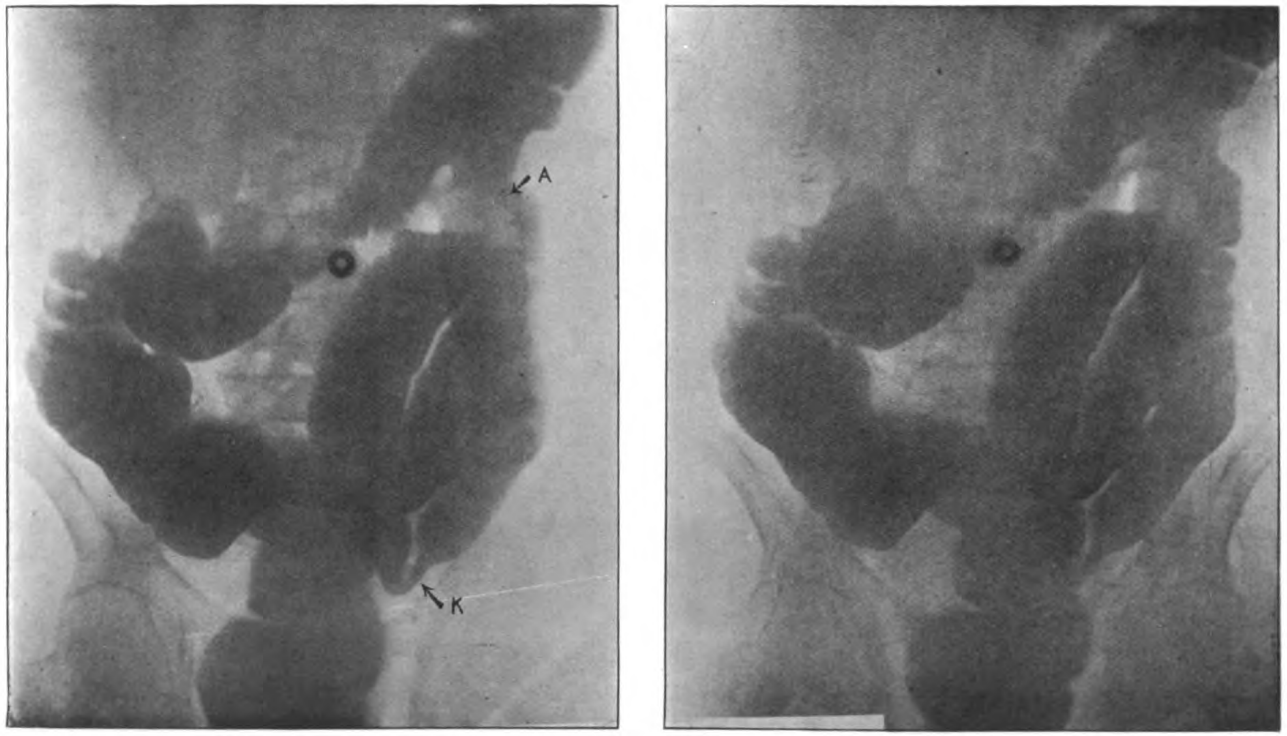
As a means of conveying to the clinician to whom we are under obligations for the privilege of examining these patients a satisfactory conception of the Röntgen findings, there is nothing superior to a pair of stereoradiograms to supplement the written reports, and when a patient must be impressed with a clear understanding of his condition the stereoradiograms are most helpful.

In conclusion, I wish to make it clear that in my presentation of the advantages of stereo-Röntgen pictures in the study of morbid conditions of the alimentary canal, I do not suggest this as a method which should take the place of the Röntgen screen examination; for it is evident that the stereoradiogram can only give a picture—a greatly improved picture, we may say—of the static condition of the parts examined, while the screen image gives a picture of the living organism in action. It is the invariable rule of our laboratory to make the screen examination in all cases of thoracic or abdominal disease, but supplementing it by the stereoradiographic plates where there is any likelihood that they will furnish additional information. The surprising thing, however, is that the stereoradiograms often furnish valuable additional information when it is least expected.

Now that the cinematograph has reached a considerable degree of perfection in its application to Röntgenography, the greatest desideratum that still remains is perfection and simplification of means for the application of the stereoscopic principle to fluoroscopic examination. This was recognized more than twelve years ago by Sir

FIG. 5.

Colon after bismuth clysma. Plate anterior, patient standing. Note the very obvious constriction (kinking?) in the pelvic colon, and the unusually long sigmoid loop reaching considerably above the umbilicus, even though the patient is standing. The highest point of the loop was fixed, as was also the region of the "kink." No amount of manipulation or change of position altered the picture; neither did atropine have any effect. Bismuth given by mouth was delayed above the constriction; the upward progress of the clysma was also delayed here. The examination was ordered to determine, if possible, the cause of the patient's obstipation.



James Mackenzie Davidson [4], who devised an apparatus for this purpose, and Reiniger, Gebbart and Schall [18] in 1902 announced a stereoscopic fluoroscope; but for practical purposes neither of these appliances came into general use. Howard Pirie [17] has recently revived interest in this method by his demonstration at the last meeting of the British Medical Association, and it is to be hoped that in the near future more general use may be made of stereoscopy for all kinds of examinations relating to the diagnostics of internal medicine.

REFERENCES.

[1] Beck. *Fortschr. a. d. Geb. d. Röntgenstr.*, Hamburg, 1910, xv, p. 303.
[2] Caldwell. *Trans. Amer. Röntgen-ray Soc.*, 1910.
[3] Davidson, Mackenzie. *Brit. Med. Journ.* 1898, i, pp., 10-13, and 1898, ii, p. 1669; *Lancet*, 1899, i, pp. 163, 304; The Röntgen Society, Lond., December 7, 1899; *Lancet*, 1899, i, p. 1447; 1900, ii, p. 496.
[4] *Idem.* The Röntgen Society, London, December 6, 1900.
[5] Dunham. *Trans. Amer. Röntgen-ray Soc.*, 1911.
[6] Eijkman. *Fortschr. a. d. Geb. d. Röntgenstr.*, 1908-9, xiii, pp. 355, 382; *Arch. of the Röntgen Ray*, 1909-10, xiv, No. 1.
[7] Hickey. *Trans. Amer. Röntgen-ray Soc.*, September 23, 1910.
[8] Hildebrand. Aerztlicher Verein, Hamburg, May 8, 1900 (ref. *Münch. med. Wochenschr.*, 1900, xlvii, p. 750); *Fortschr. a. d. Geb. d. Röntgenstr.*, 1899, iii, p. 171; *Zentralbl. f. Chir.*, Leipz., 1900, xxiv, p. 609, &c.
[9] Hulst. *Trans. Amer. Röntgen-ray Soc.*, 1910.
[10] Leonard, Charles Lester. International Congress of Radiology and Electricity, Brussels, 1910.
[11] *Idem. Arch. of the Röntgen Ray*, 1910-11, xv, p. 246; *Journ. de Radiol.* (belge), April 15, 1911.
[12] Levy-Dorn. *Verhandl. d. deutsch. Gesellsch. f. Chir.*, Berl., 1897, xxvi (i), p. 50.
[13] Mach. Goch's "Handbuch der Röntgenlehre" (F. Enke), Stuttg., 1903.
[14] Marie et Ribaut. *Arch. d'electr. méd. expér. et cliniques*, Bordeaux et Par., 1899, ix, p. 15; *La Radiographie*, 1900, No. 32.
[15] *Idem. Arch. d'electr. méd. expér. et cliniques*, Bordeaux, July 15, 1900.
[16] Morgan. *Arch. of the Röntgen Ray*, 1907, xii, p. 195.
[17] Pirie, Howard. *Brit. Med. Journ.*, 1911, ii, p. 916.
[18] Reiniger, Gebbart und Schall. *Fortschr. a. d. Geb. d. Röntgenstr.*, 1901, v, p. 197.
[19] Selby. *Trans. Amer. Röntgen-ray Soc.*, 1911.
[20] Walsham, H. "Stereoscopic Skiagraphy in Diseases of the Chest," 1899.
[21] Walter. *Fortschr. a. d. Geb. d. Röntgenstr.*, 1902, vi, p. 68; &c.
[22] Wenckebach. *Internat. Kong. f. med. Elektrologie u. Röntgen-Kunde*, Amster., 1908.

The President (Mr. A. D. Reid) said he felt sure he was expressing the feelings of the entire meeting when he tendered a hearty vote of thanks to Dr. Case for his admirable exposition of the beautiful photographs he had shown. They aroused feelings of jealousy, and he did not think it possible to thank him enough for demonstrating them, especially as he had delayed his passage to America in order to exhibit them to the Section that evening. By the time he came back to England next year—which it was the general hope that he would do—he trusted the English members would have something as good to show.

Electro-Therapeutical Section.

February 16, 1912.

Mr. A. D. Reid, President of the Section, in the Chair.

Secondary X-radiations: Their Uses and Possibilities in Medicine.

By Francis Hernaman-Johnson, M.D.

Secondary X-radiation, considered as a possible therapeutic agent, has been before the medical profession for a period of less than two years. It is as yet impossible to say what parts of the subject may or may not prove of importance to the physician; I therefore ask your pardon in advance for any crudity of arrangement or unnecessary verbiage of which I may prove guilty in the course of this paper.

It is commonly taught that ether-pulses of the nature of Röntgen rays and the *gamma*-rays of radium are caused by some alteration in the velocity or direction of a rapidly moving electron. In the Crookes tube an abrupt stoppage is brought about by focusing the cathode stream on to a platinum target. The cathodal rays, which consist of negatively charged corpuscles, are believed to correspond with the *beta* particles emitted by radium; and it is supposed that an acceleration of these latter, accomplished by some means at present unknown, is responsible for the *gamma* radiation of radio-active substances. Non-particulate rays of X-type may, however, be produced when Röntgen rays are allowed to play on certain elements: in which case the ether-pulse is apparently not produced in the manner above described. The X-radiation is not the only one generated under such circumstances; actual particles are given off from the element during excitation, and these are found to be electrons bearing negative charges; that is to say, they are comparable, in everything save velocity, with the *beta* corpuscles of radium and its allies. This is very interesting philosophically, as it means that the element behaves, for the time being, as if

it belonged to the radium group. It actually throws off a part of its substance—in other words, undergoes atomic disintegration—and must, in time, become changed into some other form of matter.

If X-ray treatment as at present practised were without disadvantages, the subject of secondary radiation might be of purely academic interest to medical men. Unfortunately, radio-therapeutics is as yet far from perfect. Each tube is a law unto itself. The primary rays are heterogeneous, and when a good result follows their application, it is impossible for anyone but the original operator to reproduce the exact conditions under which it was obtained. Constant care is necessary to maintain the vacuum of a tube at one point, and it is not feasible to treat the skin without producing at least some effect on the deeper tissues. Above all, the treatment of deep-seated disease is most unsatisfactory. It is quite easy to generate rays which will reach the lesion, but such rays are comparatively feeble as regards therapeutic activity. This may be due to the fact that the more penetrating type of ray has a tendency to be scattered rather than absorbed by substances of low atomic weight, such as the tissues of the human body. Soft rays, it is true, are readily absorbed by living matter; and we assume that where there is absorption there is physical, chemical, or electrical change. But the soft rays cannot reach deep-lying tissues; and thus there is created a sort of therapeutic *impasse*. In secondary radiation we have an agent which, employed either as an aid to ordinary X-ray treatment or as a substitute for it, promises to afford a means of escape from many difficulties.

As regards their behaviour under Röntgen stimulation, substances vary according to their atomic weight. Elements below calcium (40) merely scatter the incident beam. In the case of those beyond cerium (140) the corpuscular radiation is alone of much importance. These corpuscular rays possess a penetrative power, which depends solely upon the hardness of the exciting beam: a bulb of a given degree of vacuum will produce particulate rays of the same penetration whether the substance acted on be zinc or lead. Elements occupying a place in the periodic system between calcium and cerium give off, in addition to corpuscular rays, a powerful non-particulate or X-radiation. This latter is of extreme importance, for it bears a definite and constant relationship to the atomic weight of the emitting substance. As the atomic weight increases so does the hardness of the secondary X-radiation produced. For each element there is a homogeneous radiation, the penetrative power of which remains the same whatever be the quality

of the exciting beam. From the point of view of the electro-therapeutist this characteristic radiation so far transcends in importance all others,[1] that I have ventured to apply to it the adjective *specific*. Sometimes it is convenient to speak shortly of the "silver ray," or the "copper ray"; and by this is meant the specific secondary X-radiation of these metals.

Professor Barkla has worked out the details for many substances, taking as his standard the thickness of water through which the specific ray will pass before falling to half value. For iron it is but $\frac{1}{25}$ cm. (0·04); for silver, nearly $1\frac{1}{2}$ cm.; while the specific ray of cerium will pass through no less than 6 cm. of water before losing 50 per cent. of its initial energy. It should be noted that the incident beam must be harder than the specific ray of a given element, or this characteristic radiation will not be excited. There is little doubt but that metals beyond cerium could be induced to yield specific rays, did we possess tubes hard enough to excite them. Specific radiation is solely an *atomic* property. It is of no consequence whether an element is a metal or a non-metal, nor does chemical combination make any difference. Moreover, Sir J. J. Thomson informs me, in a private communication, that a substance in solution is still capable of being excited.

The suitability of an element for a particular medical purpose is naturally determined by the bulk of tissue which it is required to affect. What is suitable to use for a skin lesion 1 mm. in depth would not suffice to influence a cancerous mass having a thickness ten times as great. In referring to ray-emitting substances it is convenient to speak of their *effective therapeutic range*. This must necessarily be determined finally by observation, but I have taken as a working basis the distance traversed (in the tissues) which reduces the original value of the specific radiation by one-third. Thus, the Effective Therapeutic Range (E.T.R.) of Zinc is approximately half a millimetre, while that of silver is nearly twenty times as great.

All possible ways of applying secondary radiations in therapeutics are variants of one or other of two principal methods. Either the radiation is employed to enhance the effect of the primary beam, or is used as a substitute therefor. The first method I have termed "Treatment by Intensification"; the second, "Treatment by Pure Secondaries."

[1] The corpuscular radiation has already been referred to. Some of the heavier elements produce also some feeble X-radiations, but they are not likely to prove of importance medically.

(I) Treatment by Intensification.

If a thin sheet of a suitable metal—let us say, zinc—is exposed to a Röntgen tube, secondary rays are produced on both sides of the plate. The radiation takes place chiefly in a direction opposite to that of the exciting beam, but an appreciable amount leaves the plate in company with the primary rays. Secondary rays are also given off in all directions within the substance of the metal, but are not available for use. If, however, we scatter the zinc in a finely divided form throughout a mass of tissue, so that a certain amount of protoplasm intervenes between each particle, then these universally distributed rays may become of value for therapeutic purposes. In treatment by means of an intensifier we utilize secondary rays belonging to one or other of the three directional types above mentioned. That is to say, the ray-emitting substance may be placed *behind* the lesion, or *in front* of it; or again, by injecting solutions, we may distribute the molecules of the intensifier *throughout* a mass of diseased tissue. For convenience in description it is necessary to apply technical terms to these several methods. When recording microscopic observations we say that an object has been viewed by "reflected" light or by "transmitted" light. These terms do not, of course, indicate any difference of quality in the light used, but refer solely to the manner of illumination. Corresponding expressions are required for our present purpose. There has been some tendency to use the word "reflected" in connexion with secondary rays; this optical term tends, however, to confusion of thought, as it suggests an analogy which does not exist. I have, therefore, thought it desirable to invent certain new technical terms. When a lesion is treated by secondaries acting in a direction *opposite* to that of the primary beam, it is said to be subjected to *anadrastic*[1] rays. If the radiation used leaves the intensifier together with the primaries, proceeding in the same direction, the term *syndrastic* is applied. Lastly, when the particles or molecules of an intensifier are scattered throughout a mass of diseased tissue, the rays produced, as they act therapeutically in all directions, are said to be *pandrastic* in type.

I have submitted these terms to several classical scholars, who have all had some criticisms to offer. None of them, however, has been able to suggest any new words that would better convey the desired meaning. My creations may, perhaps, serve the purpose, and have at least the merit of pure Greek ancestry.

[1] ἀνα = backwards; δρεστικός = active. The other prefixes are: συν = together with; and πάν, from πᾶς = all.

(*a*) *Anadrastic Rays.*

The use of anadrastic secondary rays is at once the most obvious and the most widely applicable method of intensifying the therapeutic effect of the primary beam. It is especially indicated in the case of lesions having an extensive surface area, but small thickness. Such lesions are represented typically by *ulcers*. The intensifier should be closely applied to the denuded surface, or injected immediately beneath it in the form of an insoluble powder. Provided a substance with a suitable effective therapeutic range be chosen, a practically uniform secondary effect can be obtained throughout the diseased tissues. Silver, which has an E.T.R. of about 1 cm., is the most suitable element in the majority of cases. An ulcer of the anterior nares, backed by a silver plate, and rayed from outside by a hard tube, illustrates this treatment in its simplest form. In such a case it is possible to obtain a good deal of information as to what is happening by means of photographic experiments, some of which I purpose describing to you. For X-ray measurements the physicist relies on specially constructed electroscopes, and is inclined to look askance at photographic methods. For medical purposes, however, we require only approximate values, and in some instances it is possible to imitate the conditions of treatment more closely by photographic experiment than by observations on the electroscopic leak. I began to investigate the subject of secondary radiation by photographic methods shortly after the publication of Sir J. J. Thomson's paper in the autumn of 1909. Since then I have developed and compared between seven and eight hundred negatives, and think I may claim that my results, whether or not they are of much value, are at least approximately accurate so far as they go.

The object of these experiments has for the most part been to determine the value (photographic) of various secondary rays in terms of the primary. In most cases only the specific radiation has been allowed to act on the film. The following is a typical experiment: A plate, film upwards, and enclosed in a light-proof bag, is supported on a suitable stand. A strip of silver is laid across one end. Beneath is placed the X-ray tube, and a resistance is interposed corresponding to that of the tissue traversed in the particular case which is being investigated. After exposure and development the plate will be found generally fogged, but an image of the silver strip is plainly visible. This additional blackening is due to the specific secondary radiation of the metal, and might be called an anadrastic radiograph. For

brevity, one may speak of such images simply as *anagraphs*. The process is now repeated, but with some additions. After having taken an anagraph of the silver we cover up this part of the film with lead, and subject successive sections of the remainder to exposures increasing by a definite increment. The plate is developed, dried, and the sectors cut with a diamond. The several strips are now compared side by side with the anagraph, until one is found of the same density (fig. 1). The average of a number of experiments shows that exposure to the primary rays must be increased by from 20 to 30 per cent. (according to their hardness) before this is accomplished. The interpretation of these results requires care. They do not by any means prove that more than a quarter of the energy of the primary beam is returned as secondary radiation from the surface of the metal. They do indicate, however—

(1) That the *photographic effect* is enhanced by from 20 to 30 per cent.

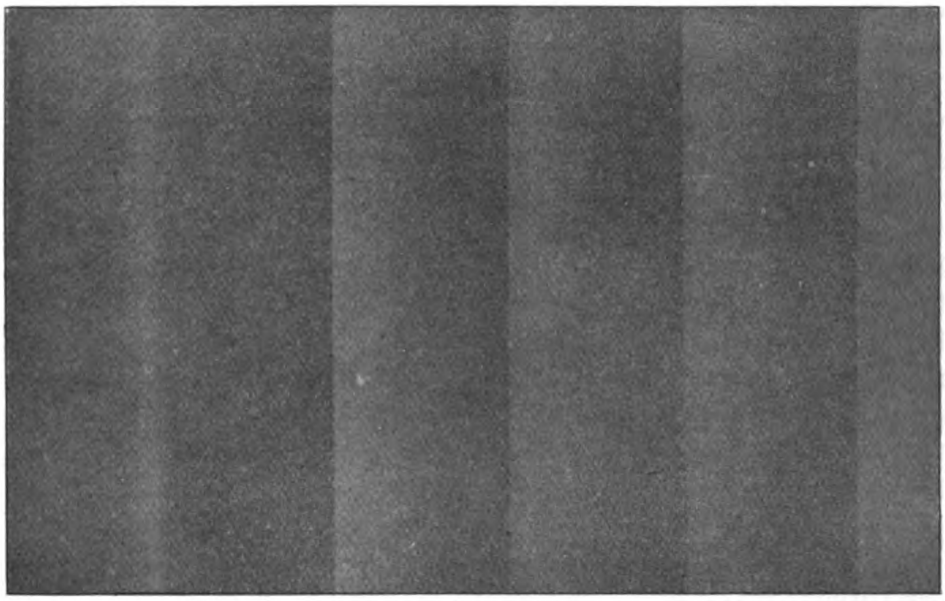
(2) That the physiological potency is intensified by an amount which is certainly not less than this, and is in all probability vastly greater.

The reason for this latter statement is as follows: the sensitive film contains bromine and silver, two heavy elements much more capable of absorbing hard rays than are the light atoms of the body. When two separate radiations, the one soft the other hard, are found to produce an equal effect on a plate in a given time, it may be safely assumed that the soft rays, *within the limits of their effective therapeutic range*, will produce a greater disturbance in the tissues than those of the more penetrating type.

By means of anadrastic rays it is possible to obtain a greater physiological action on deep-lying tissues than on the skin itself; thus enabling us to avoid, on the one hand the Scylla of dermatitis, and on the other the Charybdis of inefficient treatment. Suppose we use a hard primary beam which has been rendered fairly homogeneous by means of an aluminium filter; and assume further that this beam still retains four-fifths of its initial energy after traversing 2 cm. of tissue. If now an intensifier be injected at this depth, the effect is increased by at least one-third of four-fifths, and the sum of the primary fraction *plus* the secondary increase is greater than unity. All this sounds so like a proposition in Euclid that I feel I ought to add, "Q.E.D."

The use of metal plates as intensifiers is possible in a few sites only, notably in the nose, mouth, and urethra. For the last mentioned a

silver catheter may be employed. In many cases one has to resort to the injection of precipitated silver in suspension in some viscid fluid, or of collargol, which is a pseudo-solution of the metal. Anagraphs of these substances may be obtained, provided that care be taken to protect the plate from moisture, and their efficiency is found to be but little less than that of sheet silver. Genuine solutions, such as that of silver nitrate, are quite unsuitable for the purpose of treatment by anadrastic



(1) (2) (3) (4) (5) (6)

FIG. 1.

Print from negative illustrating method of determining relationship of secondary to primary rays. (1) anagraph of silver strip; (2) section of plate exposed for same time as (1), without silver backing; (6) section exposed for 30 per cent. longer time than (2)—this corresponds most nearly with (1). We therefore conclude that the anadrastic rays from the metal have intensified the primaries by about one-third.

Note.—A good deal of the fine shading is lost in reproduction, and can be properly appreciated, even in the original negative, only by cutting out the several strips, placing them successively side by side with the anagraph, and viewing by transmitted light.

rays. The injections referred to are of use in the case of rodent or other obstinate ulceration of the skin. An ulcer which is at a standstill under ordinary radio-therapy may sometimes be stimulated to heal by the introduction of precipitated silver beneath its base. The same substance,

preferably in olive oil, may be injected into the rectum. If it is desired to treat the mucous membrane of the bladder, its gradual impregnation by metallic particles may be secured by means of collargol, which would probably also be suitable for intra-uterine use.

The application of anadrastic secondary rays to lesions of the alimentary tract can be carried out by the employment of "silver meals." The technique of the method I described at Birmingham in July last, and I need not recapitulate what was then said. During the past six months, however, I have made further investigations along similar lines. I will therefore set forth, as concisely as possible, my more recent conclusions, and also take the opportunity of dealing with certain criticisms passed at the British Medical Association meeting, together with others which have since been made to me privately.

In the first place, it has been suggested that a risk of argyria is incurred. Now I would like to emphasize that what is used is a chemically pure preparation of *metallic* silver in precipitated form. It is, of course, absolutely insoluble, and unless it can be materially changed in the stomach and bowels must be incapable of absorption in any degree whatever. I have administered the preparation in all to a dozen persons, not counting myself, over prolonged periods of time. Despite careful observations, I have never seen even that slight greying of the skin which precedes argyria, and gives due warning of its approach. One patient has taken in all 500 grm., spread over six months, without any suggestion of evil result. By diluting the fæces of such patients with water, and stirring, a large part of each dose may be recovered unchanged, as the heavy metallic powder rapidly sinks. A minute portion probably adheres to ulcerated surfaces; apart from this, I have no doubt but that the whole amount administered could be recovered by appropriate methods. In the case of one nervous patient, I advised the taking of a saline purgative the day after the silver meal, and this put her mind quite at rest. Whatever disadvantages the treatment may have, the risk of argyria is not one of them.

In order to get the best results from X-ray treatment of external ulcers it is necessary to keep their surfaces clean. A silver breakfast, sweeping as a massive bolus through the digestive tract, may very possibly remove débris in its passage, and thus aid us mechanically by cleansing ulcerated patches of mucosa.

Again, I have been told that the silver must necessarily distribute itself over an area much wider than that of the actual disease, with consequent danger to healthy mucous membrane. But if experience

gained in treating external lesions is of any value in this connexion, such an extension of the irradiated zone is to be looked on rather as an advantage than the reverse. Personally, if I am treating a skin ulcer, I prefer to expose at the same time a considerable portion of the surrounding integument, as I find that better results accrue than if the rays be strictly limited to the diseased surface.

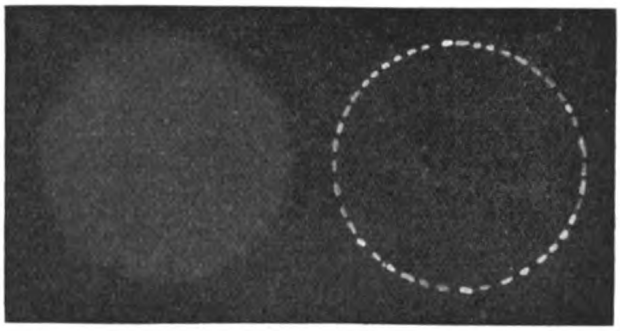
From the foregoing it is but a step to the consideration of possible injury to the alimentary mucosa during bismuth examinations. A note of alarm was sounded in the *British Medical Journal* of November 12, 1910, by no less an authority than Professor Barkla. The utterances of Professor Barkla constitute the last word on the subject of secondary radiation, so far as its purely physical aspects are concerned; but the question referred to demands for its investigation some knowledge both of medicine and of physics. It is, in short, essentially a problem for the radiologist.

The heavier metals, such as bismuth and lead, cannot, as we know, be excited to yield a specific ray comparable with that of such elements as silver or zinc. They do, nevertheless, give off an abundant corpuscular radiation, and some feeble rays of the ether-pulse type. A very dense anagraph of bismuth powder may be obtained, provided it be allowed to act directly on the sensitive film; but if a thin sheet of paper be interposed we get practically no image at all. Now it is true that between a bismuth meal and the lining of the bowel there is no sheet of paper; but there *is* a very considerable layer of viscid mucus. In order to illustrate what takes place during a Röntgen diagnosis by the aid of bismuth, I have devised what I believe to be a crucial experiment. Two pill-boxes are taken, one containing dry bismuth carbonate, the other an equal quantity of the salt made into a soft paste by means of saliva. This latter may be assumed to represent the condition of a bismuth meal in the stomach. Anagraphs are now taken of the two boxes, side by side on the same plate.[1] On development, no record will be found of the moistened salt. From this we may conclude that no secondary radiation capable of affecting a photographic plate reaches the walls of the patient's digestive tract (fig. 2).

Experience gained with metals which have an atomic weight near that of bismuth is of value in relation to the subject under discussion, and also has a bearing on a more general question—namely, the effects

[1] The boxes are filled almost to the top; the plate is laid on them, film downward; the tube is placed above.

of corpuscular rays on healthy skin and mucous membrane. Lead is a very suitable substance for use in such investigations, and I have performed several experiments with it; some deliberately, others, as I shall explain to you, without intention or understanding. Working at the matter from the side of photography we may cut out a lead letter, say an E, and allow a thin layer of gum to dry over one half of it. An anagraph will show only the uncovered half (fig. 3). This illustrates the extremely low penetrative power, not only of the corpuscular rays but of those feeble X-radiations which are at the same time generated. If we take another piece of lead and write on it in ink, which is allowed to dry into a film, a similar result may be obtained. We may also get a like effect from metals which have a specific ray, provided a tube



a *b*

FIG. 2.

Anagraphs of dry bismuth and bismuth moistened with saliva (in pill-boxes); *a*, dry salt; *b*, dotted line indicating position of box containing moistened salt. No photographic effect.

be used which is so soft that it excites only the less penetrating radiations.

An actual demonstration of physiological effect, or lack of it, must necessarily be of more value than any argument drawn from chemical analogy. Fortunately I am able to quote the results of two experiments actually performed on human beings, though for neither of them is any credit due to me. They were not intended as experiments, and it was not till long afterwards that I realized their significance. The first consisted in this: the patient, a delicate girl, aged 17, suffered from a chronic eczema of the palm of the right hand. This I treated by X-rays. I had always been taught that the skin of the back of the

hand is very sensitive to irradiation; I knew vaguely that there were such things as secondary rays, and I had it on text-book authority that a sheet of lead should be placed under an X-ray plate in order to prevent secondary effects from blurring the image. Therefore, I very carefully made the patient always rest her hand, palm upwards, on a *lead plate.* She received some thirty applications, from a tube of medium penetration, so that during each of them I suppose the dorsum of her hand must have been bombarded by corpuscular and other feeble secondary rays. The eczema was cured, but no damage whatever resulted to the back of the hand.

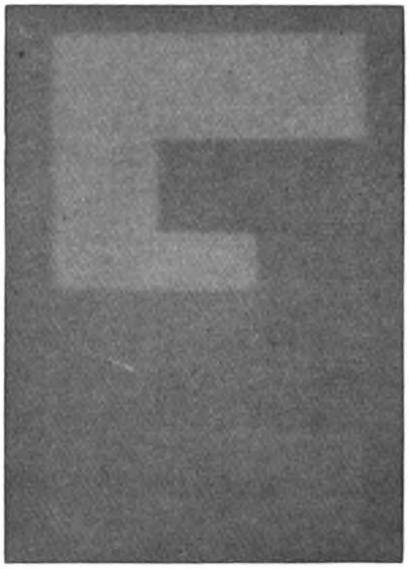


FIG. 3.

Anagraph of letter E in lead, one half of which was covered with a thin layer of dried gum. The latter part has produced practically no impression on the plate.

My second unwitting experiment was performed shortly after the publication of Sir J. J. Thomson's paper. In his address, doubtless because it was intended only as the merest sketch of the subject, Professor Thomson discussed only what I have since called the *specific* radiation of various elements; thus the fact that any secondary rays at all were emitted by lead remained still unknown to me. I was treating a depressed ulcer of the cheek, and wished to make it prominent. This I accomplished by inserting a plate of lead between the cheek and the closed jaws. I did not succeed in curing the ulcer, but, on the other

hand, my lead backing did no harm to the buccal mucosa. This failure to react was certainly not due to any lack of sensitiveness to suitable secondary rays, for in another somewhat similar case, which I have described elsewhere, great irritation was caused by a plate of silver, owing to failure on my part to reduce the primary dose so as to allow for the intensification.

This completes my reference to the supposed dangers of secondary radiation from bismuth meals. My apology for taking up so much time over the matter is the ever-growing importance of bismuth diagnosis. Where X-rays are concerned the lay public is very easily frightened, and I have even had doctors in my consulting-room who seemed to believe that incurable Röntgen dermatitis might be produced by a single radiographic exposure. Once let a false suggestion of danger get into text-books, and it will take a generation to overtake it. I have certainly proved to my own satisfaction, and I hope also to yours, that the bismuth examination is harmless. It is at least certain that the first case of injury has yet to be recorded.

To return to the therapeutic uses of silver meals. It is a reproach to the radiologist that he can do little or nothing for malignant tumours of the alimentary tract. In the case of external growths, he does at least relieve pain and prevent ulceration. This is already a great deal, for quite half the horrors of surface cancer are due to these two causes. Not so generally recognized is the fact that a parallel state of affairs exists in the case of neoplasms involving the walls of the bowel. Ulceration, fistulæ, hæmorrhage, obstruction, and foul-smelling alvine discharges render the patient's existence miserable beyond words. None of these things are necessarily the ultimate cause of death, which is not seldom due to distant metastasis. But metastasis is often long delayed, and, when it does occur, is swiftly fatal; whereas the evils I have mentioned appear early in the disease, and may plague the sufferer for many months, or even years. Therefore, if we could do as much to keep intact the mucosa of the gut as we can to preserve the skin of the body, internal cancer would be robbed of at least some of its worst features, even though life itself were not prolonged. I am of opinion that where ulceration and contraction of the bowel have not advanced very far, it is possible to check their further progress.

At Birmingham I mentioned a case of pyloric tumour, which was probably malignant. The patient, who refused operation, has just died with signs of secondary infection of the lungs. Perhaps he would have lived nearly as long if he had had no X-ray treatment. But there is

this to consider. When he came to me he was vomiting frequently, and in considerable pain. Under treatment the pain disappeared, the vomiting became of rare occurrence, and the man led an ordinary life till within two months of the end.

A second case was that of a patient suffering from cancer of the rectum. She was sent to Mr. J. W. Leech, of Newcastle-on-Tyne, who pronounced the growth to be inoperable. As she would not submit to a colotomy he referred her to me for X-ray treatment. The growth was situated high up, and I preferred to give silver by the mouth rather than to inject it. She has now taken from 5 to 8 grm. every other day for six months. At the time of her first coming to me she was passing blood and débris, and suffered from nocturnal diarrhœa, accompanied by great pain. Complete closure of the gut was prophesied within three months, and would, I believe, have occurred had nothing been done. Under treatment the hæmorrhage ceased, solid motions replaced the intestinal flux, sleep returned, and the tongue, which had been heavily furred, became clean. These good results I believe to be due solely to the healing of ulcers projecting into the lumen of the bowel, and I do not for a moment delude myself into believing that a genuine cure will be effected.

In the case of diffuse growths infiltrating the walls of the tract, it may sometimes be possible to influence the whole thickness of the malignant tissue; but the effective therapeutic range of the silver ray is only 1 cm. or less, and if a more penetrating radiation were employed it would probably be less active physiologically. Also, whatever substance is used to produce anadrastic rays necessarily remains in the cavity of the gut, and after 1 cm. the law of squares would rapidly reduce its value. Although I cannot hold out any hope that by this treatment we can *cure* persons suffering from internal cancer, yet I would most earnestly urge you to give the method a trial. I think you will find it possible to give such patients many months of fairly comfortable life.

(*b*) *Pandrastic Rays.*

I take this method next, as I have already entered upon the subject of malignant disease. Anadrastic and syndastric rays are quite unsuitable for treating large tumour masses; but such as can be reached from the surface of the body are amenable to pandrastic radiations. Some of the methods employed to produce these enable us to make use of the corpuscular rays, which account for a considerable portion of the total

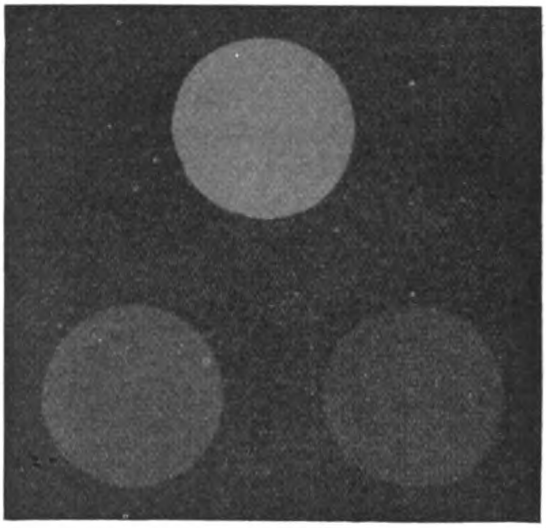
secondary output in any given case. Professor Barkla has determined that a layer of water $\frac{1}{100}$ mm. in thickness is sufficient to prevent the corpuscles from acting on an electroscope. Their E.T.R. is, therefore, probably less than 5 microns. Now, the diameter of some malignant cells, such as those of large round-celled sarcoma, may reach as much as 80 microns. A corpuscular ray, in order to produce any effect on the nucleus, must therefore be generated within the cell-body. This may be accomplished either by the injection of a diffusible salt containing elements of suitable atomic weight, or by driving in certain metallic ions by means of a galvanic current. In order to get the best results, a substance should be used which, in addition to producing a particulate radiation, emits a specific ray of some penetrating power. I have devoted a good deal of attention to this matter, using for injection solutions of ammonium bromide, strontium bromide, and strontium lactate.

Some information may be obtained from photographic experiments, the procedure being as follows: A beaker of thin glass is filled with distilled water to a depth of, say, 3 cm. This beaker is placed over a lead diaphragm, and an exposure made so as to produce a circular patch of medium density. The opening is closed, and another exposure is made on a different part of the plate; but this time strontium bromide in the proportion of 17 grm. to the litre is added to the water. By making various trials one finds that, using a tube having a penetration of 10 to 11 Wehnelt units, it requires about one-third longer to produce the same effect as when distilled water was used (fig. 4). Professor Barkla informs us that when an X-ray beam passes through substances of high atomic weight, such as strontium and bromine, at least 30 per cent. of its energy is re-emitted as secondary rays, which act in all directions upon the surrounding media. The application of all the foregoing is this: Absorption of X-rays by water may be taken to equal absorption by a tumour mass of similar thickness. Suppose the primary beam loses one-third of its energy in traversing 3 cm. of water, it cannot on this account be safely concluded that the lost fraction has been truly absorbed. Much of it is merely scattered. But if, after the addition of the strontium bromide, another third is found to have gone, part of this has certainly been converted into soft secondary rays. This fraction is not less than 30 per cent. Therefore at least one-tenth of the primary energy is so transformed; and this transformed tenth, owing to its peculiar properties, and its localized action, may be presumed to be of very great therapeutic value.

In practice one injects a solution such as the above (the osmotic pressure of which is slightly less than that of blood serum) by means of an antitoxin syringe armed with a long fine needle. A little adrenalin may be added to prevent too rapid diffusion; also eucaine, if necessary. The transformation of energy is somewhat less extensive than under experimental conditions, as the strontium solution becomes diluted by the body fluids.

The whole matter is not quite so simple as might at first sight appear. One cannot, I think, use strong solutions with safety, for it has been shown by Loeb that certain unfertilized ova may

(1)



(2) (3)

FIG. 4.

Exposures made (1) through 3 cm. distilled water; (2) through 1 per cent. collargol; (3) through solution of strontium bromide, 17 grm. to litre. The latter stops about 30 per cent. more rays than the distilled water (tube penetration = 10-11 Wehnelt).

be stimulated to segmentation by placing them for a brief period in sea-water artificially rendered hypertonic. The analogy between malignant cell proliferation and parthenogenetic development is very striking, and it is well to take no risks. Then there is the question of the diffusion of salts into living animal cells. Very few data are available, but it is certain that the rate of entry varies with different salts and different kinds of cells. For example, ammonium chloride

meets with far less resistance in the case of red blood corpuscles than does the corresponding sodium salt. How much strontium and bromine actually enter the tumour cells it is at present impossible to say. The intracellular deposition of metallic particles by ionization constitutes the one certain means by which we can, at will, generate corpuscular rays in the only situation where they can be of use. I am at present experimenting along these lines, using as electrolytes preparations containing silver and zinc. In future we may perhaps be able to take advantage of some work which is being done by Wassermann. He believes that selenium has some inhibitive action on cancer cells, and is perfecting a method of causing it to enter their nuclei, using eosin as a carrier. Selenium has an E.T.R. similar to bromine, and also gives off abundant corpuscular rays. The combination is, I understand, injected into the blood-stream, and is applicable to tumours in any organ or tissue. Thus, even should the method not fulfil the expectation of its discoverer, it may yet be of great service to the radiologist.

Solutions, or rather pseudo-solutions, of collargol may be used to produce pandrastic as well as anadrastic rays. A preparation having a strength of 1½ per cent. causes about the same extra absorption as an iso-osmotic solution of strontium bromide. The collargol is not diffusible, so that a stronger preparation may be introduced if desired. It should be our object, however, to allow at least one-half of the primary beam to emerge from the injected tissues, otherwise the deeper strata do not receive a fair share of secondary effect. The silver from collargol does not enter the cells, or, at any rate, does not diffuse into them; but after each injection a small deposit of metallic silver occurs in the intracellular spaces. The specific radiation is alone utilized.

I could describe cases of my own treated by pandrastic rays, but prefer to quote one which has been communicated to me privately. Dr. Meyrick Jones, of Charlton Kings, Cheltenham, has been very successful with a small-celled sarcoma of the antrum, occurring in a girl, aged 14. A 10 per cent. solution of bismuth and iron citrate was introduced immediately before raying. After about nine months' treatment (which was commenced late in 1910) the growth entirely disappeared, and has not recurred up to the present. This result is very gratifying, but it must be remembered that striking success has sometimes been obtained in similar cases by primary rays alone. As to the solution employed as an intensifier, I think that, on theoretical grounds, it is preferable to use elements possessed of a powerful specific radiation.

Bismuth has none, and that of iron has an E.T.R. of about $\frac{1}{40}$ cm. only, whereas the E.T.R. of bromine is $\frac{1}{8}$ cm., and that of strontium greater still.

Before leaving the subject of cancer it may be well to ask, Can nothing be done to prevent the occurrence of secondary deposits? At present our treatment of malignant disease is as futile in this respect as surgery itself. The late Sir Henry Butlin, in his recent lectures before the Royal College of Surgeons, pointed out that particular varieties of cancer seem to choose particular sites for metastasis, and seems to me that a careful study along these lines might enable us

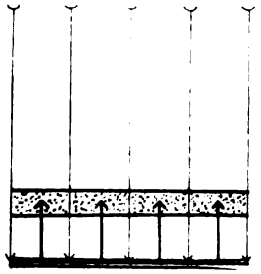


FIG. 5.

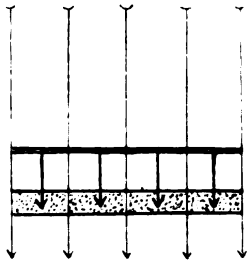


FIG. 6.

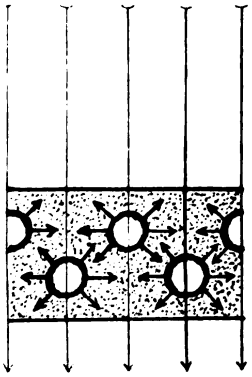


FIG. 7.

Diagrammatic representation of rays used in treatment by intensification. Primary rays are indicated by long arrows; secondaries by short arrows. Diseased tissue dotted. Fig. 5 shows *anadrastic* rays proceeding from an intensifier placed beneath the lesion. Fig. 6, *syndrastic* rays issuing from an intensifier placed above the lesion. Fig. 7, *pandrastic* rays proceeding in all directions from particles of an intensifier scattered throughout a mass of diseased tissue.

to do something to forestall this event. For example, tumours in the breast and stomach tend to form metastatic growths in the lungs, and the patient apparently dies from pleurisy. Dr. Leonard Finlay, speaking at the British Medical Association meeting in July last, showed that it was possible to infiltrate the pulmonary tissues with such a substance as iron carbonate, provided that it be inhaled in a finely divided form. A lung infiltrated with heavy metallic particles, and subsequently exposed to a hard tube, would be the seat of a soft secondary radiance of great therapeutic power; and it is scarcely to be believed that cancerous deposits could take root and flourish under circumstances so adverse to their well-being.

(c) *Syndrastic Rays.*

The analogy between the use of syndrastic rays in therapeutics and that of an intensifying screen in radiography is a very close one. The object of the latter procedure is to produce an enhanced effect on a thin film, ordinary light being many times more active, photographically, than X-rays. The fact that some energy is lost during the process of conversion is of no consequence. The rays emitted by such a screen are those of ordinary light, and act on the plate together with the beam from the exciting tube. That is, they are syndrastic rays. For therapeutic purposes we use, instead of a fluorescent salt, a substance such as zinc oxide, which gives a secondary radiation of X type. Both corpuscular and specific rays are emitted, and, if the oxide be not spread too thickly, an increased darkening of the photographic plate may be demonstrated. On the physiological side it is possible to produce an erythema of the skin in a shorter time than if primary rays alone are used—that is, unless the latter are of extreme softness.

Syndrastic rays are of value in the treatment of superficial skin lesions, enabling us to shorten our exposures and to be less dependent upon regulating devices for our tubes. The lighter elements are most suitable for this purpose, our object being to produce a powerful effect throughout a very thin layer of tissue. A tube of medium or low penetration should be used. This is in contrast to what is desirable in treatment by anadrastic rays, where one's object is, in most cases, to get a maximum of energy to the intensifier with a minimum effect on the skin. Where the intensifier is intended to produce a syndrastic radiation it may be applied as a powder or an ointment, but these methods are inexact. I would suggest that sheets of cloth be impregnated with various substances, a definite weight being deposited on each square centimetre. These should be pressed over the skin lesion concerned, and, in recording results, the observer must state the exact amount and distribution of the intensifier. This would enable others to repeat his technique with some approach to accuracy.

With the above I conclude my account of secondary ray therapeusis by intensification. It remains to consider—

(II) Treatment by Pure Secondaries.

Pure secondary X-radiations are rendered available for therapeutic purposes by means of an instrument which I have called a *Ray Transformer*. Any such apparatus must fulfil two conditions: it must

possess a transforming or converting plate, and some effective means of shielding the patient from the primary rays. My earliest instrument consisted of a lead-lined rectangular box, containing an X-ray tube at one end, and at the other a sloping plate made of sheet zinc. Beneath the zinc a circular opening permitted secondary rays to reach the skin. Great trouble occurred with sparking, and I soon modified the apparatus by removing all conducting material. The lead I replaced by X-ray proof cloth, and the zinc plate by a wooden one coated with zinc oxide.[1]

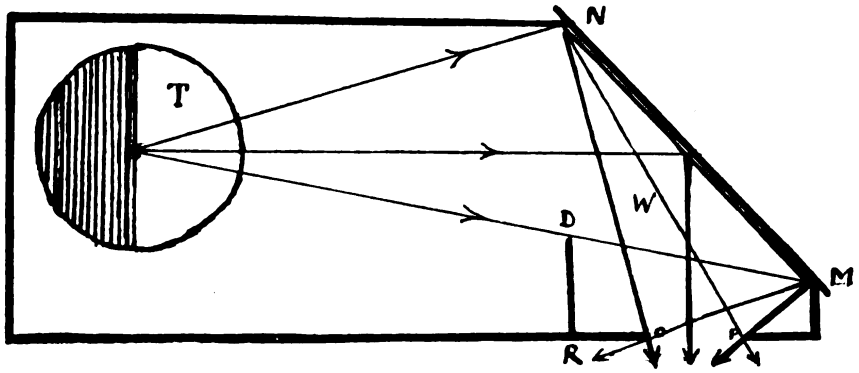


FIG. 8.

Diagram of ray transformer. T, X-ray tube; N M, transforming plate; O P, opening for secondaries to reach skin; D R, ridge rising from floor to cut off primary rays from opening; W, position of lead-glass window for adjustment observations. The course of the primary and secondary rays is indicated. Scale, one-fifth.

Later on I provided grooves for the plate to run in, and was thus able to use slides coated with various substances, such as arsenic and bromide of silver. The transforming plate should be set at an angle of 45° to the floor of the box, and the opening below it ought not to exceed one-third its diameter. The anticathode should be placed at a distance from the centre of the plate equal to at least one and a half times its diameter. These relationships are necessary if an even distribution of the secondary rays is to be obtained, combined with a maximum of efficiency. In my own instrument I use a non-regulating tube having a 5 in. bulb (between 12 and 13 cm.). The transforming plates are 22 cm. square, and the target is consequently 33 cm. from the centre of the slide. The whole affair is not at all clumsy, its external dimensions

[1] A layer of any desired powder may be coated on to a wooden plate by means of varnish.

being about 20 in. by 9½ in. by 8 in. (50 cm. by 24 cm. by 20 cm.) (fig. 8).

The instrument above described can be constructed by any local cabinet-maker, and it supplies us with a pure secondary radiation. Its chief disadvantage is that, considered as a transformer of energy, its efficiency is low. A photographic plate placed close to the opening requires, to reach a given density, from twenty to thirty times as long as if it were placed at a distance of 12 in. from the anticathode. In the treatment of skin lesions, however, the disproportion is found not to be so great. Thus, if exposures of seven minutes every other day would be required by the usual methods, half an hour daily may prove sufficient when the ray transformer is employed. By using an exciting tube designed to carry heavy currents this time could no doubt be very much reduced.

I have gone into detail in referring to this transformer because I know that it gives a radiation of definite quality, and because I have actually used it in treatment. It is, however, very slow, and can only be looked on as a stop-gap. I am at present experimenting with an instrument in which the transforming plate is modified into a tubular shape, and surrounds the Röntgen bulb. It is certainly highly efficient, producing a kind of shower-bath of secondary rays; but I do not yet know whether the radiation given is pure, and I have not employed it therapeutically. A diagram is shown in fig. 9.

Before we ask a patient to submit to longer exposures and more frequent sittings we must be sure that there is due reason for putting him to increased trouble and expense. That, in certain cases, one is justified in doing so I feel sure. Skin lesions situated over the breasts and ovaries in the female, and in the neighbourhood of the external genitals in males, are examples of what I mean. At present we often hesitate to treat, let us say, *pruritus ani*, because of the risk of sterility; but if we choose a specific ray having an E.T.R. of half a millimetre or less, the danger becomes negligible. It is true that a few scattered hard rays may reach the patient, but in such small amounts that they may be disregarded.

The employment of pure secondaries in the treatment of skin disease should, I think, make this branch of therapeutics more nearly an exact science than has previously been the case. It is well known that two tubes having the same penetration, and giving a Sabouraud dose in the same time, are not necessarily alike in their action on disease. But when we deal with homogeneous specific radiations it

will probably be found that for a given type of ray the photographic effect and therapeutic effect bear to one another a close and constant relation. We shall be able to experiment on a given disease with various radiations until we find the one most suitable; and, having found it, a Kienboeck quantimetre or some such instrument will enable us, in connexion with other data, to describe our technique so that it may be repeated by all. Such a description should mention:—

(1) The element, or combination of elements used as converters.

(2) The dose in Kienboeck or other photographic units.

(3) The time taken to administer the dose.

(4) The distance between the centre of the transforming plate and the skin.

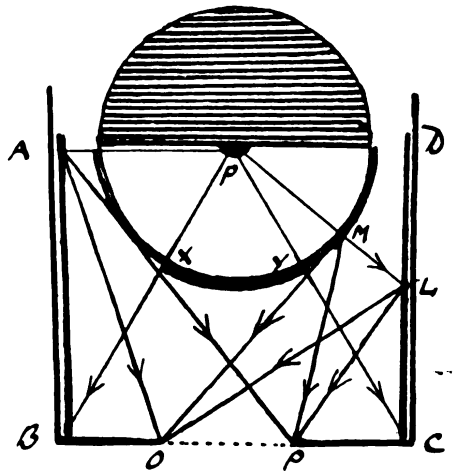


FIG. 9.

Diagram of suggested tubular ray transformer. *R*, Röntgen bulb; *P*, anti-cathode; *X Y*, ray-proof material cutting off primary rays from opening *O P*; *A B C D*, tubular box lined with substance for emitting specific secondary radiation. Cloth impregnated with the same substance covers rest of bulb, apart from *X Y*, and emits secondaries. The course of certain of the primary and secondary rays is indicated by arrows. Scale, one-fourth.

This last record is not necessary if the same make and size of ray transformer is always employed. Details as to the primary radiation are, fortunately, of no importance—except in so far as they enable each operator to standardize his own apparatus.

For the benefit of anyone interested in the scientific side of the subject, I may say that many interesting photographic experiments

may be carried out with a ray transformer. Thus, the penetrating powers of various specific rays may be investigated by interposing plates of gelatine, which represent tissues fairly well, and are more convenient than water. The thickness of gelatine is noted, which causes the rays to take twice as long to produce the same result as when none is interposed. Patience is needed, however. The path sounds easy, but is in reality beset with small difficulties and annoyances.

Radium and Secondary Radiation.

The question as to the relative value of X-ray and radium therapy is a vexed one. I am of opinion that by the aid of secondary radiation X-ray workers may be able to achieve at least some of the therapeutic feats at present performed only by the possessors of radium in some quantity. I am not one of those who believe that because two forms of radiation behave in the same manner as regards absorption by living tissue their physiological action must, therefore, be the same. Sir J. J. Thomson tells us that the specific radiation of silver has about the same power of penetration as the *beta*-rays of radium; by which, I suppose, he means the *average beta*-ray. But the *beta*-ray is a negatively charged particle, whereas the silver ray is an ether-pulse. It is because of this essential physical difference, which may well extend to therapeutic effects, that I think it so necessary to devise a sure method of utilizing the secondary corpuscular radiation previously referred to: the particles composing it being identical with *beta*-rays in all but speed. But although specific rays may not produce results precisely similar to those obtained by radium, yet there can be little doubt but that they possess a peculiar value of their own. The X-ray therapist has a new weapon placed in his hands, and before declaring in some obstinate case that "only radium can cure it" he may well experiment with various types of secondary radiation, applied by one or other of the various methods which I have outlined. The necessity for research in X-ray therapeutics is as urgent as ever, despite recent work on radium; for were we all convinced of the indisputable superiority of the latter, and furthermore possessed of fortunes, it would not be possible for many years to obtain a supply sufficient for a tithe of our patients' needs.

Although the subject is not alluded to in the title of this paper, I will ask your attention for a few moments to secondary radiations produced by radium. Works on the physics of radium state that both

the *beta*- and the *gamma*-rays produce secondary effects, and that both a particulate and a non-particulate radiation is emitted. Very little more than this bare statement appears to be forthcoming. I possess only a few milligrammes of radium bromide, but I have performed some experiments which, I hope, may at least suggest therapeutic possibilities. My radium salt is contained in a platinum tube, and in taking photographs I used a filter of 1 mm. of lead, the latter being covered by thick cardboard. Thus the effects obtained must be put down almost entirely to the *gamma*-rays. This makes for simplicity in trying to interpret the results by what is known of secondary Röntgen radiations.

The behaviour under radium stimulation of a heavy metal such as lead is of much interest, as it is markedly different from what occurs when X-rays are used as an excitant. You will remember that with the latter a photographic image may be obtained by andrastic rays, provided that the metal be placed in direct contact with the plate. In the case of radium, however, the interposition of the usual protective envelopes produces no visible diminution in the intensity of the anagraph; but a piece of cardboard 1 mm. in thickness is sufficient to cut off most of the effect. It is obvious that an abundant soft secondary radiation is emitted by the lead, the effective therapeutic range of which is probably not less than $\frac{1}{7}$ mm. A considerable part of this radiation I believe to consist of negatively charged particles—in other words, of secondary *beta*-rays. I may here remind you that the penetration of the corpuscular rays excited by a Röntgen bulb increases with the hardness of the incident beam. Now the *gamma* radiation of radium is about thirty times as penetrating as the hardest obtainable from an X-ray tube; one would therefore expect, on theoretical grounds, that the secondary corpuscular rays excited by the former would be of a proportionately vigorous type.

It is open to the radium therapist to utilize secondary radiation by any of the methods previously described under the heading of "treatment by intensification." Of these, the use of pandrastic rays in the case of tumour masses appears to me likely to be of the most importance. By ordinary methods, from 90 to 95 per cent. of the *gamma* radiation escapes on the far side of the growth, and is lost. By the injection of some heavy substance we might cut this waste down by a third, and still have a fairly even absorption throughout the tumour. As a result, there would occur within the mass a powerful *beta* radiation, which could scarcely be without therapeutic effect. We need not in this

instance trouble our heads as to whether or not the molecules of the intensifier reach the interior of the cells, for the emitted corpuscles are endowed with an initial velocity which enables them to reach the nuclei, even when discharged from the intracellular spaces.

These secondary *beta*-rays possess sufficient penetration to make them of value in skin lesions. If a patch of skin is being treated by radium in a tube at least half the total radiation is wasted. If, however, a shield of suitable metal be placed behind the tube, after the manner in which a reflector is placed behind a light, secondary rays are produced which can be shown to increase photographic effect by 15 to 20 per cent., besides causing a more even distribution. As the primary energy is strictly limited, mere increase of quantity is of importance to the user of radium. A circular concave piece of tin does very well to place behind the radium tube, and may conveniently be referred to as a "reinforcing shield," or simply as a "reinforcer." It must, of course, be clearly understood that no true reflection takes place.

As to whether anything corresponding to *specific* secondary radiation occurs where radium is the excitant, I am not prepared to say. Tin apparently gives a more penetrating ray than zinc, which is what one would expect, but aluminium also seems to be active. The subject is certainly more complicated than is the case with X-rays. Fortunately, the knowledge we have is already sufficient to be of use in the carrying out of practical therapeutic tests.

When metal tubes containing radium are buried in tumours, an area of necrosis is apt to form in their immediate vicinity. This, I think, would be best avoided if possible. It is quite possibly due as much to secondary corpuscular rays of low penetration as to the radium itself, and I would suggest enclosing the tubes in thick celluloid cases before insertion. When using *gamma*-rays to affect deep-seated growths, we should see to it that means are taken to suppress the soft secondary syndrastic rays from the lead or platinum filters.

Before the value of secondary rays in therapeutics can be finally settled, many cases must be treated by their aid, and the results carefully recorded. I have myself experimented on some thirty-five cases, mostly by some form of intensification. I cannot, however, say more now than that the results are such as to encourage me to further trials. The object of this paper has not been so much to describe successful cases as to detail methods which will enable anyone to try the treatment for himself. In employing secondary radiation in any of the

several ways which I have described, you may be sure of two things—the first is that the substances recommended, whether for injection or oral administration, will do the patient no harm; and the second is that a radiation of known quality will be given off at the particular place where it is wanted. I venture to hope that the technique and nomenclature which I have worked out may be of some use to those interested in the subject, at any rate, until something better can be devised. Compelled, as I have been, to present you with rather more of theory than of practice, I would plead that only by the aid of well-considered theories is deliberate and ordered progress at all possible. Moreover, even when a theory ultimately proves to be wrong, the research to which it has given rise is seldom without useful result, it may be in quite unexpected directions. Apart from any clinical experience of my own, it appears to me incredible, on general grounds, that a discovery such as that of specific secondary radiation should be of no use in medicine. In the course of the next year or so this subject may possibly be much written of, but I trust there will be nothing in the nature of what is popularly called a "boom." Let us remember that the secondary ray is, after all, the offspring of the Röntgen ray, and see to it that, though we spoiled the parent, we do not sing too loudly the praises of the child.

APPENDIX.

TABLE OF SUBSTANCES LIKELY TO BE OF USE IN SECONDARY RAY THERAPEUSIS.

Element	Atomic weight	E.T.R. (approximate) of specific ray	Forms in which the element is used
Iron	55·85	$\frac{1}{40}$ cm. ($\frac{1}{4}$ mm.)	*Ferri et ammon. cit.* (B.P.) in solution for injection, or any non-irritating soluble salt; iron carbonate, inhaled as a fine powder, may possibly prove of use in lung cancer
Zinc	65·37	$\frac{1}{30}$ cm. ($\frac{1}{2}$ mm.)	*Zinc oxide*, in powder form; *zinc sulphate*, in solution, for electrolytic introduction of zinc ions
Arsenic	75·0	$\frac{1}{10}$ cm. (1 mm.)	"*White arsenic*," used in powder form, for production of "syndrastic rays" or pure secondaries (by ray transformer)
Bromine	79·92	$\frac{1}{8}$ cm. ($1\frac{1}{4}$ mm.)	*Ammonium bromide*, in solution, for injection into tumours; also in combination as *silver bromide* (powder)
Selenium (?)	79·2	Same as Br.	In combination with *eosin* it is said to enter the nuclei of cancer cells when injected into the blood-stream (Wassermann)
Strontium	87·62	$\frac{1}{4}$ cm. ($2\frac{1}{2}$ mm.)	*Strontium bromide*, in solution, 17 grm. to the litre, for injection into tumour masses ("pandrastic rays"); also as *strontium lactate*
Silver	107·88	1 cm.	Silver plates; *precipitated* silver, as "silver meals," or suspended in oil for injection beneath ulcers, &c.; *collargol* (colloidal silver), in 4 per cent. strength, for injecting into the bladder. All the above for the production of "anadrastic rays"; collargol ($1\frac{1}{2}$ per cent.) may be injected into tumour masses to produce pandrastic rays; silver nitrate, in solution, may be used to introduce the silver ion
Tin	119	2 cm.	May be used as a metallic plate in the mouth when an E.T.R. of more than 1 cm. if desired

The above-mentioned substances furnish us with a means of employing specific homogeneous radiations ranging in their E.T.R. between $\frac{1}{4}$ mm. and 2 cm. Preparations of barium (atomic weight 137, E.T.R. 3 cm.) may perhaps prove of use in the alimentary tract, but possible toxic action would have to be considered. Also, beyond an E.T.R. of 1 cm. rays become more and more subject to scattering rather than absorption. The gap between strontium (atomic weight 88, E.T.R. $\frac{1}{4}$ cm.) and silver (atomic weight 108, E.T.R. 1 cm.) might, if it were deemed necessary, be filled by the oxides of molybdenum (atomic weight 96) and ruthenium (atomic weight 101).

As regards precipitated silver, my original formula for hypodermic injection was 10 gr. to 1 dr. of olive oil. I have since used the following:—

Argent. ppt.	gr. x
Cocaine solution, 5 per cent.	♏x
Glycerini	ad. ℥i

Inject 5 minims for each square centimetre of area of ulcer. This preparation is quite safe up to 20 minims at a time.

No claim is made that the above list is an exhaustive one, or even that the substances enumerated are the best for the purpose. They are given as being those with which I am most familiar.—F. H.-J.

DISCUSSION.

The PRESIDENT (Mr. A. D. Reid) said that the Section was greatly obliged to Dr. Hernaman-Johnson on two grounds: first, for the interesting line of investigation he had taken, and secondly, and perhaps even more, for the extremely careful and scientific manner in which he had carried out his experiments. It was experiments such as these that were wanted in their branch of medicine. He was especially glad that Dr. Hernaman-Johnson had described his technique so fully, so that other workers could follow it up. He gathered that the work was being continued, and he was confident that there was a great deal of scope remaining for further work by Dr. Hernaman-Johnson himself and by investigators who might follow in his steps. He greatly admired the modesty with which the subject had been approached, and the disposition to claim no more than had actually been proved. He thought the suggested nomenclature admirable and self-explanatory, and he wondered what objection the classical authorities could have found to it. He was much interested in the X-ray transformer, and thought that it must form another useful weapon in the hands of the electro-therapeutist. He had no personal experience with these secondary rays, but with the paper in print it seemed likely that much might be attempted and performed.

Dr. H. LEWIS JONES congratulated Dr. Hernaman-Johnson upon a very suggestive and interesting paper. He was reminded of a case in which a sinus had healed up after an X-ray photograph had been taken with a silver probe in it, and its almost immediate healing after the taking of the photograph was peculiar. But there they saw probably the first instance of a case successfully treated by means of anadrastic rays. A year or two ago he had treated with radium a wart-like growth near the thumb-nail, after having previously applied zinc ionization. The result was successful, and the case might perhaps be regarded as treatment by secondary radiations, by the pandrastic method of Dr. Hernaman-Johnson, zinc ions serving as the radiator. To go into the wider question of secondary rays in general, he might mention that Sir James Mackenzie Davidson, in a discourse which he had given recently at the Royal Institution, had pointed out that the secondary X-rays emitted by the glass of an X-ray tube were of considerable importance, especially with hard tubes, and had come to the conclusion that possibly a great deal of the therapeutic and of the burning effect from X-ray tubes was due to these secondary rays from the glass. This would explain a difficulty which might have occurred to some, for Sir J. J. Thomson, on the one hand, had told them that for rays to be useful they must be absorbed, and therefore that soft rays must be preferred in treatment; on the other hand, those who had experience in the treatment of ringworm knew that a soft tube required a long time before the pastille changed colour and the epilation effect was obtained, while with a hard tube the pastille would change and the epilation result be obtained after a shorter exposure. Here there was a conflict of opinion. The physicist recommended

soft tubes, the practical man hard tubes. But it was probable that the hard tube generated an abundance of suitable soft secondary rays, which were the real active agents, while the soft tube generated secondary rays which were too soft to produce any effect at all.

Dr. FRANK FOWLER said that his experience of the secondary rays only related to one instance. The case was one of scirrhus of the breast, which was doing well under X-ray treatment, but which developed slight ulceration, or cracking, at one part. He tried zinc ionization, and irradiated afterwards. The surface healed up nicely, but he never repeated the experiment, because the patient soon afterwards developed secondary infection. It occurred to him that he had perhaps stirred up this primary growth so that it became diffused in the organism. He had read in the medical papers recently some accounts of electric colloids of metals which were said to have very remarkable properties—apparently not specific properties of each metal, but only in accordance with the shape and size of the particles. It might be possible to use these metallic colloids, and not only obtain secondary radiations from them, but perhaps some other effects in addition.

Dr. G. B. BATTEN desired to know Dr. Hernaman-Johnson's opinion as to the effect of the secondary rays from the glass of the tube in the treatment of ringworm. If Dr. Lewis Jones's theory was correct, it followed that they ought not to shield their tubes at all so far as the head was concerned—they should have not a 4 in. aperture, but a 12 in. aperture. Were the secondary radiations from the glass as used in an X-ray tube likely to go to the depth of the hair-root or not? He supposed the metal used in the glass would have a definite effect upon the specific radiation from an X-ray tube. He had a new therapeutic tube by Cossor—not the Lindemann tube, but similar in the quality of the glass employed, the glass being made with metals and metallic earths, having a much lower specific gravity than the material ordinarily employed—and the pastille was turned much more quickly, and, if one was not careful, a temporary erythema of the skin was also more rapidly produced than with an X-ray tube fitted with ordinary glass. Were these effects, together with the more rapid fall of the hair, due to secondary radiation or merely to the primary?

Dr. N. S. FINZI said that he would like in the first place to deal with the question raised by Dr. Lewis Jones as to the effective rays from an X-ray tube being really due to secondary radiation from the glass of the tube itself. He was working a good deal with X-rays filtered through aluminium filters, and a filter of 2 mm. of aluminium would cut off any rays from the glass of an X-ray tube, yet the rays thus filtered were just as effective in producing epilation as the unfiltered rays. He had proved this point to his complete satisfaction. This disposed of the question of the rays from the glass of the tube. Then it might be asked whether the effect was due to the rays from the aluminium. But aluminium was of a very low atomic weight, and would not

give these specific secondary rays. He thought that this contradicted the theory that the rays from the glass of the tube were the *only* effective rays in producing epilation, although they might assist in bringing about that result. Dr. Hernaman-Johnson's paper had dealt chiefly with soft and easily absorbed rays: the speaker admitted that he was working chiefly with very hard rays, and rays not easily absorbed, but he must also admit that there might be in the work of these rays some production of secondary rays in the tissues themselves, possibly from substances circulating in the body. The hard penetrating rays might be able to excite radiations impossible with the softer ordinary rays from an X-ray tube which passed through the skin. He was very glad that Dr. Hernaman-Johnson had finally disposed—at least he hoped so—of the bismuth meal bogey. They all knew, as a matter of practice, that bismuth meals did no harm, but it was very satisfactory to have their innocence established on a thoroughly scientific basis.

Dr. G. E. BOWKER said that the paper recalled to his mind various successes that he had had in treatment without the reason for them being obvious at the time. He had in mind particularly a case of rodent ulcer of extensive area, the treatment of which was in the first instance by means of the zinc ion. He continued to use it with but slow improvement of the granulating area of the ulcer, and so persistently that at length he began to feel that so far as any therapeutic progress was concerned he was merely marking time. At last he gave up the zinc ion and turned to the X-rays, rather as a forlorn hope than with any idea that the latter method would be superior to the former. But from what Dr. Hernaman-Johnson had said that evening, it was quite clear that this would in the near future become the treatment *secundum artem*. As soon as he began to use the X-rays the advance of the growing epithelium became most marked, and it only required three or four irradiations to bring about complete success. Nothing, at least in his own experience, more pointedly proved to him the value of the secondary rays. These he looked upon as explaining the rapid improvement which immediately ensued upon the use of the X-rays. The zinc in the course of the ionization treatment had been placed in position ready to produce its secondary rays, and it only needed the change from the ionization to the X-ray method to accelerate the cure.

Dr. G. HARRISON ORTON asked whether Dr. Hernaman-Johnson had ever tried the secondary rays on any deep-seated lesion. In April last he had under treatment a case of leukæmia reacting very readily to rays filtered through aluminium. He determined to try various other secondary rays. He had a device similar to that employed by Dr. Hernaman-Johnson, consisting of a lead tube with a window sloping at right angles, so that various metals could be slipped into position, and he tried tin, silver and cadmium with the idea of directing the secondary ray energy right down on to the spleen. He was uncertain what dose to give, for it was very evident from screen and photographic effects that there was a considerable falling off in fluorescence and, he concluded, in

power. The first dose he gave, with silver, was equal approximately to four Sabouraud pastille doses. What the secondary radiation actually was he had no idea. The effect was *nil*. He tried cadmium with a similar result. The blood count began to go up. He returned again to the aluminium, and the blood count fell to approximately 12,000, whereas it had been 200,000. The patient was himself a medical man and therefore kept exact records of his own case. In deep-seated conditions it was evident, so far as he could make out, that secondary radiations from such metals as he had tried had no effect.

The PRESIDENT asked Dr. Hernaman-Johnson if he had any experience which would enable him to state a difference between the secondary ray given off, for instance, from the old-fashioned lead-glass tubes and the secondary ray from the tubes in use at the present time. With a tube of lead-glass, what would be the type of secondary rays obtained? If it was a question, as Sir James Mackenzie Davidson suggested, of the secondary rays giving the effects, beneficial and the reverse, it would be possible, by making the whole tube of lead-glass, to obtain secondary rays alone.

Dr. F. HERNAMAN-JOHNSON, after thanking the President and other speakers for their kind references to his work, said that the President had referred to his suggested nomenclature and had asked for the objections of his classical friends. One of the scholars whom he consulted was the Bishop of Durham. He asked the Bishop, "What would you think of X-rays to which the term 'pandrastic' was applied?" "I should think they acted on everything under heaven!" was his answer. But his Lordship was not able to suggest anything better. After all, technical terms could only be understood in a particular intended sense when regard was had to their context. With regard to the type of secondary radiation coming from various X-ray tubes, he had not actually worked upon the subject, but he took it that there were two kinds of radiation apart from the main or primary. In the first place, in an X-ray tube a certain portion of the cathodal stream, no doubt, was scattered, impinging upon the glass and not upon the anticathode; also there was a certain amount of inverse current in all tubes, and this produced X-radiation by causing negatively charged electrons to impinge upon the glass. He supposed that the rays produced by the stoppage on the glass would be of a much less penetrating type than those produced by the stoppage on platinum. This diffuse radiation was, however, secondary only in the sense that it is, so to speak, accidental, and not intended for use in radiography or treatment. The second type of radiation—which was truly secondary, being caused by the main primary beam—would consist of such rays as were emitted by anything in the glass itself. He did not think that ordinary glass contained much that would give X-rays unless it were specially constructed and contained some substance having an atomic weight of 40 or more; otherwise it would not give a specific radiation at all. Most of the substances in ordinary glass, he believed, had an atomic weight below that figure. If any substance such as

zinc were incorporated in the glass—and the same held true of iron and copper —rays of the syndrastic type would come out of the tube together with the primary radiation. These rays would be specific in character, but they would be mixed up with the primaries. If sufficient of the metal were used to stop the primary radiation altogether, none of the secondary would be obtained either. In the case of a lead-glass tube, the lead-glass being of a certain degree of thickness, so that the primary rays could still get through, there would be a certain amount of secondary radiation, but it would be practically devoid of any action, being unable to penetrate even a thin layer of gum. It would consist of corpuscular rays which would be destroyed within 1 or 2 cm. of air, together with a few soft X-rays which were practically negligible. It was very interesting to learn of a rapid cure following the introduction of a silver probe into a sinus and subsequent irradiation. Almost certainly the result was due to the secondary anadrastic radiation produced by the silver probe. One must be careful in such cases to ascertain theoretically that the secondary rays would have a therapeutic range capable of producing the given result. In the case of Dr. Orton's experiment, for instance, using silver as a transformer, he gave a dose equal to four Sabouraud pastilles. His apparatus would probably have an efficiency of about one-twentieth or one-thirtieth, and therefore the effective dose would only be one-fifth or one-sixth of a Sabouraud. Further, using silver, which had an effective range of 1 cm., it was extremely doubtful whether any of the rays reached the spleen at all, or even would have done so had he gone on for a much longer time. Tin had a range of 2 cm., but even the secondary radiation from tin would hardly be sufficient to reach the spleen, which would require something having a range of more than 2 cm., and he did not think there was much use for secondary rays having a range above that of tin. Above that range they began to suffer from all the disadvantages of ordinary hard rays, and one might just as well use filtered rays of the usual type. They could, indeed, use cerium, which would have a range of 4 or 5 cm. He had himself used a plate coated with cerium oxide, and with this it was possible to affect the spleen. But by any transforming device yet invented it would be necessary to give twenty or thirty times the ordinary exposure. With regard to the employment of colloidal solutions, an injection of collargol, 1½ per cent., into tumours presented a means of introducing heavy molecules into the growth and, by means of subsequent irradiation, obtaining the effect of secondary pandrastic rays in the deeper parts. Dr. Finzi had obtained an epilation effect by using hard rays filtered through aluminium. Aluminium did not produce a secondary radiation with X-rays, although it did when radium was used. He quite believed, however, that the hard rays had themselves an effect on the skin. He had experimented on a patch of psoriasis by using a water-filter of 3 cm., and covering the lower part of the glass vessel with lint. The patch of psoriasis ultimately healed, even through the agency of penetrating rays which had passed through 3 cm. of water, but the healing took five or six times as long as it would have done in the ordinary course.

[*Addendum.*—I regret that in my reply I omitted to deal with some of the points raised in the discussion. One speaker stated that dissemination of a cancerous growth had followed his application of secondary rays, and might have been caused by them. This is quite possible, for it is a rule having few exceptions, that any agent which is capable of doing good may also, in other circumstances, produce evil results. What we have to do is to find out the conditions under which secondary rays may be *beneficially* used. Just as the parent rays may in certain cases do harm, so doubtless may their offspring. Reference was also made to secondary radiations produced by heavy elements normally present in the body. Of these the most important is undoubtedly *iron*, which is present in the corpuscles to the extent of fully 1·5 parts per 1,000. I have not gone into the matter, but it is certain that a powerful secondary radiation, both specific and particulate, must be generated within the blood cells. The marked effects of X-rays on blood-vessels and their contents is well known, and the above fact may possibly help to explain them. It may also serve to account for the observation that skin rendered anæmic by adrenalin becomes more resistant to ray treatment. As regards special organs, it must be remembered that the spleen is rich in iron, and the thyroid gland in iodine. The subject is a wide one, and calls for careful investigation.—F. H.-J.]

A Method of Reducing Excessive Frequency of the Heart-beat by means of Rhythmical Muscle-contractions Electrically Provoked.

By W. HAMPSON.

STUDENTS of recent electrical work are aware that the stimulation of muscles by means of pulsating electrical currents, which cause their alternate contraction and relaxation in fairly quick succession, have a specially beneficial influence upon their development. Dr. Lewis Jones has explained the principle in several papers, and some very good arrangements for producing the pulsations are regularly employed at St. Bartholomew's Hospital. I have introduced them at the Queen's Hospital for Children, and the results have seemed more favourable both to myself and to the parents of the children than we were accustomed to when steady currents, of whatever kind, were employed.

Bergonié's form of faradization with a secondary of coarse wire, transforming a current in the ratio of two to three, with electrodes of large size, allows of the passage of current through large areas without pain or unpleasantness. This facilitates the comfortable stimulation of many large muscles simultaneously to vigorous action of an intermittent or rhythmical nature. The details of this system were described by Dr. F. Howard Humphris in the *Journal of Advanced Therapeutics*, New York, October, 1911, to which I will refer you for particulars of technique.

Lastly, we have long been familiar with that treatment of suitable cases of heart disease by properly graduated exercises with which Dr. Schott's name is particularly associated.

These principles, applied in combination, produce some results of great interest and importance. The contraction of a muscle squeezes out blood from the veins, the action of the valves securing its passage in the right direction—viz., towards the heart. The next relaxation of the muscle allows the veins to fill again from the minute veins and capillaries, and to receive a much larger charge than if the veins had not been emptied by the previous contraction. With the succeeding contractions and relaxations of a pulsating current these processes are repeated. Thus the muscle, under the influence of pulsating stimulations,

maintains a flow, but a pulsating, not a steady, flow, of blood towards the heart. It becomes, in fact, itself a modified heart, very closely imitating in its action, if not in its method of stimulation, the real heart. Such a state of things arises in any muscle or set of muscles which contract with regular intermittence, as in walking, cycling, or rowing. It usually happens, however, that this effect is so limited locally, or so interfered with by the unregulated action of other muscles, as not to produce the particular general result which will shortly be described.

Under Bergonié's system, when these intermittent contractions are applied simultaneously to most of the large muscles of the body, a large part of the body is thus converted into a pump which, while assisting the blood-stream in its natural course, creates in the veins waves of pressure and movement which occur, as the treatment is usually given, at the rate of 100 a minute or more. The resulting assistance to the flow of blood in the veins may be compared with that of the contraction of the muscular coats of the veins in a bat's wing, or of the work of the lymph-hearts in reptiles and some birds in promoting the flow of the lymph.

The treatment which is the subject of this paper depends upon the regulation of the frequency of such intermittent muscle-contractions in a definite relation to the frequency of the patient's heart. If the contractions be provoked by stimuli arranged to be as nearly as possible synchronous with those of the heart, the artificial waves of blood-pressure set up in the venous system, instead of giving their assistance to the heart in a merely general sense, sometimes at the beats and sometimes between them, now reinforce its action at periods coincident with its own impulses. Thus, that portion of the stimulation to the heart's action, whatever it be, which depends upon the pressure in the great veins is intensified sufficiently to bring the heart-beats from approximate synchronism to exact synchronism with the electrically stimulated muscle-contractions. Pursuing this line of action, if the electrical interruptions be made definitely slower than the heart-beats, the latter will, if the disparity of frequency be not too great, be brought into conformity with the former, so that the initial pulse-rate is lowered. When time has been allowed for the new rate to be established, the interruptions may be further slowed by means of the metronome, and the pulse-rate further lowered.

A lady approaching the age of 50, who had many years before had acute rheumatism, was suffering from a dilated heart complicated with

multiple arthritis and a weak digestion. For a few seasons previously she had obtained distinct benefit from courses of treatment at Nauheim. She arrived for treatment each morning a little before noon with the pulse varying from 90 to 110, according to the state of the stomach and the pain suffered during the preceding night. During a treatment of twenty to thirty minutes the pulse was usually brought down to 80 or thereabouts. It was as much improved in strength and regularity as it was reduced in frequency. The patient was at the same time greatly relieved from distress and felt in every way more comfortable. The good effects lasted well through the afternoon. A course of two months left her much better in many ways. The expression of distress was gone; the skin was fresher and brighter; the weight had increased, and so had the bodily strength.

An old gentleman who has a somewhat dilated heart, with an ordinary pulse-rate of 84 when he is not taking special care of himself, had a single treatment, which brought the pulse to 72. He felt relieved from a certain amount of distress which he had felt before treatment.

A patient who is in the habit of taking heavy doses of quinine for the purpose of warding off colds underwent the treatment when his pulse was, after six hours' quinine-taking, 92, and there was tinnitus, headache, dizziness, and intestinal discomfort. In this case the metronome was never set below 88, and when the pulse had fallen to the same rate, treatment was continued at that speed for half an hour. During the next two hours all the unpleasant effects of the heavy doses of quinine, which usually persisted for twenty-four hours, had disappeared. The pulse-rate was 72, the head was clear, and the general feeling of well-being much greater than in ordinary good health. Only the taste of the drug, so heavy had been the dosage, persisted as usual in the mouth till the next morning. This shows that there may be cases where the treatment is capable of steadying the heart and circulation, apart from the special reduction of pulse-rate at the time of treatment, as is indeed invariably found when it is given for other objects.

In cases of weak and excessively rapid hearts the treatment appears to give benefit in three ways:—

(1) The improvement in the circulation beyond what the unaided heart is capable of maintaining, increasing the pressure on the right side of the heart, and diminishing the resistance against a free flow through the capillaries, improves all the functions of the body, and with them

the general health. Especially, by giving a better circulation through the lungs, it relieves distress and difficulty in breathing, whether due to faults which are primarily of the lungs themselves or of the heart. Thus some of the most powerful stimuli to excessive action in a weak heart are removed.

(2) The heart itself shares in the improved nutrition due to an improvement in the circulation.

(3) The heart is relieved of some portion of the muscular effort necessary to maintain the circulation, so that a greater amount of circulation work is done while the heart is nevertheless in a partially resting condition. The treatment may be looked upon as the conversion of the large muscles of the body into subsidiary hearts.

In other cases this treatment is given in the form of very vigorous contractions: in delicate heart cases the contractions must be kept at a minimum. The smallest contractions are sufficient to move the blood. Strong ones would set up activities of metabolism which in turn would cause stimulation of the heart to excessive action. This rule must be carefully followed even though, in a sensitive patient, pain, discomfort, or distress should for the time prevent success in reducing the pulse-rate. The reduction should be gradual, so that the contractions governed by the metronome are never entirely out of step with the beats of the heart.

This treatment might well be tried as a means of restoration from impending asphyxia in cases of drowning or poisoning by anæsthetics in surgical operations. However actively artificial respiration may be carried on, if there is no blood circulating through the lungs half of the requirements for success are wanting, and the exposure and direct massage of the heart are a measure too heroic for common employment. But vigorous contractions of the large muscles might succeed in driving the blood through an entirely passive heart and through the pulmonary arteries and capillaries as well, and however feeble the stream of aerated blood returning from the lungs under artificial respiration, it might just suffice to maintain the bodily functions till vitality was restored to the nerves upon which depends the stimulation of the heart and of the muscles of respiration. If the heart were not absolutely passive, the help and stimulus of the artificial blood-stream would probably prevent it from becoming so.

These, however, and similar occasions for employing the method are only accidental and comparatively rare. But there is a large field of usefulness for it in the numerous cases of dilated, weak, and irritable

heart in those who cannot go to Nauheim, and have been disappointed in the results obtainable from the imitation Nauheims at home. It is not unlikely that in a large proportion of these cases the tonic effect on the heart will be found to be under this treatment more prompt and definite than under the Nauheim régime itself. Those who absolutely must stay at home and continue to some extent their previous work should find in it a means of staving off the deterioration otherwise inevitable, and of obtaining improvement instead. The lady referred to above contemplates resuming this treatment instead of returning each season to Nauheim.

DISCUSSION.

The PRESIDENT (Mr. A. D. Reid) thought that Dr. Hampson's paper was particularly interesting, if only for the suggestion—upon which Dr. Hampson did not lay great stress—as to the use of the method in cases of sudden shock occurring in the operating room. It was just possible that an apparatus of this kind might furnish a means of dealing with sudden shock effectively where other methods failed.

Dr. F. HOWARD HUMPHRIS said that he had had the privilege of seeing the case of the lady referred to by Dr. Hampson and he could confirm all that had been said upon it. He had had several cases since Dr. Hampson brought this matter to his notice—cases of dilated, irregular and rapid hearts; the way in which the heart responded to treatment was remarkable, and there was also marked subjective improvement. In one case, which was also under the care of Mr. Arthur Evans, for some ear trouble, that gentleman remarked upon the ease and absence of breathlessness with which the patient went up into his room having undergone this treatment. He thought that the observation of the effect of this method on the heart was purely Dr. Hampson's own. He wrote to Bergonié, who had been using this method of electrical stimulation for more than ten years, and he replied that he had never noticed the effect. The discovery was entirely due to Dr. Hampson. The good effect of the treatment made its appearance even from the first two or three days, the breathlessness and other symptoms improving from the first week. Dr. Humphris thought that this form of treatment might save patients the trouble of going to Nauheim, and it was truly exercise without fatigue.

Mr. W. DEANE BUTCHER added his testimony to the benefit derived by this treatment. He had had the pleasure of seeing one of the cases to which allusion had been made. He believed the method to be capable of further development and likely to prove useful, combining the effects of passive

auto-massage in patients of feeble circulation, with this regulating and slowing of the pulse which was very marked and instructive. He had himself submitted to experimental treatment and had found a certain amount of slowing of the pulse and reduction of blood-pressure.

Dr. N. S. FINZI said that he failed to see why the contraction of a muscle should cause the blood to go in one direction more than in another. It seemed to him far more likely that the treatment was an excellent and special form of massage of the muscles.

Dr. HAMPSON, in reply, said that practically the only point he had to answer was that raised by Dr. Finzi, why should the contraction of a muscle help to move the blood in one direction rather than in another? Dr. Finzi had forgotten for the moment something very essential in our anatomy. The veins were provided with valves and were, indeed, other forms of heart, but on a tubular instead of a globular plan. Every time that these were squeezed, either by more or less perfect contractions or by massage itself—every time that any part of the body which contained the blood was squeezed—the blood was undulated in the direction in which the valves offered no resistance to it, that is to say, towards the heart, thus naturally assisting the circulation.

Electro-Therapeutical Section.

March 15, 1912.

Mr. A. D. REID, President of the Section, in the Chair.

The Radio-therapeutic Treatment of Uterine Fibroma.

By Dr. H. BORDIER [1] (Lyons).

(Translated by W. DEANE BUTCHER.)

I HAVE to thank the President and Council of the Electro-Therapeutical Section of the Royal Society of Medicine for the honour they have done me in asking me to come here to give a detailed account of my technique on the radio-therapeutic treatment of fibroma of the uterus. I shall endeavour to present before you my method in all its details, so that you may apply the new treatment in all security. In this way one may hope that English radio-therapeutists will avoid the experimental stage and the difficulties through which I have myself passed, and more especially escape those unfortunate accidents of radio-dermatitis, whether immediate or deferred, which we have occasionally to deplore in France.

I can only briefly refer to the history of the method. In 1903, Albers-Schönberg showed that X-rays exercised an injurious influence on the cells of the male reproductive organs. Soon afterwards Halberstaedter showed that the X-rays produce marked atrophy of the ovaries in rabbits, and Bergonié, Tribondeau, and Récamier confirmed the extreme fragility of the sexual cells when exposed to the X-rays. It was in 1904 that Foveau de Courmelles treated cases of fibroma with X-rays and observed an amelioration in the hæmorrhage in patients so treated. These results were confirmed by Deutsch at the end of 1904, and Professor Imbert, of Montpellier, also published some favourable cases. In November, 1905, Kocher also published a case of amelioration, and later on Langpelzer, Görl, and Frankel reported a number of cases of fibroma whose growth had been definitely modified by X-ray irradiation. In August, 1909, I myself reported

[1] Professeur Agrégé de la Faculté de Médicine de Lyon.

three cases of fibromatous patients cured by radio-therapeutic treatment. In these patients the hæmorrhage was arrested, the menstrual period was suppressed, and, moreover, the volume of the fibroma was considerably diminished.

We were thus in possession of a means, which was both safe and efficacious, of producing the regression of certain uterine fibromata. Albers-Schönberg, in April, 1910, even asserted that radio-therapeutic treatment was the "method of choice." Since then numberless observers have confirmed his results. In August, 1910, Bergonié and Speder, and later on Escluse, published successful cases.

At the Congress of Physio-therapy at Paris, in 1911, Dr. Siredey, of Paris, expressed himself thus: "It is in reality surgery alone that can ensure the radical cure of fibromyomata. The advance in surgical technique and the skill of the operator have rendered surgical intervention so successful that it has become the treatment of choice. Nevertheless, for some years past a new method has made its appearance. The Röntgen rays already count a number of decisive results, and these are so numerous that radio-therapy has already established its place in the treatment of fibroma. The X-rays apparently provoke the atresia of the Graafian follicle, and this is followed by a process of sclerosis. The X-ray treatment is, in fact, a bloodless castration, which promptly brings on an artificial menopause. The influence of the X-rays is felt alike by the uterus and the myoma, of which it determines the rapid regression by a mechanism which is, as yet, but ill understood."

The first experiments in gynæcological radio-therapy by Foveau de Courmelles were made with a considerable number of séances—a hundred to a hundred and fifty irradiations. Later on some radiologists gave an irradiation every fortnight, while others proposed to irradiate the ovaries from the back, a method which is not to be recommended, since the ovaries are nearer the front than the back of the abdomen, and in the prone position the tissues to be traversed by the X-rays are thicker and more opaque.

In my early attempts I employed the method by cycles of irradiation. My method was to give a series of nine irradiations, three at each portal of entry, with an interval of twenty to twenty-five days between each series. At each irradiation I gave a dose such that the tint of the pastille corresponded to tint O of my chromo-radiometer. At that time the pastille was placed *under* the filter.

Bergonié and Speder preferred to apply the whole dose at one sitting.

This has its disadvantages, as the integument is much better able to support fractional doses, as may be seen in the treatment by epilation. In the treatment of hirsuties it is possible to give a dose of 2·5 H on eight successive occasions, using an aluminium filter 0·5 mm. thick. This corresponds to a total dose of 20 H without the production of the slightest erythema, provided we allow an interval of three weeks or more between each irradiation.

My first method by a series of three cycles was good enough in principle and I have continued it in my later method. I found, however, that the pastille dose O, the pastille being under the filter, was too strong to be frequently repeated. Moreover, when a long series of irradiations were given it was found that the absence of immediate erythema or radio-dermatitis did not absolutely insure us against a later tardy and deferred injury to the skin. After seven or eight series of irradiations the skin might preserve its apparent integrity, but nevertheless it had sustained certain trophic injuries which might in certain cases make their appearance six or even twelve months after the termination of the treatment. These trophic troubles have also been observed by Speder, of Bordeaux. They appear to be due to a diminished vitality of the skin of the abdominal region, which is often badly nourished and invaded by adipose tissue. Sometimes even after a long interval a chronic arteritis appears to set in, with swelling of the walls of the arterioles, and the production of an ulcer which is very difficult to heal.

I was obliged to modify my treatment so as to avoid all danger of these tardy trophic changes. I found this is perfectly possible by taking one or two precautions. First, the number of cycles should not exceed four or five at most. Experience has shown me that after this—i.e., five series, each of nine irradiations—nothing further is gained by prolonging the treatment. As an additional precaution we should take care that the radio-therapeutic treatment should not endanger the success of any surgical intervention which may be necessary later on. The skin of the median region of the abdomen should be but slightly irradiated, so as not to interfere with the healing of any subsequent surgical incision.

In the radio-therapeutic treatment of myoma by far the most important point is the dosage. I measure the dose by the aid of my chromo-radiometer, which you all know, and which depends on the alteration in the colour of a platino-cyanide pastille. The quantity of the X-rays is measured in H units, or better in I units. The I unit is more exact as it depends on a chemical reaction which can be weighed. One unit I = 1·4 units H.

In my new technique I always employ the same dose, 5 H, or 3·6 I. I give this same dose whatever the portal of entry, and whatever the thickness of the aluminium filter; the measuring pastille is placed over and immediately on the filter. The dose is thus always constant and equal. The only thing that is changed is the thickness of the filter, and this varies in accordance with the portal of entry and the number of the series. In this way the amount of the rays absorbed by the skin can easily be regulated by careful adjustment of the thickness of the aluminium filter. The constant dose 5 H is measured on my chromo-radiometer by bringing the pastille, which is above the filter, to a tint corresponding to tint 3 of my scale. The comparison should be made by artificial light—a candle or a small petroleum lamp. If it is preferred to measure the dose by daylight, it is best to use a photometer, such as is described in the *Archives of the Röntgen Ray*. In this case we must obtain a tint corresponding to tint 1 of the scale.

It is convenient to place the anti-cathode always at the same fixed distance from the abdomen of the patient. A practical means of doing this is to adjust the Röntgen bulb just a hand's breadth from the skin, so that the four fingers can pass between the glass and the patient. In irradiating the median area of the abdomen the aluminium filter is in every case the same—viz., 3·5 mm. thick. As regards the two lateral regions, the filter is increased in thickness for each cycle, so as to diminish the dose of X-rays absorbed by the skin.

A series of irradiations consists of three cycles, one at each portal of entry—i.e., nine in all. Each day only one irradiation is given, for instance, the right flank on the first day, the left flank on the second day, and the median region on the third day. Thus a complete series requires nine days. The treatment should be carried out in the interval between two menstrual periods.

The following is my formula of filtration for the lateral ovarian regions:—

FORMULA OF FILTRATION.

	First irradiation	Second irradiation	Third irradiation	Total dose under the filter
First series ...	0·5 mm. (2·0 I)	... 0·5 mm.	... 1·0 mm.	... 7 H
Second series ...	0·5 ,, (2·0 I)	... 1·3 ,,	... 1·5 ,,	... 6 H
Third series ...	1·0 ,, (1·3 I)	... 1·5 ,,	... 2·0 ,,	... 4 H
Fourth series ..	2·0 ,, (0·8 I)	... 2·5 ,,	... 3·0 ,,	... 3 H
Fifth series ...	2·5 ,, (0·6 I)	... 3·0 ,,	... 3·5 ,,	... 2 H

In order to determine the exact dose corresponding to each irradiation, I propose to make use of the experiments of my friend Dr. Guilleminot on the absorption of the rays by different thicknesses of

aluminium. These experiments are made with rays of a high degree of penetration—i.e., very hard rays, as measured by the radio-chromometer—such as should always be used in the treatment of fibroma. Taking 5 H as the quantity of X-rays incident on the filter, the following quantities are transmitted by an aluminium filter of the thickness named :—

Thickness of filter	Percentage of rays transmitted	Filtered dose
0·5 mm.	55 per cent.	2·0 I
1·0 ,,	37 ,,	1·3 I
1·5 ,,	27 ,,	0·97 I
2·0 ,,	22 ,,	0·8 I
2·5 ,,	17 ,,	0·6 I
3·0 ,,	14 ,,	0·5 I
3·5 ,,	12 ,,	0·43 I

These figures give us the quantity of rays received by the integument at each irradiation. To these we may add the small amount due to the median irradiation, which filters through 3·5 mm. of aluminium. Under such a filter the skin receives only 0·4 H.

In adjusting the Röntgen tube for the lateral illumination it is important to direct the normal ray—i.e. the direction of maximum illumination—directly over the ovarian region. The tube should be as nearly as possible 9 or 10 Bénoist. I use a water-cooled Müller tube, 16 cm. in diameter. With this tube, at the distance I have recommended—i.e., a hand's breadth from the skin—the interval between the anti-cathode and the skin is 18 cm.

As regards the aluminium filter, there are certain necessary precautions. It should be carefully earthed, and should, moreover, be held in the hand of the patient, and this hand should itself be covered by a sheet of lead. This is to avoid discharge or sparking from the filter which is in contact with the abdomen, and is itself charged by induction from the static electrical charge of the Röntgen bulb. During the lateral irradiation, in addition to the aluminium filter above described, a strip of lead 10 to 12 cm. in breadth should be used to protect the median region of the abdomen. Moreover, it is important to protect the thighs, the mons Veneris, the breast, and the epigastric region. For this purpose I use large sheets of caoutchouc 0·5 cm. or more in thickness. This is a good insulator, nearly opaque to the X-rays, and in addition protects the patient from any sparking from the leads or X-ray bulb.

As already said, only one exposure should be given on any one day. This is a good practice, since there is thus an interval of two days between two irradiations following on the same region. These fractional doses are of great importance for maintaining the integrity of the skin. Moreover, we thus avoid the malaise and constitutional disturbances

which are often very marked when one ovary is irradiated with a considerable dose, or when the two ovaries are irradiated during the same séance.

The filter is increased in thickness with each cycle in order to diminish the total quantity of the rays absorbed by the skin. This also has the advantage of increasing the average hardness of the bundle of rays which pass through the filter.

With these precautions we may give four or even five series of irradiations without producing radio-dermatitis or even erythema of the abdominal wall. The only result is a brown coloration of the skin, and even this is absent in certain cases. If all the above precautions are taken we may, moreover, feel perfectly safe against the deferred and tardy radio-dermatitis to which we have already alluded.

We may ask if it is possible to estimate the quantity of X-rays which penetrates the ovaries and the fibromatous tissue, after having traversed the integument, the muscles, the adipose tissue, and the other superincumbent layers. According to Guilleminot, the mean density of this layer of superincumbent tissue is 1·04. The thickness of this layer varies in different patients. In a thin patient the cellulo-adipose tissue may have a thickness of only 15 mm., but it may be double this thickness in a fat woman. This fatty layer absorbs the X-rays, so that after the first centimetre 75 per cent. only of the sheaf of incident rays is transmitted, and after 2 cm. only 60 per cent. The cellulo-adipose tissue between the skin and the surface of the fibromyoma may be regarded as a filter, each layer of which absorbs more and more of the softer rays.

On arrival at the substance of the myoma itself the rays are still further filtered out. According to Guilleminot, the fibromatous tissue absorbs the rays in the following proportions:—

At a depth of	1	cm.	...	...	60 per cent. of the incident rays.	
,,	2	,,	...	...	40	,, ,,
,,	3	,,	...	...	30	,, ,,
,,	4	,,	...	...	22	,, ,,

From these premises we may estimate the proportion of the rays which penetrates to a depth of 6 cm.—i.e., 2 cm. through the cellulo-adipose tissue and 4 cm. through the tissue of the fibromatous tumour.

During the course of the first series of irradiations we may calculate that the skin under the filter has received a dose of 7 H. From the table of co-efficients of transmission of the various tissues it is easy to calculate that the surface of the fibroma will receive 4·2 H, and that 0·9 H will have penetrated to a depth of 4 cm.

During the second series of irradiations the dose received by the skin will be 6 H. Of this, 0·75 H will have penetrated to a depth of 4 cm. in the thickness of the fibroma.

During the third series the skin will have received 4 H, and of this 0·5 H will have penetrated to the same depth.

Similarly during the fourth series the skin will have absorbed 3 H, of which 0·4 H will have penetrated to a depth of 4 cm. into the tumour, that is, 6 cm. below the surface of the skin.

During the whole of the four series the total dose absorbed by the skin is 20 H. Of this, 12 H has reached the surface of the fibroma, 7·2 H has penetrated 1 cm., 4·8 H has penetrated 2 cm., 3·6 H has penetrated 3 cm., and 2·6 H to a depth of 4 cm. The total amount of X-rays penetrating to the deeper layers of the tumour is thus by no means a negligible quantity, and may in part account for the regression of the tumour after radio-therapeutic treatment.

TABLE SHOWING PENETRATION OF X-RAYS INTO THE SUBSTANCE OF A FIBROMA.

Skin	First series	Second series	Third series	Fourth series	Total
Cellulo-adipose tissue :—					
1 cm. ...	—	—	—	—	—
2 ,, ...	4·2 H	3·6 H	2·4 H	1·8 H	12·0 H
Fibromatous tissue :—					
3 cm. ...	2·5 H	2·1 H	1·45 H	1·0 H	7·2 H
4 ,, ...	1·7 H	1·45 H	0·95 H	0·72 H	4·8 H
5 ,, ...	1·25 H	1·0 H	0·7 H	0·54 H	3·6 H
6 ,, ...	0·9 H	0·75 H	0·5 H	0·39 H	2·6 H

The next question to consider is the quantity of X-rays which will reach the ovary. For this purpose let us suppose that the anterior surface of the ovary is 5 cm. from the skin of the abdomen. As the ovary is 1·5 cm. thick, this will give a distance of 6·5 cm. for the posterior surface of the ovary. Under these conditions, according to Guilleminot, the anterior surface will receive 25 per cent. of the incident rays, and the posterior surface will receive 20 per cent. The median plane of the ovary, equidistant from its two surfaces, will therefore receive the following doses: during the first series, 1·5 H; during the second series, 1·3 H; during the third series, 0·8 H; during the fourth series, 0·6 H.

The total dose received after each series will therefore be: after the first series, 1·5 H (for patient of 48); after the second series, 2·8 H (for patient of 45); after the third series, 3·6 H (for patient of 42); after the fourth series, 4·2 H (for patient of 39).

The dose of X-rays required to procure an artificial menopause varies with the age of the patient—the nearer the woman is to her

natural menopause the less the dose. An experience of over four years enables me to speak with some certainty as to the dose of X-rays required to procure the atrophy of the Graafian follicles, and these doses I have set out in the above table.

Of all the organs which are affected by the X-rays, the ovary is without doubt the most sensitive. It is to Bergonié and Tribondeau that we owe an exposition of the laws which regulate the radio-sensibility of the various tissues. Cells are increasingly radio-sensitive in direct proportion to their reproductive activity, to their capacity for future karyokinesis, and to the variability of their morphology and function. The action of the X-rays on the ovarian cells was first demonstrated microscopically by Halberstaedter in 1905. He showed in the ovary of the rabbit a marked atrophy and regression of the Graafian follicles. Further evidence was obtained by Reifferscheid, of Bonne, who examined microscopically six ovaries of women who had been previously irradiated. He found in each case signs of degenerescence of the follicular epithelium, equally distributed throughout the whole organ, and in addition a number of small hæmorrhages in the cortical layer. One of the best proofs of the radio-sensitiveness of the ovaries is the occurrence of malaise and nausea during the evening or night following an irradiation, and especially when both ovaries have been irradiated.

It is not the ovary alone that is radio-sensitive, the fibromatous tissue is itself modified by the gradually increasing dose of X-rays. The ovary, it is true, exercises a certain influence on the circulation of the uterus, and also on that of the fibroma, which is itself a vascular tumour. As the activity of the ovary decreases there is, of course, a tendency to the regression of the fibroma. But in addition to this there is a direct action of the X-rays on the tumour. There are abundant evidences of this direct action, and of the radio-sensibility of the fibromatous tissue. There is, moreover, considerable difference in the radio-sensibility of different fibromata according to their age and vascularity. A recent myoma atrophies much more quickly and more completely than one of longer growth. This is found invariably to be the case whatever the age of the patient.

Indications and Contra-indications.

The indications for the radio-therapeutic treatment of myoma depend on the following factors:—

(1) *The Age of the Patient.*—I consider 39 to be the inferior limit for radio-therapeutic treatment. Patients below this age are better

treated surgically. To produce an artificial menopause in younger women would require too strong a dose of X-rays and too prolonged a treatment, with the danger of producing a dermatitis, either immediate or tardy. At this age, 39 or 40, a dose of 4·2 H is required to produce an artificial menopause. It is difficult to fix the superior limit of age. One sees patients with fibroma still bleeding at the age of 56, or even 58. In these cases the chances of success will be greater if the patient has not attained her natural menopause—or at most has not passed the menopause by more than one or two years.

Nature of the Fibroma.—It is the interstitial fibroma that is most sensitive to X-rays. Multiple or pediculated fibromata should be treated surgically.

Hæmorrhage.—The most satisfactory cases are those which suffer from abundant hæmorrhage at the menstrual period. In patients who have a discharge with clots for ten days or so each month radio-therapy acts like a charm, provided always that the patient is within the limits of age above indicated.

Volume of the Fibroma.—The most favourable cases are those in which the tumour is of moderate dimensions. Nevertheless, I have seen large tumours projecting above the umbilicus reduced to the size of an orange. As a rule, however, large fibromatous tumours should be treated surgically.

Hæmorrhages of the Menopause.—Radio-therapeutic treatment gives most excellent results in the hæmorrhages of the menopause. The table which I have already given shows that at that age one or two series of irradiations are sufficient to produce the desired result.

Contra-indications.—Radio-therapeutic treatment is subject to many contra-indications. Calcified myomata or tumours which have undergone necrobiotic degeneration should be reserved for surgical intervention. The same may be said of malignant, infected, suppurating or gangrenous tumours. When the case is complicated with suppurating salpingitis or pelvic peritonitis, it should be reserved for surgical treatment. Moreover, a fibromatous uterus may be the seat of cancer, a condition which is sometimes not easy to diagnose. Tumours of the uterine adnexa adherent to the uterus may also be difficult of diagnosis. We must also guard against mistaking tuberculous or inflammatory lesions of the adnexa for fibromata of the uterus. Notwithstanding these exceptions, we may say with M. Pollosson, "that a vast number of patients may be benefited by this treatment, which, though non-operatory, is really efficacious."

Results.

I may now proceed to give you a general résumé of my results, which will be of greater interest than a history of isolated cases.

After the first series, when the patient returns for the second treatment, we see but little change. The periods may have been less abundant, or in certain cases, when the woman is under 40, her periodic discharge, although less profuse, may not be diminished in duration. If, however, the patient be 47 or 48 years old, the coloured discharges will have been suppressed.

After the second series the results will also vary with the age of the patient. At 45 there may be a total absence of hæmorrhage and a suppression of the menstrual periods. At an earlier age the patient will still retain her menstrual periods, but the discharge will have sensibly diminished, while the fibroma will have greatly diminished in size.

After the third series, most patients over 40 will have totally lost their discharge, whether red or colourless. The fibroma will have become atrophied—in cases where it formerly projected above the umbilicus, it will now have become quite imperceptible, or at any rate have sunk three or four fingers' breadth below the navel.

If the period has recurred we must proceed to a fourth series. This is usually only necessary in cases where the patient is 39 or 40 years old. It is rare that we have to go further, although, if necessary, we may proceed without risk to a fifth series, taking care to use all the precautions which I have indicated above.

The artificial and early menopause produced by Röntgen irradiation is accompanied by the usual constitutional symptoms—hot flushes, vapours, and nervous symptoms, which may continue for some weeks. In order to facilitate resorption of the fibromatous cells I usually prescribe some diuretic drink. At the termination of the treatment we invariably observe a considerable amelioration of the general health; the colour and complexion are improved, and the patient looks better and younger. At the production of the artificial menopause the vapours do not long continue. This seems to show that the X-rays have not sensibly modified the internal secretion of the ovary. This differential action of the X-rays may be explained by the greater radio-sensibility of the Graafian follicles. The ovigenous layer is, in fact, the seat of greater cellular activity and is therefore more radio-sensitive. This conservation of the glandular function of the ovary may account for the improvement in the general health of the patient after a series of irradiations.

To impress on you the importance of these results I cannot do better than quote to you the opinion of one or two well-known gynæcologists:—

Dr. Hirschberg, of Paris, thus expresses himself as to the results in a case which I treated for him: "I am pleased to tell you that the treatment to which you have subjected Mme. X. has considerably modified her fibroma. Before the treatment the tumour was the size of a child's head—it is now the size of a small orange, the metrorrhagia, which was very abundant, has completely disappeared—indeed, her periods have completely ceased. The case is one more brilliant success for radio-therapeutic treatment."

Dr. Repelin, formerly gynæcologist to the Charité at Lyons, writes thus: "I have just examined Mme. D. and find a considerable amelioration. The fibromyoma has diminished greatly in size, to less than one-half in the course of two months; moreover, the hæmorrhage has been arrested. It is a great success."

I may also quote Dr. Siredey, who is not himself a radio-therapeutist, and who thus expressed himself at the Paris Congress of Physiotherapy in 1911: "I could report to you a number of cases in which a complete cure has been obtained without the slightest accident to any of the abdominal viscera. The treatment is also of great use in those rebellious hæmorrhages which frequently occur at the menopause quite apart from tumour, but which are usually accompanied by pronounced hypertrophy of the uterus. Radio-therapy constitutes a new treatment, the value and importance of which cannot be ignored. It determines in the genital apparatus modifications which are as profound as they are rapid, which remove functional troubles and diminish the volume of the tumour. Surgical treatment, on the other hand, conserves all its rights as regards voluminous tumours, pelvic fibromata, or multilocular tumours with symptoms of compression."

I may terminate this portion of my subject by a quotation from Dr. Polosson, Professor of Gynæcology of the Faculty of Medicine at Lyons, in the discussion on my paper read at the Societé des Sciences médicales, January 2, 1911: "I desire to add my testimony to the facts related by Dr. Bordier—facts which he has shown me and which I have personally experienced. When a year ago Dr. Bordier related to me his favourable results in the treatment of fibromyoma, I confess I received his communication with a good deal of scepticism. The results which I had previously seen had been completely negative, and in some instances disastrous. I had, indeed, seen the radio-dermatitis

provoked by X-ray irradiation, but I had failed to see the diminution of the fibroma. During this last year, however, Dr. Bordier has shown me ten cases of fibromyoma treated by his method. In five of these cases I saw the patients myself before the treatment. In the other five cases the patients had been examined by other surgeons. In every single case the radio-therapeutic treatment had produced a total suppression both of the menses and of the metrorrhagia. In every case I was also able to verify a diminution of the volume of the fibromatous uterus, the tumour having shrunk to one-half or one-third of its volume, or even more. In two or three cases the diminution was such that a surgeon unaccustomed to the combined examination by palpation and touch would have failed to recognize any remains of the tumour. These results had been obtained by three or four months' treatment. In no case did I see any trace of radio-dermatitis, but only in certain cases a slight redness of the skin."

I trust I have made it clear that the radio-therapeutist is no enemy of the surgeon. We do not expect to do away with all hysterectomies or other operations for myoma. Certain patients will, without doubt, still come under the surgeon's care. Other cases, which the gynæcologist will be in a position to indicate, will be submitted to X-ray treatment. We do not doubt that in the choice of treatment the physicians and surgeons consulted will in any given case be guided solely by the desire to cure their patients by the most simple method, and that in which they run the least risk.

DISCUSSION.

Dr. J. CURTIS WEBB said that it was not necessary for him to add any words of thanks to Professor Bordier for the trouble he had taken. It had been his good fortune to work for a short time with the Professor in Lyons, about eighteen months ago. At that time, Dr. Bordier took much trouble to show him the whole technique at that period in use. Not only so, but he had just returned from his holiday, and he collected together five or six cases which he had treated, and allowed him (Dr. Webb) to examine them and question them. Those cases were quite a revelation to him. One of the women described her fibroid tumour as of such a size that her acquaintances thought she was nearly at term with a child. He examined her bimanually, and in that way he could detect that a tumour was present, but while she was dressed there was nothing visible in the shape of a tumour. It was the size of an orange, and was causing her no symptoms. After returning from Lyons, he, in conjunction with Professor Bordier, wrote an article on the position at

the time of the X-ray treatment of fibroids. He was sorry to find that it was now necessary to recall nearly the whole of that article, because in the paper just contributed Dr. Bordier's technique was found to have altered almost entirely. What had impressed him more than anything, except the accuracy of detail with which the author had explained his methods, was the marked and distinct indications which he gave for his treatment; he showed that there was a type of case which was suitable for the treatment, and another type which was unsuitable for it. His (the speaker's) experience in the method for these conditions had been small—he had only had two cases. His first was treated with X-rays in conjunction with intra-uterine copper ionization. Whether it was the copper ionization or the X-rays which did the good, he did not know, but the result had been satisfactory. In his second case he now proposed to stop treatment. It was sent to him by a gynæcologist, and his opinion then was that it was suitable for the treatment. There was a large tumour extending to within 1½ in. of the umbilicus, and extending over each flank. He felt sure now that it would be impossible to influence it by X-rays. He had recently seen the report of an American doctor who had been using X-rays in the treatment of fibromyomata, and who, to avoid dermatitis, had been employing a freezing mixture spread on to the skin or layers of cotton-wool with ice. It was said that by that means he could give larger doses without fearing radio-dermatitis. He would like to hear whether Dr. Bordier had had experience of that freezing method. From the arrangement of filters and lead shown by the Professor, dermatitis seemed unlikely, but such might occur, and it would perhaps be an advantage to have an auxiliary means.

Dr. AMAND ROUTH said that, as President of the Gynæcological Section, he would like to thank the Council of this Section for inviting them, and to add his congratulations and thanks to Dr. Bordier for his admirable address. In his (Dr. Routh's) presidential address at the Gynæcological Section he alluded to the X-ray treatment of fibromyomata, and mentioned that he had had experience of half-a-dozen cases in which he had seen more or less improvement result. But at that time, last October, it was not known what class of cases ought to be thus treated, and practically only those cases were irradiated who refused operation. One great value of that evening's paper was that Dr. Bordier had laid down the precise indications for treatment by X-rays. He would like to understand Dr. Bordier's view of how the rays acted on the fibroid. It had been proved that the treatment sterilized the patient by atrophying the Graafian follicles, and that it acted in some measure also on the muscle cells of the fibroid itself. It was possible that it was necessary to look for something a little further back in the chain of events with regard to the effect of the irradiation upon the hæmorrhage. It was possible that the reason it did good was that the lutein tissue which was normally developed in the Graafian follicles after ovulation ceased to be formed, for it had been thought that the formation of fibroids was due to excess of lutein tissue in the ovaries. With regard to the indications laid down, he was rather surprised

to hear Dr. Bordier put the inferior limit of age at 39. From the gynæcological point of view, perhaps, it was satisfactory, for it left the surgeon something to do, but he had hoped that when the method came to the front all interstitial fibroids, not degenerating or inflamed, would be benefited by irradiation. One case which he had had was that of a nurse, aged 33. She had severe menorrhagia due to a fibroid the size of a fœtal head, and was obliged to give up hospital work. She was averse to operation, and he accordingly transferred her to the care of Dr. Ironside Bruce at Charing Cross Hospital. Dr. Ironside Bruce had dealt with all his cases which required X-ray treatment, and he had been very successful with her. He gave her eighteen exposures, nine each month, on alternate days, between the periods. The fibroid diminished in size, and her periods, though still lasting eight days, became almost normal in the amount lost. She got married and went to West Africa, a very bad place for fibroids, and stayed there fifteen months. On her return she saw Dr. Routh owing to some metrorrhagia, and he found this was due to a mucous polypus in the cervix, which he removed. The fibroid itself was smaller. There was no doubt that irradiation was most useful for pedunculated sub-peritoneal fibroids. Those with a small pedicle could hardly be expected to yield to the treatment, and submucous fibroids, especially those with a tendency to be polypoid, could be more easily dealt with by intra-uterine treatment. With regard to hæmorrhage, he had noticed that if the treatment was given just before a period was expected, the hæmorrhage came on with much greater severity than it had on previous occasions. One lady, aged 49, with a fibroid to the navel, whom he had seen with Dr. Barton Smith and Sir Francis Champneys, had a very severe hæmorrhage on her way home, and on another occasion the same lady had a severe hæmorrhage the day afterwards. This lady's periods occurred now only every two or three months. He believed that Dr. Bordier's recommendation to use the treatment at a time midway between the periods was quite the correct thing, and gynæcologists should bear that in mind. He asked whether the author had known this treatment to be used in the condition known as fibrosis of the uterus, hæmorrhage from a uterus which was undergoing fibrous change, with the vessels undergoing arterio-capillary fibrosis, a condition which no known treatment, except hysterectomy, had any effect upon. It seemed likely that this treatment might be of some use in those cases. The contra-indications spoken of were what one would expect. In the cases which he had had he had noticed that local senile changes seemed to come on rapidly and to be well marked, especially changes in the vagina. One patient ceased to have severe menorrhagia and the fibroids shrank, but she had losses at times between the periods, especially after coitus, and he found that there was a granular vaginitis, such as one sees in senile cases after the menopause, the vascular vaginal areas bleeding when touched. Another patient, aged 45, whom he saw with Mr. Stanley Boyd for menorrhagia and pressure symptoms, due to a large pelvic fibroid, suffered so severely from tachycardia that they prevailed upon her to have X-rays instead of hysterectomy. She had eighteen

exposures. Six months later the intervals between her periods were about six weeks, and the periods only lasted four days instead of ten. The pelvic fibroid also was much smaller. Then she nearly lost her life from a very severe flooding, and hysterectomy was done, lest another hæmorrhage should occur. This was a new method of treatment, and was worthy of careful trial in suitable cases, but one had yet to discover whether women sterilized by this treatment would be liable to other risks of which we were at present ignorant.

Dr. W. S. A. GRIFFITH said his purpose in attending had been to listen; he must apologize to Professor Bordier for venturing to criticize him in English. The Society was much indebted to Professor Bordier for coming over and presenting his own views of the treatment so clearly. He did not think the question as to the advantages of this treatment could be considered as established. There were many difficulties which the gynæcologist would point out, and he would be able to show that the matter was not as simple as it would appear from the blackboard illustrations. Nevertheless, if the results of the treatment were so good, theories did not matter much; they were quite secondary in importance. But if a woman of 35 or 36 years of age had her ovaries functionally destroyed, in addition to the diminution of her fibroid, and if an immediate menopause was inevitable, should one recommend that form of treatment in preference to a form of treatment of far shorter duration, in which the ovaries, if healthy, could be left, and the menopause practically delayed for a very long time? He thought that was a point which the paper had not raised. Another point was, that if in cases of fibroid of the uterus the ovaries were standing out nicely, one on each side, as in the diagrams, so that the rays could be got to attack them directly, he would say that the treatment of the ovaries might prove very useful, not only in these cases, but in some others. But every operator knew that when he opened the abdomen for fibroids, in very many cases he did not know where he would find the ovaries. He might find, as Dr. Griffith did the previous day, one ovary lying in front of the tumour and the other deep in the pelvis behind the tumour. He gathered that the evidence of this treatment being effectual could not be from direct influence on the ovary in very many cases, but the influence of the X-rays on the tumour as a whole. Then, with regard to the selection of cases, he asked who was competent, in any but exceptional cases, to suspect early malignant or red degeneration of a fibroid? And if those cases could not be suspected, how could they be excluded? They were not very common, but they were seen, and they were often unexpected. Where there were multiple fibroids, the one which caused most trouble was often a sessile one which was projecting into the cavity of the uterus, and might be no bigger than a walnut, the fibroids which formed the bulk of the tumour causing no symptoms except pressure. In such a case the treatment would probably fail unless the intra-uterine tumour was removed by operation. Next, he would ask whether a tumour which was essentially intrapelvic, as many fibroids were, could be successfully treated by X-rays, so as to cause a diminution of symptoms, or a reduction in the size of the tumour. The cases in which it ought to prove

of the greatest value were those in which apparently, according to the Professor, he would not attempt it—namely, in younger women. Sometimes one had to deal with girls under 20 years of age who had a bleeding fibroid, and it was only with great regret that one had to remove the uterus, but leaving the ovaries. In some cases it was possible to enucleate the fibroid which was causing the trouble, and leave a mutilated uterus. But it was still a uterus, and that was preferable to removing the whole of it. If some form of treatment applied to the tumour, which did not destroy the ovarian function, could be evolved by this method, it would be of the highest possible value. He would also like to ask if there was any evidence that the treatment now set out was final; or was it that in these younger women, under 36, their symptoms would recur, their tumour would again grow, and their menorrhagia return? Was that the reason why Dr. Bordier did not recommend the treatment in younger women? Was it only in older women that the treatment was permanent? He did not know whether anybody present had had a sufficiently long experience to answer that question, but he regarded it as a very important one. He was very glad to have had the opportunity of hearing so much about the method.

Dr. AGNES SAVILL asked whether, in any of Dr. Bordier's cases, the tumour remained of the same size although the menopause had been brought about. She had herself treated one case, that of a woman, aged 43, who had very severe hæmorrhage, which had persisted for ten years; but nothing would induce her to go to a surgeon; she therefore was advised to try treatment by X-rays. She (the speaker) followed exactly the technique which Professor Bordier gave in a paper in the *Archives* of 1909. At that time it was recommended that the pastille should be placed under the filter, and a dose corresponding to tint O given with each application, a series of nine (three a week for three weeks) and intervals between each series. The patient sometimes left town between the series and allowed the intervals to be too long. At the beginning of the treatment the hæmorrhage was certainly worse; it was worse after the first series, but gradually it decreased. The patient suffered greatly while under the X-ray treatment; the mental depression at the onset was almost unbearable. She had violent headaches, congestion, and a swelling of the tumour after each sitting. She (the speaker) felt it swollen during one of those attacks. The patient was a vasomotor subject, and she frequently had a pronounced angio-neurotic condition, with swelling of the face, for instance, when she had indigestion. The same events occurred after the majority of the X-ray administrations. Eventually, however, the menopause was brought about, the hæmorrhages ceased, and she was now in good health. But the tumour was very little decreased, and still reached as high as the umbilicus. The case was published in full in the *Lancet* of October 15, 1910.

Professor BORDIER replied briefly to the questions in French, and his answers were translated by Dr. Curtis Webb.

Electro-Therapeutical Section.

April 19, 1912.

Mr. A. D. Reid, President of the Section, in the Chair.

The Treatment of Rodent Ulcer.

By Reginald Morton, M.D.

The treatment of rodent ulcer is a subject that has a more than ordinary interest for the members of this Section. This is due to the fact that rodent ulcer is a superficial condition; in its earlier stages at least, it is locally malignant, but does not tend to reproduce itself in distant parts. The methods we use are mostly for local application, and whatever any of us may have to say about their use in malignant disease, we cannot get away from the fact that not one of them gives us the least control over metastasis. Rodent ulcer being without this tendency and yet undoubtedly malignant, makes for us a most excellent test of the efficiency of our methods and gives us great assistance in the elaboration of our technique.

The methods mostly in use at the present time are the X-rays, radium, zinc ionization, and solid carbon dioxide. When we consider that the disease is a malignant one it is astonishing how easy it is to get it to heal over, provided it has not invaded cartilage or bone. In fact, it heals over so readily as to constitute a real danger; numbers of cases have been reported as cured that were only healed over without having completely destroyed the disease, and recurrence has set in sooner or later.

It is on account of the relatively large number of cases of recurrent rodent ulcer I have seen during recent years that I feel constrained to bring the subject to your notice. It is a matter of serious importance to the cause we all have at heart as well as to ourselves; we must improve the permanence of our results before we get a reputation for bringing about cures that are incomplete, or at least incapable of

standing a reasonable test of time. The causes of this failure to produce more uniformly permanent results are not very hard to find. Treatment by any form of radiation depends on the absorption of the rays by the tissues, and according to the extent of the absorption the effect may be anything from a mild stimulation to an intense inflammation. It seems to be fairly well established that the rays most easily absorbed are the most active, and we have in consequence the greatest amount of reaction upon the surface towards which the rays are directed. As we go below the surface the effect of the irradiation diminishes very rapidly, and it is doubtful if it is possible to get the effect we desire in this disease at a greater depth than ½ cm. from the surface, short of producing a superficial necrosis. Assuming these ideas to be approximately correct we may consider their bearing upon the disease in question. In a typical case we get a history of a small spot or pimple upon which a crust formed, and re-formed as often as it was removed, gradually increasing in size. This has been going on for two or three years probably, and at the time we see it the area of the ulcer is equivalent to that of a threepenny piece, more or less. While it tends to increase its area more rapidly than its depth, processes are sent downwards to an extent that has an important influence upon the future of the case.

If we use the X-rays or radium the radiations absorbed superficially are so efficient in dealing with this condition that we may get the ulcer healed over before the deeper extensions of the growth are properly dealt with. They are left in a state of what might be termed suspended animation, decreasing in degree as the distance from the surface increases. This is even more marked if the case is treated with zinc ions. The salts of zinc and other metals make a superficial layer of albuminate no more than 1 mm. or 2 mm. thick, through which more ions pass with great difficulty, if at all. The two cases of recurrence in the shortest time—under three months—that I have seen were treated by this method. In a case of recurrence after one or other of these methods of treatment we may safely assume there has been one or more processes of the growth extending well down from the surface, and the farther from the surface the less has it been affected by the treatment. It is thus at the lowest point of the growth that operations are first resumed, and as the patient usually does not seek further advice until there is a superficial sore, we find a condition of things that is quite characteristic. Instead of being broad and shallow like a saucer, it is now narrow and deep, and if we clean it out with a sharp

spoon we shall find that it extends more widely and deeply than its external appearance suggests. The shape of the ulcer is now more like the interior of a Florence flask than a saucer, and the difficulties of efficiently treating such a condition with any form of radiation or ionization is quite apparent.

It is a matter of common knowledge among radiologists that numbers of complete and satisfactory cures of rodent ulcer have been made by all of the first three methods I have mentioned. I have used them all myself with at least average success; one of the first cases healed by radium was referred to me by Mr. (now Sir Frederic) Eve in 1903. It was of fairly large size, and the amount of radium available was small and of only moderate activity. Some sixty applications were given in all and the case apparently cured. Being a sailor he was not located later to find out his subsequent history. This, however, is by the way; what I wish to point out just now is that I have not come here to decry the use of methods that served us well in the past, but I am sure it will be agreed that we do get a certain number of recurrences, no matter how carefully we do our work. It is with the object of reducing the number of these recurrences that I am going to ask you to give a more general trial of a method that seems to promise well.

It was about the time I took up the use of solid carbon dioxide that my attention was drawn to these recurrences, but I hesitated to use solid CO_2 until my experience with the method had been more extensive. This was just two years ago, and I made a preliminary note of the first cases in the *Lancet* of July, 1910.[1] My first three cases were shown at the London meeting of the British Medical Association at the end of July and were considered very satisfactory by all who took the trouble to examine them, and up to a few years ago there was no sign of recurrence. In the first one the crayon was not quite large enough to cover completely the ulcer at one edge, but I left it to see what would happen. The ulcer measured roughly $\frac{3}{4}$ in. by $\frac{1}{2}$ in., and it was completely healed on the twenty-first day—the untreated edge closing in as quickly as the others. The results have been so extremely satisfactory in every way that from that time I have used no other method for treating this disease. I have now treated twenty-seven cases, and so far as I know there has been but one recurrence. In this one I again purposely left a small part uncovered by the crayon. Though it all healed over very rapidly the patient came back eight months later with a recurrence at this point. It was quite small, and a single application removed it.

[1] *Lancet*, 1910, ii, pp. 130, 198, 258.

My experience so far justifies me in saying that any case of rodent ulcer that is superficial and has not invaded cartilage or bone can be completely cured with a single application of solid carbon dioxide, and that the result will easily bear comparison with any form of radiation or ionization in regard to appearance, and I am equally confident that time will prove it to be even more permanent.

I will describe a typical case: On December 1 last a lady came to me, sent by Dr. Curgenven, with a rodent ulcer over the right malar bone. It was of two years' duration, of irregularly circular form, and its area rather less than that of a sixpence. After cleaning the surface with a sharp spoon I applied the crayon. A thin slough separated on the fourth day, and the healing was well established on the eighth. On the fifteenth day it was completely closed over, leaving scarcely a trace to show what had been there for so many months before. Of course, it is too soon to speak of permanence in this case, but it serves as an example of many I have done before and since, and except for the one mentioned there have been no recurrences as far as I know.

It may be asked upon what grounds I consider solid carbon dioxide an eminently suitable remedy for rodent ulcer. I have satisfied myself that there is little risk of producing necrosis of normal tissue by any application used in treatment, and also that it is quite easy to destroy certain abnormal tissues—rodent ulcer among them—by the same means. I do not say that freezing by solid carbon dioxide has a "selective" action on the abnormal tissues; in fact, I consider that such a term should not be used respecting a physical agent of any kind, since it conveys a wrong impression to most minds. The vitality and resisting power of the abnormal cells are less than the normal—no other explanation is required. If we heat some fragments of zinc and copper in a crucible, we know that the zinc will melt first and may be oxidized and consumed before the copper relinquishes its solid form. I have never heard it said that heat had a selective action on zinc or any other substance. I think the analogy is sufficiently close to show that the term "selective" should not be used in this connexion. Another reason why I prefer solid carbon dioxide is that it is possible to get the efficient reaction we require more easily and quickly than by any other means, and to a sufficient depth to make recurrence a much more remote event. The depth of the freezing is regulated by the time and pressure, and it is found easy to carry this far enough to encompass the region invaded by the average rodent ulcer. While the intensity of the freezing decreases with the depth, starting with a temperature of -79° C.,

there seems to be ample margin, since it does not appear that the reaction is materially affected by a reduction of temperature beyond a moderate number of degrees below zero. It is the depth of a rodent ulcer that determines the difficulty of curing it, and if cartilage or bone is involved this difficulty becomes a very real one.

The most obstinate cases I have had lately were recurrences, one of them having been apparently cured on three different occasions, first by the X-rays, then by zinc ions, and lastly by radium. On each occasion the treatment was carried out by one known to you all, each of them eminent in his branch of work. This is mentioned that you may feel sure the treatment was carried out in the best way. Owing to the depth to which the disease had reached it was worse than ever, and more difficult to treat with success. The case is now making excellent progress, and I have every reason to believe it will be satisfactory. After repeated applications it is now closing in like an ordinary healing sore. It may take several weeks to bring this about in severe cases, and of this the patient should be warned, as also of the fact that, to him, the ulcer will appear very much worse during this stage.

In one case that had been apparently cured by the X-rays, it was many weeks before it took on the healthy conditions I was trying to bring about. It is now quite closed over except for a small point of exposed cartilage, for which a slight plastic operation may be necessary. I feel confident, however, that the disease has been eradicated.

There are other reasons why I wish to put forward solid CO_2 as a regular method of treating rodent ulcer. It is well known that when recurrence follows treatment by radiation the tissues have been in some way altered, and they no longer react to the rays as they did at first; so much so in some cases that another method must be tried. I have satisfied myself that under these conditions carbon dioxide will act with its usual efficiency, and also that former freezings do not impair the reaction produced by later ones. Apparently no permanent alteration takes place in such areas, excepting, of course, a certain amount of fibrous tissue developed as a result of the inflammation. Another point in favour of the method is that we need have no fear of anything in the nature of telangiectases such as frequently follow the use of the X-rays and radium.

As I have already stated, I have now treated twenty-seven cases by this method. I have treated many other cases by other methods, but I have come to the conclusion that solid carbon dioxide is the best of them all, both for the original ulcer and for the recurrences that follow other

methods. Up to the present, in my hands recurrences have been conspicuous by their absence after treatment by this method. I do not say that such will not take place; it is not only too soon to say anything of the kind, but this would be against all human experience. My original cases have remained well over two years and seem as perfect as one could wish for at the present time. There are no serious objections to its use in this disease. It is simple, rapid in application and in result, as well as thoroughly efficient—I believe it to be more uniformly efficient than any other means at our disposal. It will, of course, be understood that I refer only to such cases that come within the province covered by the methods we practise in this Section. Severe and extensive cases requiring the interference of the surgeon are outside the scope of these comments.

DISCUSSION.

Mr. THURSTAN HOLLAND said that the treatment of rodent ulcer had been of interest to electro-therapeutists for many years. During the last two years, not wishing to depend wholly on CO_2, he had adopted the method of first freezing the rodent ulcer with solid CO_2 for one or two minutes, and immediately afterwards giving a full dose of X-rays. This was repeated in three or four weeks, sometimes renewing the CO_2 and sometimes the X-rays. He did not doubt that the method was a good one; certainly the cure was quicker than by X-rays alone. After what Dr. Morton had said, he wondered whether the effect in his (Mr. Holland's) cases had been really due to the CO_2 only. He agreed that it was yet early to speak of recurrences; unfortunately in the case of hospital patients one often lost sight of them when they seemed to be cured, or nearly so. In the old days cases of rodent ulcer attended regularly for months for X-ray treatment, but now with CO_2 and X-rays the difficulty was to persuade them to come again, because apparently there was nothing to treat after a few applications. One should continue with full measured doses of X-rays at intervals of three or four weeks, as in the case of breast conditions, and then, as in those instances, one might expect cases of recurrence to be less frequent.

Dr. FINZI said that to him the chief objection to the treatment of rodent ulcer by carbon dioxide snow was the amount of reaction produced, and the consequent pain. Fortunately, it was now the rule to get rodent ulcers in an early stage, when they were comparatively superficial, and most of the methods employed resulted in the destruction of the ulcer, although, as Dr. Morton said, there was always danger of recurrence. Probably the ordinary cautery was a very efficient method of destroying rodent ulcer, but no

one would speak of it as a painless method, and not many would regard it as the best method. Still, it was extensively used by people of large experience; and he believed that in French clinics the cautery was used almost exclusively for early rodent ulcer. The carbon dioxide snow acted very much as the cautery did in destroying the tissues. He agreed with Dr. Morton that it had a greater effect on the ulcer cells, but the snow did not act at any great depth. If a generally destructive method was required, he thought it better to use a diathermic treatment. The treatment which he now exclusively adopted for rodent ulcer was filtered radium rays: $\frac{1}{2}$ mm. of platinum was a very good filtration, but less might be used for quite small quantities of radium. That was an efficient method even when the ulcer had been healed over and there was recurrence under the skin. He had treated forty to fifty cases of the condition in that way during the last three years, and, so far, he had had no recurrence. He failed, however, to cure one case—namely, that of a man who had the whole of one cheek and the eye eaten away. In one or two cases—hospital patients who ceased to come—the treatment was not completed. Cases which had refused to heal from applications of X-rays and other measures responded well to radium, and the latter, if properly applied, was painless. Unless the ulcer was very deep no redness was caused, and a crust formed over the ulcer. This was left alone for six weeks, then removed, and another application made, and so on, the amount of crust getting gradually less. One or two additional applications should be made after the apparent disappearance of the ulcer, so as to get rid of any deeply seated ulcer cells. He admitted that Dr. Morton's was a very convenient method, but he did not think it was the best.

Dr. FRANK FOWLER (Bournemouth) said he now had a case of rodent ulcer which he had been treating for two years, for which he had tried all the remedies suggested by Dr. Morton, including CO_2. The ulcer healed for a time, but other nodules came up. The situation was the limit of the growth of the beard in the neck. At present he was trying collargol and giving X-rays afterwards. He thought it was healing up better than before.

Dr. G. B. BATTEN asked how soon after the first freezing Dr. Morton usually froze again, and for how long a time the CO_2 was applied. He had been recently shown a very simple method of making the stick of CO_2 snow. He took a cylinder of any size from that of a thermometer case to a ruler, put blotting paper round the cylinder of a length of about 6 in. to a foot, pulled the blotting paper tube off, pinched one end, held it against the bottle, turned on the CO_2, unrolled the blotting paper, and dropped the snow into a funnel of the size desired, and rammed it down into the spout.

Dr. HARRISON ORTON said he considered three years too short a time in which to decide whether there would be a recurrence of rodent ulcer. Some cases which he treated eight or nine years had only recently begun to show recurrence. He remembered one man who had a large number of rodent ulcers

over his face, and after treatment they disappeared; it was only in the last year or two that this patient had come back. X-rays seemed to have no effect on recurrences, and the question arose whether there would not be the same difficulty in the matter of recurrences when the treatment had been by means of CO_2 snow. When recurrences had been removed by the surgeon, they often recurred in the scar, so that all treatment seemed rather hopeless in certain cases. He thought there was often a difficulty in being quite sure of the diagnosis of rodent ulcer. There were two distinct classes of case, in one of which after two or three doses of X-rays the ulcer disappeared. Those he regarded as true rodent ulcers. The other class when treated with the same doses did not disappear, even though the treatment was continued for a long time. He regarded these as epitheliomata, and X-ray treatment of epithelioma he considered very unsatisfactory.

Dr. REGINALD MORTON, in reply, agreed, as he had stated in his paper, that the time was too short to enable one to say anything about the permanence of the method; but it was so easy to apply, so efficient and rapid in its action, and the results had been so good, that he thought, having published a preliminary note on the subject in the *Lancet* two years ago, that it was now desirable to report further progress. He hoped others would take up the same line of work in at least some of their cases, so that in a few years it would possible to judge of the measure of permanence it gave. It had been mentioned that recurrences had taken place after as long as six years. But if one had cured a rodent ulcer, and another rodent ulcer came in approximately the same place after six years, it was at least a debatable point whether the second could properly be called a recurrence. Why should not a person develop a quite independent second ulcer? Mr. Thurstan Holland's method of using both X-rays and CO_2 did not give the rodent ulcer much chance; it was a very thorough treatment, but unfortunately one could not be sure whether the result could be attributed to the X-rays or to the CO_2. But Mr. Holland knew, as others did, that X-rays would cure it, and he (Dr. Morton) knew from his experience that CO_2 would do the same thing. In order to get a clear idea, he had not combined the treatment with any other method. He thought Dr. Finzi was inclined to make too much of the pain following treatment by CO_2, for his patients had not made any particular fuss about it. He reminded Dr. Finzi that in the typical case which he mentioned the ulcer was healed in fifteen days. There had been no recurrence and he did not expect to hear from the patient again; but Dr. Finzi would have asked to see the patient for another application in six weeks' time, and even then recurrence might occur. Of course, if the carbon dioxide was applied in the orbital area there would be swelling; he had seen the eye closed for eight or ten hours as the result. But that was not complained of by the patient, in view of the great benefit to the ulcer, especially as he had warned the patient of what would happen. The pain was not severe, it might more strictly be called discomfort. He had used the treatment for eczema on the back of his own hand with great success and the pain was really trifling. If there were no pain whatever the

carbon dioxide treatment would be ideal, but the ideal treatment had not yet been attained. He disagreed with Dr. Finzi's statement that the action of the CO_2 was very similar to that of the actual cautery. The similarity probably held up to a point, but with the actual cautery one burnt and destroyed the tissues at the point of application, whereas with CO_2 that was not so, and when thawing out took place there was an intense reaction in the tissues, which were very much alive—not burnt away. Carbon dioxide was not a caustic. Moreover, one would not apply the cautery without an anæsthetic but he had never employed an anæsthetic when using CO_2. He could see what difficulty Dr. Frank Fowler had had with his case, because it was difficult to produce the freezing at a sufficient depth owing to there being nothing solid to push against at that point. He thought that was one reason why that case had not done better. Many rodent ulcers occurred over the facial bones, where there was a solid structure to push against, and in the case of the nose he liked to get down to the periosteum, and make firm pressure for from forty to sixty seconds. In an ordinary simple case of superficial ulcer the average application was forty seconds, the range of time being anything between twenty seconds and two minutes. With regard to recurrent cases, of which he had had several, if they were bad cases one could find out by means of the spoon that there was burrowing underneath, and he applied the treatment to those cases every week. It was only by carefully following those cases up that one obtained eventual healing of the sore. In the manufacture of the CO_2 stick, he used the simple method of rolling a towel over a ruler; he had never seen the necessity of a special apparatus, and if one was careful there need be no waste of the substance. His predecessor at the West London Hospital left him a legacy of one or two rodent ulcer cases, which he treated by X-rays for a long time; as they did not seem to improve he used the carbon dioxide, and then they immediately commenced to heal. Occasionally one came across cases which were not rodent ulcer, but carcinoma; and it seemed safe to consider that where they did not react readily the condition was one of carcinoma, not rodent ulcer. On the other side of the Atlantic, more of these cases seemed to be epitheliomatous —he did not know why—whereas in England there were more cases of rodent ulcer.

The Clinical Use of the Active Deposit of Radium.

By C. R. C. Lyster and S. Russ.[1]

It is well known that radium spontaneously produces a gas, called radium emanation. This gas, although chemically inert, has radio-active properties, and is gradually transformed into a solid substance known as radium A. In the act of transformation an alpha particle is ejected from the emanation atom, which is now an atom of RaA. This substance in turn is radio-active, emits alpha particles, and is transformed into another solid substance RaB. The transformation of this substance into RaC is not accompanied by the emission of alpha rays, but by beta and gamma rays. Radium C emits alpha, beta and gamma

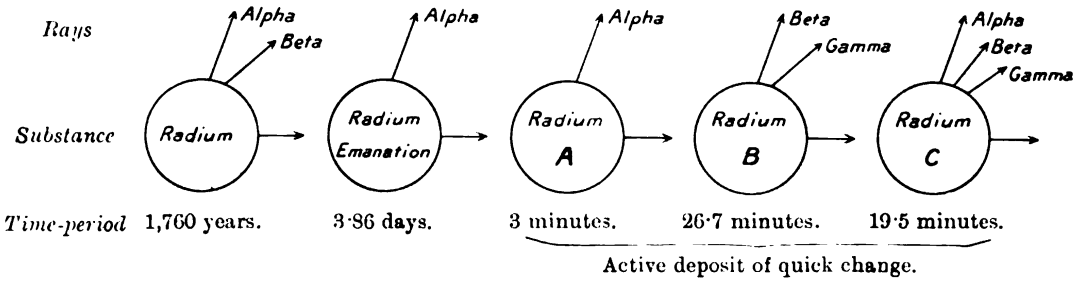


Fig. 1.

rays, and is converted into another substance, RaD, which is radio-active to such a slight extent that the consideration of the radium series may be left at this stage for our present purposes. Radium C has recently been shown to be complex, consisting of two substances, RaC_1 and RaC_2, so that in speaking of RaC its dual nature must be understood.

The complex nature of the changes occurring in the sequence of events narrated is conveniently illustrated by fig. 1, which is a representation of what is believed to occur to the atoms of the radio-active bodies in question.

The time-period is the time taken for the activity of a radio-active body to be reduced to one-half of its initial value. Thus 1 grm. of radium would by its continuous conversion into the emanation be reduced to 0·5 grm. in 1,760 years; 1 c.mm. of pure emanation would be reduced to 0·5 c.mm. in 3·86 days, owing to its transformations into RaA,

[1] Beit Memorial Research Fellow.

and so on. The three substances, RaA, RaB and RaC, have short time-periods, and for this reason they are collectively known as the active deposit of quick change in distinction to the bodies RaD, RaE and RaF, which have longer periods. When a sample of radium is sealed up, the active deposit is also present in a definite ratio. If the emanation is let into a vessel, and after being kept there some time, is pumped out, the walls of the vessel are found to be radio-active; this is owing to the active deposit which has been formed from the emanation.

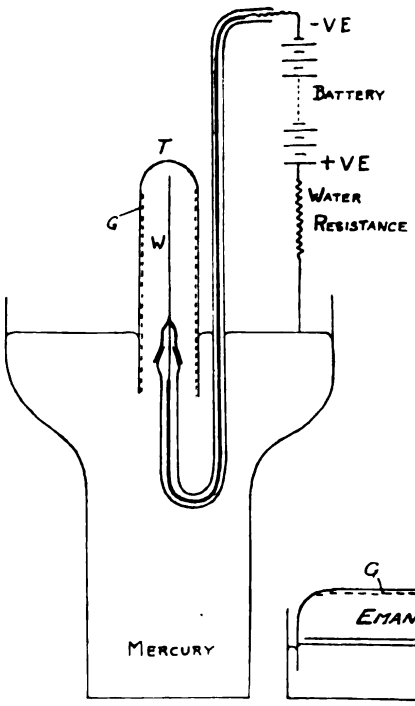


FIG. 2.

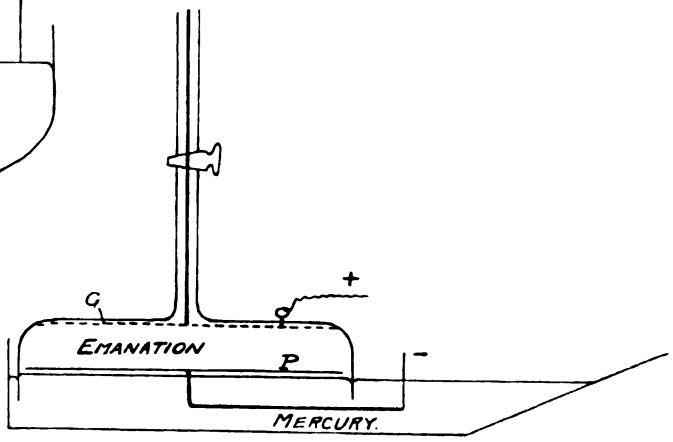


FIG. 3.

Under most clinical conditions of irradiation the alpha rays play no part (this is not the case, however, when fluids containing emanation in solution are injected), and the effects attendant upon irradiation are therefore due to the beta and gamma rays and the secondary rays which they produce in their passage through the tissues. Inspection of fig. 1 shows that the beta and gamma rays from radium are almost entirely confined to a part of the active deposit (RaB and RaC); hence, by abstracting this active deposit from a sample of radium, its beta and gamma ray activity may be temporarily almost entirely withdrawn.

It was first shown by Rutherford[1] that if the emanation be let into a vessel, the active deposit, as it is formed, may be concentrated on a negative electrode and removed from the gas. The negative electrode may take any form which the clinical conditions indicate desirable. For some time we have made use of the active deposit, upon wires for application to superficial cases, upon surgical needles for the introduction of radio-active material into growths, and upon large metal surfaces where the area to be irradiated has been extensive.

A method by which the active deposit may be concentrated from the emanation is shown in fig. 2. The case is that of a wire or needle upon which the material is to be deposited. The emanation is let into a small glass tube T, which is held over mercury. This tube is lined by a piece of iron gauze which dips into the mercury and through it is connected to the positive pole of a battery giving several hundreds of volts. The wire W to be made radio-active is mounted on a piece of capillary tubing so as to lie along the axis of the tube T. By letting the wire dip into a thread of mercury in the capillary tubing, connexion may be made to the negative pole of the battery. A water resistance is introduced to prevent a possible short circuit of the cells if the wire touches the gauze. It is thus seen that an electric field exists between the wire W and the gauze G, hence the active deposit is directed to the wire as it is formed from the emanation. A small percentage (about 5 per cent.) goes to the gauze, but with a field of about 500 volts per centimetre nearly all of the active deposit may be obtained on the wire when dealing with moderate quantities of emanation.

Similar considerations hold for the case in which the active deposit is obtained upon the metal plate P, the method of exposure of which is shown in fig. 3. After about three hours' exposure of the wire to the emanation the maximum quantity of active deposit is obtained upon it. Upon removal from the emanation RaA, RaB and RaC are in radio-active equilibrium, and alpha, beta and gamma rays are emitted by the wire. The activity of these wires is of course not permanent, but decays somewhat rapidly, the rate of decay depending upon the type of rays by which the activity is measured. This is so only because the three substances do not emit identical radiations.

The diminution with time of the gamma ray activity is illustrated by fig. 4, and it will be seen that after two hours the initial activity of the wire has been reduced to about 10 per cent., so that in practice irradiation of the lesion in a patient is not usually continued beyond this time. If

[1] Rutherford, *Philosophical Magazine*, 1900, 5th ser., xlix, pp. 161-92.

it is desired to express the dose in terms of so much radium, the radiation from which is constant, this may be done by evaluating the area beneath the curve. To take a concrete example: Say that we start with an active wire equal in its gamma ray activity to 10 mgrm. of radium, then after one hour the activity of the wire is reduced to nearly 4·5 mgrm. and after two hours to about 1 mgrm. From the nature of the decay curve it is found that the total radiation for the first hour is equivalent to that from a constant supply of 7·5 mgrm., and for the two hours to a constant supply of 4·9 mgrm. This may not be the exact *therapeutic* equivalent owing to the varying intensity of the active deposit.

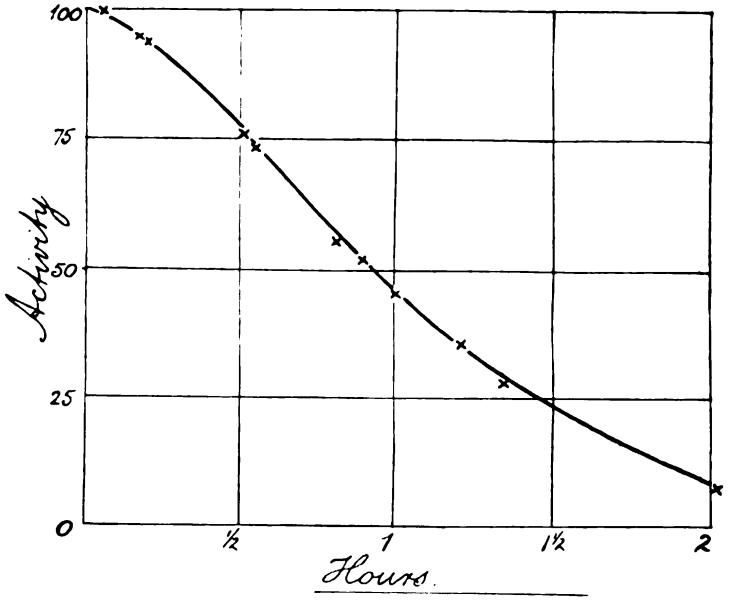


FIG. 4.

The advantages of this method of using radium are :—

(1) Convenience. Active wires of any shape or size may be prepared and applied with greater precision and convenience than is possible with capsules of radium.

(2) The option of using alpha and beta as well as gamma rays. For superficial conditions they are probably more effective than the latter.

(3) Economy. Several wires or needles may be exposed to the emanation at once.

The bactericidal effects of the alpha and beta rays[1] should ensure the

[1] Chambers and Russ, *Proceedings* (Path. Sect.), pp. 198-212.

sterility of the surfaces over which the radio-active material is deposited. The active deposit on these surfaces is not easily removed. An active needle after having been passed through an inch of muscular tissue was found to have lost about 30 per cent. of its gamma ray activity.

The number of cases in which these methods have been put into practice by us is so far quite small, being confined to four cases of rodent ulcer, in which active wires having a gamma ray activity equivalent to a few milligrammes of radium were successfully used, and to two deep-seated conditions in which surgical needles covered with active deposit were introduced.

The main purpose of this communication is not to record the results of any unique clinical manifestations as a consequence of radium treatment, but rather to indicate that the introduction of radio-active material into any desired locus may be considerably simplified by the methods indicated, and that the utility of preparations of radium from which the emanation can be obtained may be considerably extended.

DISCUSSION.

Mr. C. R. C. LYSTER said Dr. Russ had asked him to say a few words on the clinical aspects of the work just outlined. There seemed to be in it several points of value which were worthy of consideration, and upon which one would welcome further investigation. He had been much impressed with the difficulty of getting effects in the deeper parts of rodent ulcers, and it seemed likely that Dr. Russ's method of using the active deposits on needles would get over that difficulty to some extent. In a few cases such needles had been introduced into the deeper parts of malignant growths. The needles could be screened by covering them with a platinum or silver cannula, and so the whole of the alpha rays and the softer beta rays could be cut off, leaving for use only the pure gamma rays and harder beta rays. He thought most people had neglected the use of the alpha rays; if the alpha radiations could be got sterile into the deeper parts of a malignant growth the method had great promise. He hoped to see further investigation on the subject. At present he had used the method in four or five cases of quite superficial ulcerations. The platinum wires were very convenient, as they could be bent into any desired shape; they were very light, and no trouble for the patient to wear. Moreover, the dosage was quite accurate, and the needle could be replaced if a longer application was needed. There was room for further investigation on the question of radium deposits; the important point was the need for more radium.

Dr. G. B. BATTEN asked what was the most convenient way of making the emanation to begin with.

Dr. NORMAN ALDRIDGE asked whether large quantities of the radium were required, whether the making of radium emanation used up larger quantities

of radium, and whether those who had not command of large quantities of the substance could yet make effective use of it in the way described.

Dr. RUSS replied that the best way was to get the radium in solution, because the radium emanation did not come off well when the substance was in the powdered form. In the solution, however, it came off freely. It was kept in a bottle with a long neck, and the bottle was half filled with dilute hydrochloric acid. The whole thing was connected with an exhaust pump, and the pressure in that was reduced. As the emanation grew from the radium, it would bubble up into the vessel. In reality one should wait a month for its equilibrium value, but in the laboratory it was the custom to pump off every week, and one simply transferred the gas by the pump into any receptacle. The precaution was taken to spark it down, because the radiations decomposed water into hydrogen and oxygen, and that increased the bulk, whereas it was wanted in concentrated form. The sparking down was done with a small induction coil, which re-combined the hydrogen and oxygen, and the volume was got to less than 1 c.c., the little tube shown being of 1½ c.c. capacity. The quantities were comparatively small. Generally speaking, a few milligrammes were obtained on the wire—i.e., a quantity corresponding to a few milligrammes of radium. The measurements were made on gamma ray electroscopes, and since one had taken out from the emanation the substance which gave the gamma rays, the comparison was a direct one. There should be no difficulty in getting a sufficient quantity if one had 20 mgrm. of radium. By waiting a week one got 75 per cent. of the emanation possible, and one reckoned to get half at the lowest limit, so that with comparatively small quantities of radium one got enough on these wires for ordinary purposes. The radium was not appreciably affected in quantity. The period for radium was 1,760 years, so that for practical purposes it could be regarded as suffering no diminution.

Demonstration of a New Compressor.

By REGINALD MORTON, M.D.

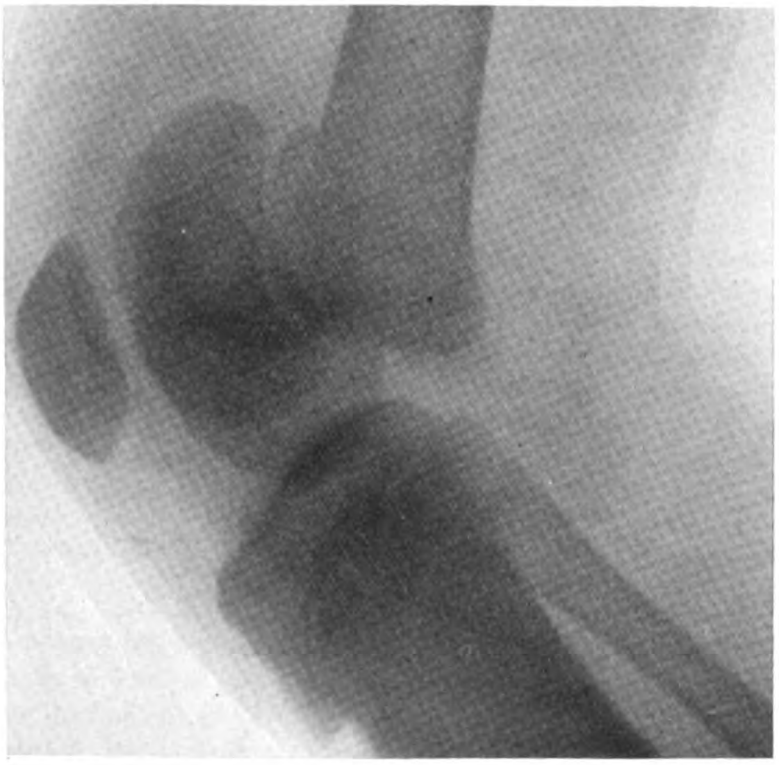
THIS diaphragm compressor differs in no essential particular from that of Albers-Schönberg, being no more than a modification of the latter. In this the lever action is done away with, but all the other movements are amplified and more easy to actuate. With the patient lying on the middle of the table it is possible to radiograph any part of the body without moving the latter, the cross-movement being particularly easy and convenient. In hospital work it has proved most valuable in saving time and temper. A further improvement lies in the substitution of an X-ray-proof box for the tube in place of the tray supplied in the original instrument, which gave no protection to the operator.

Skiagram of a Case of Separation of the Lower Epiphysis of the Femur.

Shown by G. Harrison Orton, M.D.

(For P. Clennell Fenwick, F.R.C.S.Ed.)

The skiagram showing the unusual accident of separation of the whole of the lower epiphysis of the femur was sent from New Zealand



Separation of the lower epiphysis of the femur in a child, aged 11 years.

by Dr. Fenwick. The patient was a little girl who was playing in a "three-legged" race on Empire Day. On the next day the knee was swollen and very painful; on examination under an anæsthetic the condition was diagnosed and verified by the skiagram.



